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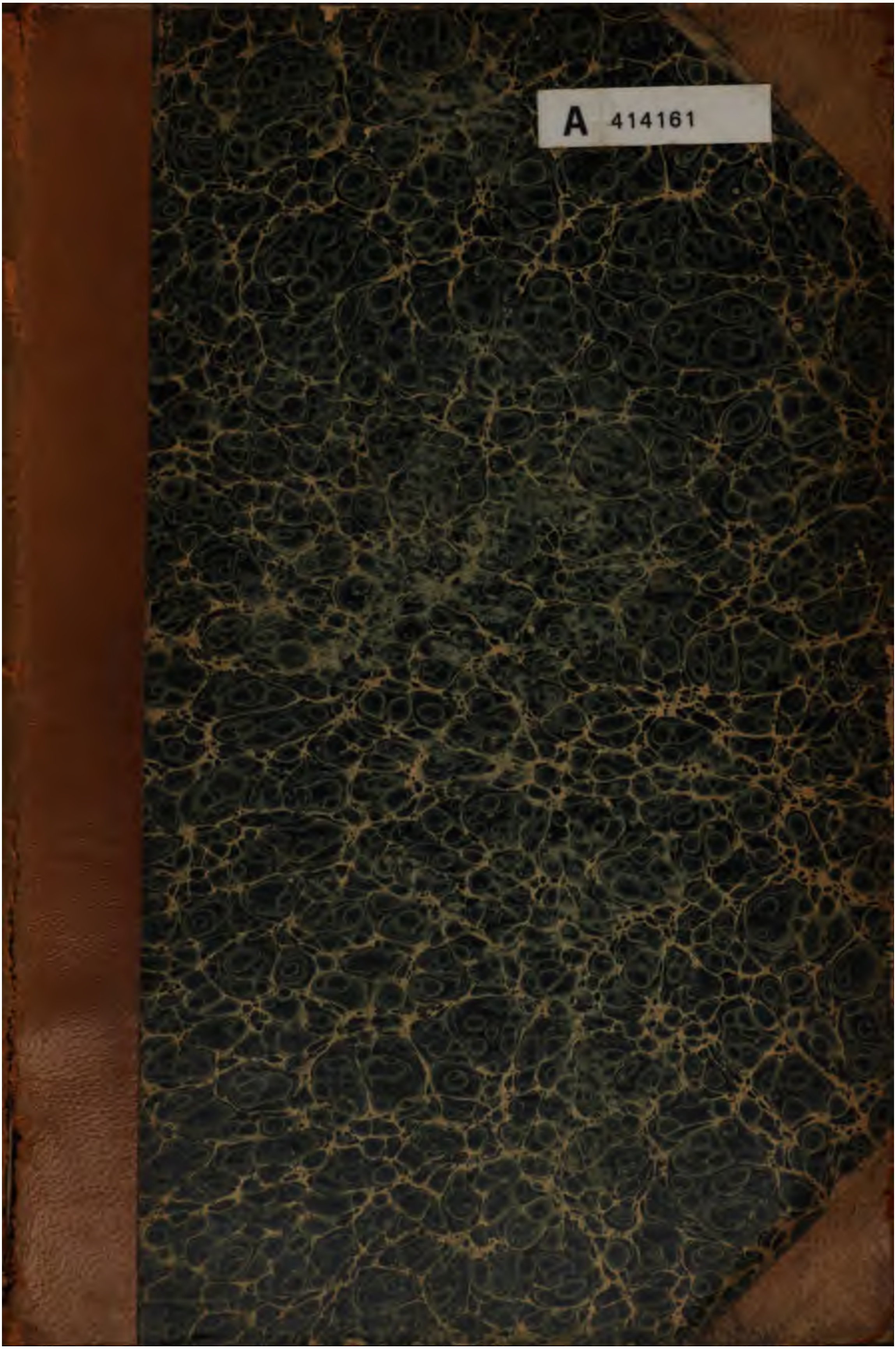

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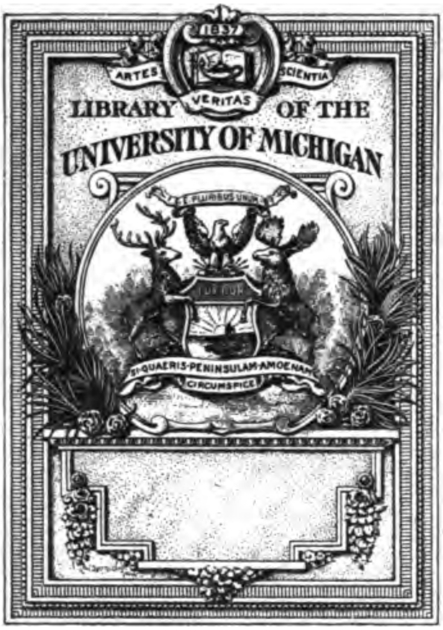

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THE

RETROSPECT

OF

U. S. M.

PRACTICAL MEDICINE AND SURGERY,

BEING

A Half-Yearly Journal,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE,

LECTURER ON OBSTETRIC MEDICINE AT THE LEEDS SCHOOL OF MEDICINE, ETC.

PART XL.........JANUARY.........1860.

UNIFORM AMERICAN EDITION.

NEW YORK:
W. A. TOWNSEND AND COMPANY.
1860.

C. A. ALVORD, PRINTER, 15 VANDEWATER STREET, NEW YORK.

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PRACTICAL MEDICINE.

&c., &c.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ART, I.—ON FEVER—THE DUALITY OF ITS SOURCE.

By Dr. WM. ADDISON, F.R.S, &c.

[Dr. Addison starts with the assumption that "fever is the expression of disorder in the corpuscles of the blood;" then, as these corpuscles derive the materials of their growth and nourishment from two sources, viz., the atmosphere and the plasma, the argument is continued that they may be disordered by injurious matter derived from these two sources. Thus we have two forms of fever, designated respectively contagious and hectic. Yet in common with other cellular bodies, the corpuscles of the blood possess considerable power of resistance to disease. It is not every passing impurity of the atmosphere, nor every injurious change of quality of the plasma, that establishes symptoms of fever. Hectic fever is produced by the effects of a local disease (as pulmonary suppuration, or suppuration owing to necrosis of bone,) on the plasma of the blood. To show that a local disease may and does produce changes in the blood plasma, an experiment is detailed in which agglutination of red corpuscles and formation of colourless matter, could be seen in the capillaries of a frog's foot, as the result of irritation—these altered blood elements passing into the circulation along the dilated veins. The author then continues:]

No one can doubt that the fluid of the blood is altered, and may be distempered, by unwholesomeness of diet, and by neglect of the daily excretions by the skin, bowels, and kidneys. It is also evident that these common sources of distemperature of the fluid of the blood must operate not only in persons in health, but also in persons who may be afflicted with chronic forms of inflammation, such as are present in necrosis of bone, in diseased joints, pulmonary consumption, &c. And if, in these last mentioned examples, distemperature of the fluid of the blood from errors in diet, or other such causes, concur with distemperature from absorption of spoiled matter from places of chronic suppuration, then there will be *deuteropathy of the plasma*, or disturbance of the qualities of the fluid of the blood from two points at the same time; namely, unwholesomeness of food and absorption of morbid matter. And it follows from the physiological relation subsisting between the corpuscles and the fluid of the blood, that an increasing debasement of the qualities of the fluid *must* at length disorder the corpuscles.

But one of the chief points we have been arguing for, is the therapeutical relations of inflammation to the fluid of the blood. Suppuration is a means whereby injurious matter is eliminated from the plasma; and granulations and pus may perform the office of a depurating organ vicariously. Now we are saying that chronic suppuration and ulceration will occasion deuteropathy of the plasma, and thereby fever. This seems an incongruity. A little consideration, however, will show that it is only seeming incongruity. Diet sustains life and health only by measure: it is pathological in excess and by deficiency. Heat or temperature contributes to life and health only by measure. Oxygen, an essential constituent of the atmosphere, is an element of health and life only by measure; any great variation from a mean amount is pathogenetic. Too much or too little would equally occasion disturbance of health.

So likewise of the matters we are discussing; the process of repair in the commonest injuries has its pathological as well as its therapeutical aspects.

The reaction upon which cure depends may be too much, or too little, or too long about. Granulations may be languid, or indolent, or deficient; or they may luxuriate, and usurp the place of fibrous tissue when fibrous tissue is needed for reparation. And fibrous tissue may hold its ground when osseous tissue is demanded for cure. This is sometimes the case in fractured bones. In ordinary contusions, great swellings appear and disappear. In their appearance, matter from the plasma of the blood must have become stationary in the part. In their disappearance, this matter must have been absorbed again into the blood. There must be, therefore, in these cases, in some way or other, a ready passage for elements from the injured tissue into the fluid of the blood.

Analogously, inflammation as a depurative reaction in distemperatures of the fluid of the blood, may be hindered and interfered with in various ways. There may be too much or too little of it; and certainly it is very often protracted by the persistence of the blood-disturbing causes. If, then, there be a ready passage—to and fro, as it were—between the fluid of the blood and the common tissue, it is not difficult to perceive that interference and hindrances may interrupt, or even reverse, the action in this particular.

The ordinary process of repair, then, has a double aspect; and so, also, has inflammation. And our argument is, that protraction or chronicity in either of them introduces the liability to absorption of spoiled material, and that thus therapeutical reactions may operate retroversely and pathologically upon both parts of the blood; the fluid first, and then the corpuscles.

But, that we may give an outline of the argument as it relates to hectic fever, we take as examples necrosis of bone, gout, and scurvy; and, in contrast with these, scarlet fever.

Necrosis of bone produces inflammation. There are hindrances to the removal of the dead bone; therefore inflammation passes into protracted suppuration and ulceration. These gradually weaken the patient; they disable him from taking exercise; digestion is impaired; and the functions of the depurating organs are disturbed.

This is one source of distemperature of the plasma. Distemperature of the plasma aggravates the existing inflammation; but the antecedent—the dead bone—cannot, in the case we are contemplating, be removed. Therefore disorder must proceed, until at length, from the places of suppuration, morbid matter ebbs back into the circulation; and the plasma, thereby thoroughly disordered, reacts upon and disorders the corpuscles, and hectic fever, more or less, appears. Upon this interpretation of the sequence of events between dead bone and fever, to cure the fever the blood corpuscles must be relieved from their disorder; to relieve them the qualities of the plasma must be improved; to amend the qualities of the plasma, the chronic suppuration must cease; and that chronic suppuration may cease, the dead bone must be taken away. We all know that the effectual removal of the dead bone will cure the fever.

Errors in diet by excess produce distemperature of the plasma. And if the depurating organs, or some of them, fail in removing the distemperature, inflammation arises. In gout, the patient is surrounded with every comfort. The error in diet is most probably one of excess; it can, therefore, be easily interdicted; the antecedent can be readily removed; and, by medicine, the depurating organs can be stimulated to a more active working. For these reasons, distemperatures of the plasma is concluded to be simple; its qualities are disordered from manageable sources, which may be attacked and abolished before disorder is communicated to the corpuscles. Inflammation in gout is, therefore, acute, and without fever.

On the other hand, in scurvy, the errors in diet are those of deficiency or unwholesomeness, and are much more difficult to deal with, especially where persons are crowded together in unhealthy localities, or limited to camps or ships. The individuals are poor, or, from other circumstances, cannot command the necessaries of life. Therefore, forms of inflammation, which in the rich are simple and acute, are here (or in the poor) chronic, and pass on to suppuration and ulceration, as in the sailors before mentioned, whose bare legs and feet were bitten by mosquitoes; upon which example we observe that because the unwholesome diet and confinement could not be changed, therefore the bitten parts passed into chronic ulcers. And if, in persons thus situated, with forms of chronic ulceration from continued unwholesomeness of diet, or other privations, morbid matter should be continually ebbing back into the circulation from places of chronic ulceration, the elements of fever, from a double debasement of the plasma, would exist; and fever thus arising would obviously be different from fever arising through miasma in the air.

In scarlet fever, it is concluded, that disorder of the blood begins, not with the plasma, but in the corpuscles. The illness commences, not with forms of inflammation, but with symptoms of fever. There has been no error in diet: a miasmatous air has acted on the blood; a specific poison is generated; and the plasma is distempered posteriorly to disorder of the corpuscles. But (here as in small-pox) no natural depurating organ seems adapted for the removal of the poison of scarlet fever from the plasma; therefore inflammation arises—that is to say, reactions between the plasma and the common tissue. The forms, amount, and duration of inflammation in scarlet fever, indicate the amount and severity of the disorder of the blood. Without these reactions, the patient would die from a poison shut up in the blood; with them, in their severest forms, there is a battling for life. When a joint has been crushed, death would take place from mortification, were there no reaction; but, this established, the patient is saved from the first and most pressing danger, though afterward he has to pass the ordeal of inflammation, abscess, suppuration, ulceration, and very probably hectic fever too, as best he can, or suffer amputation for a chance of life. In scarlet fever, to cure the inflammation, the plasma must be freed from poisonous matter; and no more must enter it. That no more may enter it, the corpuscles must cease to generate and excrete a poison. Now, from the course observed in normal cases of an exanthematous fever, we may probably conclude that the corpuscles pass through their disorder in from four to six or eight days. When their disorder has passed, no more poisonous matter is discharged from them into the plasma; and, no more poisonous matter mingling with the plasma, the inflammatory reactions and the natural depurating organs together succeed in restoring the plasma to its natural state; whereupon, the blood regaining its normal constitution, inflammation comes to an end, and the patient is cured. The pathological and therapeutical sequences are the same as in small-pox.

In the midst of these therapeutical actions and reactions for the depuration of the blood in fever, it would seem that a depurating organ is sometimes coerced. as it were, to an increased and incongruous working; matter not naturally found in the secretion of the organ appearing in it at the crisis of the fever. In the performance of this enforced duty—the elimination of poisonous matter from the plasma—the parenchymatous elements of the organ may be overtasked and injured. Thus, in scarlet fever, the poison in the blood sometimes occasions parenchymatous disease of the kidneys; and, in such cases, there is evidence also of inflammatory reactions in the common tissue of the organ. This complication may have the same reflex effect upon the blood as chronic ulcerations. Spoiled material from the overburthened kidneys may ebb back again into the circulation; and a new blood-distemper may be inaugurated from elements of urine retained in the plasma. Such being the case, there would be present the antecedent of a second or reactionary fever; namely, deuteropathy of the plasma—that is to say, distemperature—from disease of the kidneys, superposed upon the remnant of the poison of scarlet fever. And it is in perfect accordance with the argument, that a secondary fever from disease of the kidneys should be more apt to appear as a consequent of the primary fever, where the inflammatory reactions in the skin are too slight or insufficient for the full and effectual discharge of the poison. But it is to be observed, that the second fever is not a relapse or reappearance of the first; it is another fever, of different origin. The first fever was occasioned by an aërial miasm; the second is occasioned by a debasement of the plasma acting injuriously on the corpuscles of the blood.

Let us give a brief summary of the facts and of the arguments.

In necrosis of bone, the pathological series begins with dead bone. If this cannot be taken away, it ends with fever, from deuteropathy of the plasma disordering the corpuscles of the blood.

In pulmonary consumption, the pathological series begins with tubercles in the lung. There are hindrances and difficulties in their discharge: suppuration is made chronic; and the phenomena end with fever from deuteropathy of the plasma.

In scurvy, the series begins with unwholesomeness or deficiency in diet, or other privations which cannot be changed. Ulcerations arise; and the series may end with fever, from deuteropathy of the plasma.

In these examples—namely, hectic fevers—disorder of the blood-corpuscles is posterior to a debasement of the fluid in which they swim: and forms of inflammation, protracted for longer or shorter periods, precede the fever.

On the other hand, in the contagious primary fevers, the pathological series begins with disorder of the corpuscles. It ends with forms of inflammation; because distemperature of the fluid of the blood is, in these fevers, posterior to disorder of the corpuscles. Thus we interpret the relations of fever to inflammation, and of inflammation to fever, by the difference between the two parts of the blood. The facts are, that sometimes fever precedes inflammation, sometimes forms of inflammation precede fever; because sometimes (from aërial poisons) the corpuscles are disordered before the plasma; and sometimes (from unwholesome diet, privations, and chronic ulcerations) the plasma is disordered before the corpuscles. If you accept these interpretations, the whole subject of repair, inflammation, and fever, presents a coherency which is worthy of your attention. Thus:

Mechanical objects injure the common tissue; and the process of repair arises.

Errors in diet disorder the plasma; and inflammation appears.

Miasms in the air affect the corpuscles of blood; and primary fever is the result.

Both the process of repair and inflammation, from hindrances and difficulties, may pass into chronic or protracted forms of suppuration, ulceration, and discharges; whereupon, if spoiled material should enter the circulation, and, by reiteration or quantity, thoroughly debase the plasma, the corpuscles suffer, and fever appears; namely, reactionary, hectic, or a plasma fever.—*British Medical Journal, May* 28, 1859, *p.* 424.

2.—ON DIPHTHERIA.

By George Bottomley, Esq., Croydon.

[The opinion of the profession differs widely on this disease, some practitioners considering it a new disease, others holding that the materies morbi is identical with that of scarlatina.]

It appears to me that at the commencement of the attack there is but a slight congestion of the mucous membrane of the pharynx, accompanied with slight constitutional disturbance; but, in a few hours, the membrane puts on a livid appearance, and runs rapidly into the gangrenous state: and that the false membrane is a deposit of layers of lymph in the early stage of the disease, which soon loses its vitality, and acts as an extraneous body, thereby preventing the parts from performing their natural functions. Accompanying this change, great depression of the vital powers of the system takes place.

Now whence arises the rapid and fatal change in a few hours? Is it not from meteorological causes? for it is certain that locality has but little to do with it. It has been as severe in high and dry situations as in low and damp; in isolated dwellings, and in crowded cities; in clean and well drained places, and the reverse. Therefore, ought we not to look to the atmosphere as producing those rapid changes, acting upon the blood, occasioning the gangrenous condition of the throat, accompanied with an extremely low and depressing type of fever, from which the patient rapidly sinks into a state of extreme and fatal exhaustion? It therefore appears consistent with pathology to administer a most powerful antiseptic remedy, to act promptly both locally and constitutionally. After taking the above view of the disease named by Bretonneau *diphtherite*, I beg to submit to my medical brethren the plan of treatment which, I am happy to say, I have found very successful.

I had under my care several severe cases in the autumn of 1857, and again in the autumn of 1858. Those in 1857 occurred in the town and neighbourhood of Croydon, and those in 1858 in a new building, an asylum for the reception of children whose parents died young: consequently, their offspring were not constitutionally the best fitted to resist so formidable a disease.

The treatment I adopted in all the cases under my care was as follows—for *children*:—

℞. Solutionis chlorinii ℥ ss; syrupi simplici ℥ ss; aquæ destillatæ ad ℥ vj. M. Fiat gargarisma sæpe utendum.

℞. Solutionis chlorinii gtt. iv; syrupi aurantii ʒ j; aquæ destillatæ ad ℥ ss. M. Fiat haustus 2ndâ quâque horâ sumendus.

The dose was increased according to age. Calomel was given in doses of one grain and upward, according to age. The diet, too, consisted of concentrated jellies, strong beef-tea, wine, etc.

The same mode of treatment was adopted in adult cases; except that, instead of calomel, I gave the hydrargyrum cum cretâ.

For many years I have ordered the chlorine solution in malignant scarlet fever accompanied with a diphtheritic state of the throat, with marked success. In the *Cyclopædia of Practical Medicine*, published in the year 1835, Dr. Tweedie states that, in an epidemic sore-throat which made its appearance in Tours in 1818, and to which Bretonneau gave the name of *diphtherite*, hydrochloric acid was found most efficacious.

The orphan asylum to which I have before alluded is a new building erected on the top of a hill, open to the south-west; the rooms spacious and lofty; every attention paid to ventilation. It stands alone, in a most healthy situation; and the children, before admission, undergo a medical examination; so that, at the time of admission, they are perfectly healthy. So that does away with the supposition of its being confined to ill-drained, low, and swampy situations, and densely populated, poor localities. In the months of July and August, 1858, there were fifty cases in the above asylum, of the most malignant form. They were all placed under the treatment before named. After the third day, quinine was added to the chlorine solution. Many were covered with the scarlet eruption; eighteen had a severe form of measles; and some few were free from eruption; but all had the diphtheritic throat, and but one case out of the fifty proved fatal—a child four years old. The disease made its way through the various tissues of the throat and neck, and broke externally, giving vent to a most offensive discharge, the patient sinking from exhaustion, not being able to take sufficient stimulant and nourishment.—*Brit. Med. Journal, July* 16, 1859, *p.* 561.

3.—ON THE TREATMENT OF DIPHTHERIA.

By Dr. J. S. Bristowe, Southwark.

It may, I believe, be safely affirmed that we are unacquainted with any specific capable of arresting the course of diphtheria; and, therefore, that our treatment, unless intended to be experimental, must, in the present state of our knowledge, be simply directed to the piloting of the patient in his progress through the disease, to the guarding him from any accidental perils, and in the event of such befalling him, to the repairing of their ill effects. The main danger in the uncomplicated affection evidently arises from debility, and hence it has been properly insisted on by nearly all practitioners that stimulants are desirable. And, further, since that debility often manifests itself fatally only at a late period of the disorder, it is obvious that the use of stimulants alone, the action of which can merely be temporary, is not sufficient, and can only be regarded as subordinate to the exhibition of nourishment, the effects of which, if slower, are yet solid and permanent. It is highly probable, I think, that at an early period of the disease antiphlogistics, administered with discrimination, may be useful; but if so the period during which they alone should be employed speedily passes away; and on the whole it is manifestly safer to ignore that period, and to adopt from the beginning the stimulant and nutritive line of treatment, than to persist one moment longer than needful in the opposite course. Under these circumstances it seems to me most important that stimulants, combined with nourishment, should be commenced with early, and should be systematically persisted in. The amount of both, of course, must depend upon circumstances; but in order to insure a sufficiency they should be judiciously varied, administered in small doses, but at regular and frequent intervals, and if rejected by the stomach, then given in the form of enemata.

An important question is that having reference to the mode of treatment of the affection of the throat; and I may here at once state, as may have been inferred from the perusal of my cases, that I, for one, disapprove of the application to the diseased surface of strong caustics and escharotics, and should prefer the employment in all cases of mild detergent gargles, or of warm milk, and such-like bland and soothing fluids. The reasons which have led me to discard heroic applications are the following:—1st. That the throat affection is merely a local evidence of a constitutional disease, which is unlikely to be arrested in its progress by any treatment directed to its secondary manifestations only. 2nd. That the throat affection rarely kills, except by involving organs such as the trachea, and deeper tissues of the neck, which are beyond the region of the possible influence of such agents. 3rd. That even if the theoretical correctness of such treatment be admitted, the application of remedies to the surface of a thick false membrane, with the hope that they may affect the subjacent mucous tissue, is not only clumsy, but as regards the object intended, practically useless; and that the prior forcible removal of the membrane from the entire surface, in order to their efficient employment, is unjustifiable in the early stage, even if possible, and is likely only to be followed by increased inflammation, and reproduction of false membrane—by more real mischief, in fact, than the benefits presumably to be derived can possibly counterbalance. 4th. That the application of such agents around the diseased tract, for the sake of limiting, should such be the intention, the spread of the disease, must, even if its efficiency be allowed, be an exceedingly difficult, and generally impossible operation; and from the tendency which the membrane has to spread from more than one centre, would probably, even if in the first instance thoroughly performed, become ultimately useless. Of course if a gangrenous state of the tonsils, or any other local complication supervenes, such topical applications as are commonly had recourse to in like conditions of the throat, should be employed; and in all cases, even from the beginning, such treatment should be directed to the exterior, as we usually resort to for the sake of allaying inflammation or relieving pain.

As I have purposely refrained from going into details of treatment, even in regard to those two points which alone I have made the subject of remark hitherto, I have little to add in order to carry out the intentions with which I started. There are still, however, a few considerations to which I may refer. I am not disposed to place much reliance on medical treatment during the earlier period of the disease; and, guided by general principles, should use such adjuncts only to the plan of practice sketched above as the condition or tendency of the case under observation might indicate; yet, at the same time, should carefully watch my opportunity for commencing the employment of tonics. Opium will sometimes be needful, and so also purgatives; but I should prefer the administration of the latter in the form of enemata. Lastly, any complications, renal, pulmonary, laryngeal, or whatever other they may be, need not, I imagine, any peculiar mode of treatment, but should be dealt with according to the principles which guide us commonly in our treatment of like affections arising under different circumstances; or, to be more explicit, we should endeavor to cure or obviate by ordinary means any secondary lesion which may arise; yet regulate our special efforts according to the general condition of the patient at the period of invasion of the superadded disease.—*Med. Times and Gaz.*, *Sept.* 3, 1859, *p.* 228.

4.—*Treatment of Diphtheritic Sore Throat.* By C. SWABY SMITH, Esq., Burbage, Wiltshire.—During the last three months I have had about forty cases of diphtheritic or malignant sore-throat under my care, and I have especially noticed that one and all of these cases have been in houses situated either near a pond or pool, or at the foot of a hill, and frequently where there are many trees about the house; not one having occurred in houses situated on high ground. On inquiring into the history of these cases, I have had but one answer—namely, that the disease commenced by a sore-throat, which the patient thought was only a cold; and consequently when I have seen them they have been in such a high state of inflammation and ulceration, that the patient has told me that he has then applied because he could not take food, either from the pain occasioned by swallowing, or else that it was no use trying, as it only returned by the nose; and very often the voice has been almost inaudible. I have tried many modes of treatment, and so far with very good results; but the one that I have most faith in is one that I would advise those who have not used it at any rate just to give it a trial. On first seeing my patient, I apply the strong solution of chlorinated soda to the fauces, and then follow up my treatment by ordering a sinapism to the throat; a gargle, composed of solution of chlorinated soda, two ounces; tincture of myrrh, two drachms; water, six ounces: to be used every half hour; and in cases where the children are too young to gargle, I order the throat to be frequently washed with the same mixture by means of a piece of sponge. Internally I give to an adult (of course varying the dose according to my patient's age): chlorate of potash, two drachms; dilute nitric acid, three drachms; solution of cinchona (Battley's), one drachm; water, to six ounces; the sixth part to be taken every two hours. And in cases where there is much pain in the limbs, I generally add a few minims of tincture of colchicum, which addition has proved decidedly advantageous; the diet to consist of strong beef-tea, port wine, and, in short, all the nourishment the patient can take. I also strongly urge the necessity of free ventilation.

Out of these forty cases, I have lost only two, and both were in a moribund state when I first saw them. Although these means are undoubtedly useful in decided cases of malignant sore-throat, they are far too active to be resorted to in simple cases, as they would only tend to aggravate the symptoms.—*Lancet*, *Sept.* 10, 1859, *p.* 264.

5.—ON DIPHTHERIA.

By JAMES P. M'DONALD, Esq., Bristol.

[The writer commences by stating that he has had under his care a large number of serious cases of diphtheria.]

I consider diphtheria to be a disease produced by a specific poison taken into the system, acting through the blood, and *seen* at the throat. The following are the usual form and course of the disease in its severest type. The patient is *suddenly* (and generally in the morning) seized with violent vomiting of a thin, yellowish-white matter, of a very offensive character; then purging of a fluid of similar appearance and smell. These dejections last an hour or so, and are followed by great prostration and stupor. The patient lies for a period varying from six to sixteen hours in a heavy sleep, from which he is with difficulty aroused, and then only to sleep again. The skin is hot; pulse 100; the tongue is of a bright red; drink is taken with avidity, if offered, but only to be immediately returned. And now the important question is put, "Is the throat sore?" The answer is *always* the same—

"*Not in the least.*" The reply, to a physician inexperienced in the horrible malady, may be fatal to the patient. The diagnosis is that this is not a case of diphtheria. On the other hand, the experienced man *expects* this reply; he forthwith carefully examines the throat, and then he *sees* the disease. In this early stage the tonsils, the soft palate, and the back of the pharynx present a bright shining red appearance. The small vessels are not seen individually injected, as in many forms of sore-throat, but the appearance is as though the parts had been brightly painted and then varnished. Hanging from the velum to the tongue is seen, in this stage, a transparent film of a tenacious fluid, which is burst by expiration, sending its particles over the mouth and the instrument used to depress the tongue. The next moment a similar curtain is formed. After a period varying from six to sixteen hours, the condition of the patient materially changes. The stupor has passed off, and delirium, often of a violent character, takes its place; there are the usual symptoms of cerebral excitement, and the fever runs high; breathing is quickened; the voice is changed to a thick yet shrill tone; there is a short, dry cough; (in children, evidences of coming croup); the neck is puffy and blushed; the tongue is coated with a white fur, and all those parts hitherto so brilliantly red are thickly spotted with a whitish substance, which, in a wonderfully short period, conglomerates, and forms one thick, plastic deposit, which in time may cover the whole palate to the teeth, so that the appearance, on opening the mouth, is as though it were lined with plaster-of-Paris. The violent delirium then subsides: the powers of life fail rapidly; the horrible sensations of choking and suffocation come on; the sufferer tears at his neck with his nails, and tries to open his mouth, yet full power of swallowing still continues, and he greedily gulps anything given him in the shape of drink; large livid spots form on the extremities, amounting sometimes to purpura; the diarrhœa of a white and offensive matter is incessant; muttering delirium comes on, and in a long tetanic convulsion death closes the scene.

This is a truthful picture, drawn from realities, of how a previously strong and healthy man may, *in six days or less*, cease to be.

Taking the above as a fair example of diphtheria in its most marked and deadly aspect, as I have seen it, we get the resemblance to it more or less in all minor cases. We must not expect to meet with all the symptoms in every case, but the condition of the throat is invariable. Whether that condition goes on to the second stage depends on the severity of the poison or the success of the treatment adopted. In all cases where there is either nausea or vomiting, followed by drowsiness, the throat ought to be examined, and if the redness and the "glassy curtain" appear, the immediate use of the proper appliances may, I am quite certain, save many valuable lives.

There has been considerable confusion with respect to scarlet fever and diphtheria. Some have contended for the identity of the two, maintaining that those cases in which no rash appeared were to be considered as "suppressed scarlet fever." To combat this view, it will be sufficient, I think, to draw attention to the great difference in the symptoms I have described from those of scarlatina, and to state the fact of its having been my painful experience to have attended families some members of which have been swept off by scarlet fever *with diphtheria*, whilst other members, who had previously suffered from scarlet fever in a severe form, were now attacked with true diphtheria. That scarlatina invites diphtheria is very manifest, but that the diseases are perfectly distinct and different is equally certain.

Now as to the treatment. The constant attention to the condition of the throat should be our first care, the second is to resist the "tendency to death." By skilful personal application of strong solution of caustic to the glazed and reddened parts, the fungoid matter may not appear, or if formed, may be separated from the surface and brought away, and thus the horrors of the disease prevented. Still, even then, there is much to be done in supporting the powers of the constitution, so as to give it assistance in eliminating the terrible poison from the system.

Bearing these two essentials of treatment constantly in mind, I know of no epidemic disease we may be more hopeful about than this. Terrible as it is to behold—its very name spreading dismay and dread to all around, yet its severity and fearful characteristics seem to succumb to the judicious and speedy treatment of the attentive physician with a kindness hardly to be expected.

Diphtheria is no respecter of person, age, condition, rank, or temperament. Should it become more seriously epidemic than it has been, no doubt it will mow down many of those unhappy people whose hard necessities oblige them to live in the overcrowded and ill-ventilated courts and alleys of our large cities. In such cases it will clearly be the mission of our profession, as in visitation of cholera, to go *to* the disease, and not to let it *come* to us.—*Lancet, Aug.* 20, 1859, *p.* 183.

6.—*Diphtheria Treated by Irrigations with a Solution of Common Salt.*—M. Roche mentions in *L' Union Médicale* of July 26th, that by this treatment he saved his patients in six cases of diphtheria. The false membranes were first freely cauterized with the lunar caustic, and injections then made every hour against the fauces with a solution of common salt, the strength of the solution being such as not to create nausea. Chlorate of potash was also given internally; and tincture of iodine, as a topical application, was used in half the cases: but M. Roche considers that the irrigations with the solution of common salt were the chief agents in the cure. One little girl was not cauterized at all. The author likewise holds that solutions of alum, chlorate of potash, iodide of potassium, chloride of lime, &c., would perhaps be as efficacious.—*Lancet, Aug.* 20, 1859, *p.* 189.

7.—TREATMENT OF DIPHTHERIA.

By J. C. S. Jennings, Esq., Malmesbury.

At the first outbreak of the disease, no cases of scarlatina had appeared in the neighbourhood; nor were there any until the second outbreak, during the month of January in this year, when a few cases of diphtheria occurred; but scarlatina maligna ran through several families. In those cases, however, in which the rash was well developed, and not suppressed, there was little or no throat affection; and *vice versa;* and when the tonsils *were* affected, there was not the peculiar leathery exudation of diphtheria.

The plan I have invariably adopted, regardless of sex or age, or incubation of disease, has been to give an active emetic of antimonial wine, from half an ounce to an ounce, according to age; to freely cauterize the throat with solid nitrate of silver; to have a mustard poultice applied from ear to ear; the feet and legs plunged in a hot bath; and the patient confined to bed. After the emetic action has ceased, from three to five grains of calomel with five of compound extract of colocynth were given (or, for a child, two grains of calomel with two grains of compound antimonial powder); and, four hours afterward, the following mixture:—

> ℞. Quinæ disulph. ʒss; potassæ chloratis ʒj; acidi hydrochlorici diluti ℥ss; aquæ ℥viij. M. Fiat mistura cujus sumatur pars sexta 4tis horis.

A gargle of chlorine solution was directed to be used frequently, prepared by impregnating water as much as can be borne with the protoxide of chlorine, generated from two parts of chlorate of potass, one of hydrochloric acid and one of water, and the fauces to be sponged out frequently with the same. The emetic I have rarely repeated more than once; but when the inflammatory stage has been severe, the fauces tense and shining, and the throat œdematous, spirit of nitrous ether and liquor of acetate of ammonia, or nitrate of potass, has been added to the mixture.

The diet has been at first farinaceous, and afterward consisting of strong broths and jellies. Stimulants have been very rarely administered, and then only as sherry whey, alternately with the quinine, which I have trusted to as the sheet-anchor. For infants, quinine may be given in jelly, washed down with a mixture of tincture of sesquichloride of iron.

Upon this plan all my cases have been successfully treated with the exception of one fatal case in a stout young man, where much valuable time had been allowed to elapse; the mushroom-like exudation having extended over the soft palate, completely blocking up the fauces and chink of the glottis; beside which, the emetic was not rightly administered, or failed to produce effect; neither was the purgative given as ordered; and the patient died from apnœa, suffocation being produced from spasm of the glottis when turning to lie down in bed.

Too much stress cannot be laid upon tartar emetic, quinine in large doses, and the avoidance or guarded use of alcoholic stimulants.—*Brit. Med. Journal, July* 16, 1859, *p.* 562.

8.—ON DIPHTHERIA.

By Thomas Heckstall Smith, Esq., St. Mary Cray, Kent.

There are three forms in which the disease presents itself, viz., simple ash-coloured diphtheria membrane in patches, with very slight congestion of the surrounding parts, and without fœtor. Secondly, a deeper colour, and more widely spread membranous exudation, with fœtid breath, and intense engorgement of dark hue. Thirdly, the membrane with much tonsillitis, in a few cases resulting in quinsy. But there has been a fourth and more formidable state of things to contend with, namely, an extension of the membrane in either of the above forms, to the larynx and trachea, the symptoms of which I need not describe. In the three cases of 1857, and in the earlier of this epidemic, there was no appearance of ulceration. If the membrane was peeled off, the part under presented a clean patch of smooth surface, distinguishable from the surrounding parts, but in no degree abraded. But lately, there has been more tonsillitis, and frequently superficial ulceration, in several cases in the last few weeks, excavating more deeply into the tonsil; the membrane being still distinctly marked. During this period, east winds have prevailed, and more feverish constitutional disturbances have been observed.

With reference to the more general symptoms, the prominent feature is depression of the vital powers. The pulse is not always quick, but when so, is that of an irritable circulation. More frequently the pulse is slow, waving, and very compressible; the skin readily cools down (as when a hand is exposed out of bed), is moist and soft, almost clammy to the feel. Where fever has been observed, it is of a remittent or intermittent type.

Some cases have been ushered in with slight diarrhœa, with discharge of blood. The nose, and sometimes the passage of the ears, become involved; and, in the former case, epistaxis has occurred, not readily arrested. In some cases, blebs of serum have arisen especially on the fingers, and, in some cases, urticaria has come on as convalescence approached. Debility remains in a marked degree, even in milder cases, after the local symptoms have disappeared, and I know that fatal cases of syncope have occurred, even when convalescence was supposed to be advanced; but I have not seen this in my practice.

I have mentioned above that there have been several cases of pompholyx. In two of these cases, the first symptoms were those of diphtheria. The first was a very marked case, of which the following is a sketch.—

The daughter of a gentleman's gardener, residing in the garden, and in a healthy situation, aged sixteen, had hitherto been healthy, had menstruated, and was employed in her father's house. I found the membrane formed over the tonsils, fauces, pharynx, and part of one cheek. The next day the nose was implicated, when violent epistaxis came on, which was with great difficulty arrested. Two days afterward, the gums began to bleed; then she vomited blood. On the fifth day, pompholyx blotches appeared on different parts of the body, at first, filled with serum, but soon others appeared, filled with dark blood. These latter rapidly increased in number, so that the whole body, with exception of the face, became, so to speak, covered with them. There was hæmoptysis; blood was discharged with the stools, and the urine was albuminous. This case, after a very hard struggle between life and death, recovered.

The treatment, with the local treatment of diphtheria, to be hereafter described, consisted of bark, ammonia, vegetable acids, abundance of wine, beef-tea, &c.; but chiefly, gallic acid, in full doses. Lastly, the sesquichloride of iron, which seemed to have the most marked effect of any remedy. The recumbent position was most strictly enforced for a long time, as she could not move without evident danger of fatal syncope.

The other case of diphtheria combined with pompholyx, was that of a woman, aged sixty-four, never very strong, and not, perhaps, fully fed; but who had never had any previous illness. She resided in a healthy locality; but the cottage drain was offensive. When I visited her she consulted me for a large bleb on one leg, two inches by one inch and a half, oval, filled with grumous serum; on discharging which a slough formed, and a very ugly sore was the result. Just before this healed, another formed on the other leg, of about the same size, and on a similar spot. The urine was not albuminous. I observed, on my first visit, some thickness of the voice, and examined the throat, of which she did not complain. I found a membrane nearly covering one tonsil. She ultimately recovered; the principles that guided the treatment being the same as in the last case.

With reference to the urine in diphtheria, it is often very abundant, having the character of hysterical urine. I have not examined in all cases for albumen; when I have done so I have found it but in few. The lymphatic glands under the jaw, the small glands down the neck, and even the surrounding cellular tissue, are in some cases affected; but chiefly in strumous subjects. In one case, now under my care, extensive abscesses formed in the cellular tissue.

With regard to the localities in which diphtheria shows itself, it has not, in my experience, selected the malarious spots; but when it has done so the cases are more urgent. In many years practice in one locality those spots become known, and it happens that I have paid much attention to that subject in this locality. At each of its epidemic visits, cholera has selected those spots. As I have elsewhere observed, some years since, "Where fever had been, there came cholera," but not so diphtheria. It has not altogether *avoided* those spots; but has not by any means *chosen* them.

At first, I was disposed to doubt if it was communicable; but I have evidence to satisfy me that it is contagious to a limited degree.

Have we seen this disease before? and what is its nature? In answer to the first question, I can say confidently that during a period of upward of thirty years' practice I had seen no case of diphtheria until 1857. I had read Bretonneau's earlier papers, many years since, and should have recognised the disease had it presented itself. Of its nature it is less easy to speak. It is evidently, I think, a blood disease, and not merely a local one. But what is the nature of that abnormal condition has yet to be explained, or rather, I fear, has yet to be discovered.

In observing the progress of this epidemic, I have been instinctively led to reflect on the altered type of disease in general. I have myself no doubt of that alteration in the type of disease, observed since the year 1832, in England. From that date there has been a departure from the old sthenic type, and this has been more pronounced the last few years, until at length a genuine sthenic form of illness is almost, if not quite, unknown amongst us. We have instead, low types of inflammation, low forms of cutaneous diseases, low types of fever, having more and more a tendency to the remittent form; and a very marked increase, in localities where it was before almost unknown, and where no known local causes have arisen to occasion it, of intermittent fever. What was before a mere chill, a slight cold, thrown off with the first reaction, becomes now an attack of ague.

We have abundant evidence of this depression of vital power in the general symptoms of diphtheria. We have also a low type of local inflammation in unison with the general type; but why it should just now seize the throat as its local seat instead of showing itself as boils, carbuncles, whitlows, thecal abscess, necrosed bone, and in kindred forms, I do not know. But this I may venture to say, if I attack the malady with the local appliances, and the general treatment I should employ for erysipelas, the cases recover.

Treatment.—The principles that have guided my treatment of this disease, are: *first*, to arrest the local inflammation by exciting another of a different character; *second*, to employ elimination according to the individual case; *third*, in all cases to sustain vigorously the vital powers.

To accomplish the first indication, I prefer the employment of a strong solution of the nitrate of silver. Having first cleared the fauces, &c., as far as practicable by gentle means, I paint every affected part,, and *beyond* it, with the solution, of the strength of fifteen grains to a drachm. In mild cases I have frequently tried one of milder strength, say five grains; but I am satisfied that in all cases an efficient application of the full strength is the best. It is perfectly safe, and has at once a marked effect. It is more efficiently applied by a full-sized camel-hair pencil than a sponge. Severe cases must be seen again in twelve hours, and the application repeated should the so-called membrane spread. Later in the treatment, a weaker solution may be used, or Bretonneau's application, one part of hydrochloric acid to three of honey. And later still, when the membrane has disappeared, but much fullness and puffiness of the parts continue, a gargle, containing the sesquichloride of iron, or tannic acid. Where, as in my second case, there is much fœtor, the chlorate of potass is applicable. And where, as in my third case, there is more tonsillitis, we may with advantage, employ inhalation of steam, or warm milk gargle. After the membrane is removed, and the tendency to diphtheritic deposit supposed to be arrested, the throat must be carefully watched; for until the endemic condition of the system is conquered, we may have a relapse of diphtheria.

I commence the treatment of almost every case with a purge, varying with the state of the tongue, pulse, &c.; but by far the most frequently, calomel and rhubarb, carefully avoiding salines. In some cases, with loaded tongue and suffused countenances, I have given, with the greatest advantage, emetics. Indeed, I am now so satisfied of their value, that I shall for the future, employ them more frequently, especially where the congestion is marked, or there is unusual tonsillitis. The further general treatment is of *great importance*, namely, that directed to sustain the vital powers and remove anæmia.

I need not dwell upon the necessity of wine, beef tea, &c. In the severe cases these are most urgently required, and must be liberally supplied. In the more trifling cases, if well marked, convalescence will be delayed, and danger of relapse continue, if these, or their equivalents, are not employed.

Of all the medicines that may present themselves for our choice, there is one far superior, in my experience, to all others; and upon which, I indeed, chiefly rely: tincture of sesquichloride of iron. I have tried others that were obvious; but none sustain the vital powers, steady the pulse, lessen its frequency, and give potency to it; none remove the soft clam of the skin, steady the action of the kidney, and remove the anæmic pallor of the face, as does this. My confidence in its employment, and also in the use of the nitrate of silver, is fortified by their effects in erysipelas, in which they are almost specific. Cases will occur in which this treatment must be deferred, or modified, as where the tonsillitis is severe. In those cases, with the appropriate local treatment, I have first used the decoction of cinchona, with liquor of acetate of ammonia, or the latter with ammonia; but we afterward come to the steel.

Such is a brief outline, and time admits of no more, of the treatment of cases in which croup has not intervened. How are we to meet this formidable extension of the disease? Shall we, in any cases, resort to tracheotomy? I think not. Success, in reported cases, has not justified it; and we cannot tell how far the membranous deposit has extended. I have had urgent cases of this description, and, happily, have hitherto treated them with success. My sheet-anchor is emetics, repeated, and very active ones, always of ipecacuan and sulphate of zinc, never of antimony.

Did time admit, I would detail these cases, but they present no peculiarity except the urgency of the symptoms. In one child, three years of age, I gave seven emetics before the symptoms were fully relieved. Portions of the membrane were detached and thrown off in the act of vomiting. I gave wine and ammonia in the intervals. In this case I gave also repeated small doses of calomel, because Bretonneau recommends it: and the case being of extreme urgency, I would not neglect one of such authority.

In the more severe cases of diphtheria, I cannot too impressively recommend strict horizontal position. I have seen more than one case in which fatal syncope was to be apprehended if this had been neglected.—*British Med. Journal, July* 16, 1859, *p*. 563.

9.—SOME ACCOUNT OF DIPHTHERIA AND EPIDEMIC SORE-THROAT, AS THEY HAVE PREVAILED IN THE PARISH OF ISLINGTON IN 1858-9.

By Dr. Edward Ballard, late Lecturer on the Practice of Medicine at the Grosvenor-place School of Medicine: Medical Officer of Health for Islington.

Relation of Diphtheria to Scarlatina.—The following facts may contribute toward solving the problem of the mutual relation of these diseases. In themselves they appear opposed to the hypothesis that they are merely phases of the same malady:

1st. In 7 cases of Class 1 the diphtheritic sore-throat was stated to have been associated with recent scarlatina. In only two of these did the rash appear in the course of the throat affection. Out of the remaining 5 cases in which the rash appeared first and the diphtheritic sore-throat secondarily to it, the rash had in 4 cases left the skin before the exudation upon the mucous membrane made its appearance. I may add that in other instances that have come under my notice both the rash and sore-throat of the scarlatina have quite disappeared before the symptoms of diphtheria set in.

2nd. In 47 instances, 38 of them being of Class 1, I was able to obtain satisfactory information as to the patient having suffered from scarlet fever at more distant periods. The following represents the proportion of those who had and had not thus suffered:—

—	Had Scarlatina previously.	Not had Scarlatina previously.	Total.
Class 1	9	29	38
Class 2	1	2	3
Class 3	2	4	6
Total	12	35	47

This proportion of 1 to 3 does not, I imagine, differ greatly from that which would be found to exist among persons selected in any other manner from the same ages and the same stations of life. In the 12 patients who had had scarlatina, the occurrence of the disease was dated back to periods varying from six months to five or six years. Now, few occurrences in medical experience are more rare than a second attack of scarlet fever; all writers seem to agree in this. Dr. Willan only met with a single instance in 2000 cases, and Bouchut says that he has never met with a well-authenticated case at all. Still, as I desire to state this question fairly, I may say, that, although I have never myself seen a second attack of scarlatina with rash, I have on more than one occasion seen persons who have had scarlatina attacked with sore-throat when attending upon persons suffering from true scarlatina, and this at a period as short as a year from their own attack. This appears to me a new mode of viewing the matter; it is one on which the experience of the profession should be expressed.

There is a Polymorphism in Disease, as in crystallography. Witness the varieties of true and marked agues, the almost identity of small-pox and vaccinia, the undoubted relation of erysipelas and puerperal fever, of regular and irregular gout, of the several forms of cancer, &c. And with respec to scarlet fever, practical accoucheurs know very well the danger to which a puerperal woman is exposed when subjected to the influence of scarlatina poison. An impression has long been growing up in my mind that there is a form of fever to which they are liable that bears to scarlet fever the same relation that another form does to erysipelas, and that this fever is of a very fatal type. I have seen several such cases in consultation where no rash or even sore-throat have been developed, but where the woman has sunk as under the influence of a powerful depressing poison. I know that true scarlatina with rash may be developed under such circumstances; but what I am now speaking about is a fever without rash, and bearing characters different from ordinary scarlet fever.

It may be that the amount of protection afforded by a previous attack of any of these polymorphous maladies in one shape against an attack in another shape varies from *nil* to something as nearly absolute as can be. It seems to me that this is a point to which scientific enquiry might be usefully directed.

3rd. Just as an attack of true scarlet fever furnishes no protection against an attack of diphtheria, so the following case indicates that diphtheria furnishes no protection against scarlet fever. Three children in a family in my district were attacked with diphtheria in August, 1858. Two of them died; the third, aged three years, recovered. I saw these children, and satisfied myself that there was no error in the diagnosis. In January, 1859, the child that recovered was attacked with scarlet fever, after playing about upon a carpet brought from a house where a fatal case of this disease had occurred. There was both the rash and the usual throat affection, but no diphtheritic exudation; and the child died.

4th. Among all the cases, fatal, or otherwise, that I have investigated, I have not met with any instance in which diphtheria has appeared to have been communicated from a patient with scarlatina, except where the scarlatina has been communicated also. Neither have I at present met with more than one instance in which cases of diphtheria and cases of scarlatina occurred about the same time in such a manner as to favour the idea that they were varieties of the same disease.

5th. The mortality from epidemic sore-throat during 1858, if this disease were but a form of scarlet fever, would be expected to have borne some relation to the mortality from the latter decease, and to have fluctuated in harmony with it. Such, however, was not the case, as shown by the following table:

DEATHS WHICH OCCURRED IN ISLINGTON DURING EACH OF THE FOUR QUARTERS OF 1858.

——	1st Qr.	2nd Qr.	3rd Qr.	4th Qr.
Scarlatina	9	4	34	69
Epidemic sore-throat	5	15	22	15

The greatest number of deaths from epidemic sore-throat occurred in the third quarter of the year, but from scarlatina in the fourth. The number of deaths from epidemic sore-throat was the same in the second and fourth quarters, while in the former there was only 4 from scarlatina, and the latter 69.

I may add that as scarlatina declined in the first quarter of the present year, the deaths from epidemic sore-throat (nearly all diphtheria) increased. Thus there were registered 29 deaths only from scarlatina, but 30 from epidemic sore-throat.

Again, the greatest mortality from scarlet fever occurred in the six weeks from October 17 to November 27. During this period 48 persons died of scarlet fever, but only 5 of any form of epidemic sore-throat, and during 4 of those weeks, viz., from October 24 to November 20, no patient died from epidemic sore-throat. When, however, the mortality from scarlet fever was checked, as it was most remarkably, on the occurrence of the very severe cold about the 26th of November, the deaths from epidemic sore-throat reappeared in the returns.

Relation of Diphtheria to other Forms of Sore-Throat.—The prevalence of sore-throat not diphtheritic in character during the past year, has been matter of general remark. Many, if not most of these throats exhibited some approach to the colour of the mucous membrane when about to become the seat of diphtheritic exudation. These sore throats appear to bear the same sort of relation to diphtheria as diarrhœa bears to cholera in epidemic seasons. Just as in any cases of diarrhœa in an epidemic period, it is impossible to predicate that it will not pass into cholera, if neglected, so, in the ordinary sore-throats which have lately presented themselves, no one would be bold enough to assert that any one might not before long exhibit the characteristic symptoms of true diphtheria. A fact upon which I would especially rely in support of this view, is the occurrence of non-diphtheritic sore-throats equally with diphtheritic sore-throats among the members of families where deaths from diphtheria had taken place. Out of forty-seven families in which a death from diphtheria occurred (Class 1.)

	Families.
Some other members of the family suffered from diphtheria in	9
From other forms of sore-throat in	15
Some members suffered from diphtheria, and others from sore-throat, in	8
Neither diphtheria nor sore-throat occurred in other members of the family, in	15
	47

The severity of these cases of sore-throat varied considerably. In some, it was a slight attack, lasting but a few days; in others, there was so much febrile disturbance as to demand medical assistance; in others, again, there was ulceration, suppuration of the tonsils, or sloughing of some of the tissues. In some families the earlier cases were of sore-throat, in others the earlier cases were diphtheritic.

Is Diphtheria an Infectious Disease? I will state the facts which incline me to the affirmative.

1. A *prima facie* case appears made out by the facts mentioned in the last section. Infectious diseases habitually spread in families they invade. Out of forty-seven families there were only fifteen in which the other members all remained healthy. Of course it may be argued, in opposition, that all the members of a family are equally exposed to the operation of local causes of disease.

2. As a rule, it spread in the houses it invaded chiefly among those members of the several families who were most closely in communication.

3. In no case where separation from the sick person has been effected early in the disease have I noticed that it has spread to the separated individuals. In one case where communication had been allowed for three days before separation, a child was seized with diphtheria on the sixth day of removal from home.

4. The following special instances may be adduced of communication of the disease from one house to another, viz.:—

Jane J., aged ten years, resided at Islington with her mother, an aunt, and three sisters. On May 1st and 2nd she was on a visit at the house of an uncle, whose daughter, Jane's cousin, was kept at home because she was believed to have a cold. On the 2nd this child exhibited decided symptoms of diphtheria; the attack was slight, and she recovered. On May 6th a servant in this house was taken ill with a severe attack of diphtheria, recognised as such by the physician at St. Bartholomew's Hospital, to which she was removed, and where she died. On the 2nd, Jane returned home, was taken ill on the 3rd with diphtheria in a severe form, and died on May 9. Her mother, and a sister aged fourteen years, were both taken ill on May 11th. She had not been so much with her daughters as other members of the family up to the 8th, when she sat up with her all night. She was of a highly nervous temperament. The tonsil sloughed, and there was a complete cast of the trachea expectorated. She died on the 18th. The sister, who was also attacked on the 11th, slept with her mother, and when not at school was continually in and out of Jane's room, sitting there sometimes for hours together. She died on May 14, asphyxiated. Another elder sister who slept with Jane and the aunt, suffered from nothing but a slight sore-throat.

On September 18, George B., aged two years, was attacked with what was believed to be diphtheria. He was an obstinate child, and the throat could not be fairly inspected. The tissues, however, under the jaw were swollen, and there was difficulty in swallowing; the breath became fetid, the voice lost, there was great dyspnœa, and a discharge from the nose. He died on the 24th. His brother, Edward B., aged nine months, was attacked on September 22, that is, on the 4th day of George's illness. In this case the diphtheritic exudation was marked; there was great external swelling, great dyspnœa, fetor of breath, and bloody discharge from the nose, and from the first the child refused the breast. He died also on the 24th. The father and mother both suffered from sore-throat, being attacked on the 25th. A grandmother also had sore-throat on the 24th, 25th, and 26th. She laid out the children on the 24th, and immediately returned to her own residence in another part of the parish. A child who lived with her, aged sixteen months, a cousin of those who died, was seized with sore-throat and fever on the 29th. I saw him on October 4. There was then the diphtheritic membrane commencing upon one of the tonsils. He was attended by a general practitioner in the neighbourhood, and died on the 19th.

About the middle of May, sore-throat began to prevail in a large school in Islington. One of the boys, who had just returned home to Kingsland for his holidays, was seized with diphtheria on May 23rd (Whit-Sunday.) The attack was characteristic, and he recovered with difficulty. On June 5th, a boy, aged nine years, who had not returned home, was attacked with true diphtheria, accompanied by laryngeal complication, and died on the 8th. On June 9th, another boy, who was on a visit with some friends for his holidays, was seized with diphtheria; the exudation was well marked, and in consequence of the laryngeal complication, tracheotomy was performed, but he died on the 12th. The sanitary arrangements of the school were not objectionable; the drainage good, and the water-cisterns regularly cleansed. On June 15th another boy returned home from the school with diphtheria. Previously to his return, all the family were in good health. Early in July a sister, aged two years, was attacked with diphtheria in a marked form, and died on July 22nd. A brother also was attacked with diphtheria and recovered, and three female servants also had slight attacks. Another sister had an attack of sore throat. An infant and the father and mother all escaped.

A little girl, aged six years, went to a day-school. A child belonging to the lady that kept the school was ill with diphtheria. On March 3rd, the little girl was attacked, and I saw her twice in consultation. The exudation was well marked and extensive. She died from prostration on March 24th. The father and mother both suffered subsequently from a slight attack of sore-throat. A brother of the patient, who was carefully kept apart from her and ultimately sent away from home, escaped. In this instance, however, there was a local source of disease in a very offensive condition of the drainage of the house, and the water-cistern was disgustingly foul.

The connection of Epidemic Sore-throat with Local Causes of Disease.—The following table exhibits the results of inquiries instituted at fifty-seven houses where fatal cases occurred:—

	No. of Houses.	House damp, offensive smells, or defective drains.	Overcrowding or defective ventilation.	Foul drinking water and other nuisances or noxious accumulations.	Nothing discovered amiss.
Class 1........	46	19	4	6	20
Class 2........	6	3	..	..	3
Class 3........	5	2	..	2	2
Total........	57	24	4	8	25

In more than half the houses, then, which were examined, there was some defect or other in the sanitary arrangements, or in the surrounding conditions of the patient. In the greater number of the houses thus deficient, the fault was discovered in the state of the drainage.—*Med. Times and Gazette, July* 23, 1859, *p.* 77.

10.—TREATMENT PREVENTIVE OF THE SEQUELÆ OF MEASLES AND SCARLATINA.

Many precautions are adopted by physicians to prevent the unfortunate sequelæ of these diseases, and the confinement to the sick chamber for several weeks after convalescence ranks among them. To avoid this, M. Scoutetten, of Metz, has devised the following method, which we find in the 'Gazette Hebdomadaire,' for April 1, 1859.

As soon as convalescence commences, that is to say, when the skin is no longer red with the eruption, he rubs over the whole body slightly warmed oil of sweet almonds or olive oil, and puts the patient in bed again, for two hours. The next day he gives him a tepid path for an hour, then places him in bed, and if the skin is very dry, a new friction with the oil is made. These two frictions and one bath, are usually enough to remove all danger. Still, in severe cases, it is well, to avoid any risk, to repeat the means indicated from time to time, until the skin regains its suppleness. These precautions taken, convalescents may be permitted to go out without fear of bad results.

In order to justify this method and explain its importance, it is necessary to remember the state of the skin in infants affected with measles and scarlet fever. At the commencement of the disease, the dermis is red and swollen; during convalescence the tissues return to their normal condition, but the epidermis, which has been distended, not being elastic, becomes detached, and falls off in the shape of fine powder when the attack of measles has been light, or is removed in large scales when it has been severe, and especially when the eruption has been that of scarlatina. The skin beneath is dry and harsh; perspiration and transpiration are badly performed, and the functions of this important organ are impeded or suspended. When the skin acts badly, the kidneys and mucous membrane of the air-passages or of the digestive apparatus undertake to supply its place; thence arises a thick, sedimentous, and sometimes albuminous urine, severe diarrhœa, which terminate in emaciation and death: obstinate coughs, sore throat, croup, pneumonias, pleurises with effusion; finally, serous infiltrations into the areolar tissue of the limbs, or accumulations of liquid in the abdomen. and in other cavities where serous membranes exist. These severe symptoms occur after an exposure to cold of the skin, which inflammation has rendered more sensitive, and the functions of which are interfered with by an inert epidermis, which obstructs its pores. The object of the treatment proposed by M. Scoutetten is to oppose the causes of these symptoms.—*American Medicat Monthly, Aug.* 1859, *p.* 114.

11.—ON THE TREATMENT OF RHEUMATIC FEVER.

By Dr. G. Whitley, Registrar to Guy's Hospital.

[The object of this paper is to ascertain, as far as possible, by an examination of cases treated at Guy's Hospital, by the different physicians of their large staff, what mode of treatment is most successful. Cases of acute fibrous rheumatism alone have been selected for this purpose, being the most definite in their character. The exciting causes of acute rheumatism are cold and moisture; but, there is no more active aiding cause than over-fatigue of a joint. Now if we look upon the heart as being virtually a joint, it becomes easy to understand the liability of persons with irritable hearts to cardiac affections in rheumatic fever. Dr. Whitley continues:]

When we proceed to enquire into the pathology of rheumatic fever we are little better off than before. Several high authorities speak of acute rheumatism as being essentially a blood-disease, a theory which appears to receive much support from one of its most striking phenomena, viz., a great excess of fibrin in the blood. The other chief change observed in that fluid is the presence of a large quantity of uric acid. The evidence in favour of other important changes in the blood, such as an excess of lactic acid from checked cutaneous excretion, is too vague at present to be taken into account.

The excess of fibrin in the blood being assumed to be the most essential change observable in rheumatic fever, while the most striking symptom of the disease is an affection of fibrous tissues, especially the joints, consisting mainly in the exudation of fibrin, the question of the priority of the joint-affection or the blood-change presents itself as one of paramount importance. Here, I confess, I am inclined to adopt the view of Dr. F. T. Bond, that the excess of fibrin in the blood precedes the affection of the fibrous tissues, though traumatic lesions, such as burns, no doubt produce a similar excess. To use Dr. Bond's own words, "Is the hyperinosis merely an effect of the reaction of the local disease upon the system at large, as is generally believed, or is it the primary source of the exudation, the causative agent of the latter, without which it could never exist? I believe this last to be unquestionably the true statement of the case, and shall endeavor to bring satisfactory evidence that it is so. Considered simply in a teleological point of view, the former supposition throws no light whatever upon either the economical object of the hyperinosis, or upon the mode of its connection with the local affection; whereas, on the latter hypothesis. it is highly probable, *à priori*, that an excess of fibrous plasma, rapidly accumulating in the blood to an amount which is incompatible with perfect health, should be attracted as it were, out of the vascular system, in the shape of an exudation, to those tissues for which it has a physiological affinity—such a process, in fact, being no more than a mode of excretion."

I take this opportunity of alluding to a point of great importance in the diagnosis of rheumatic fever, often mentioned by Dr. Addison, viz., the fact that the heart is not unfrequently affected before the joints or any other part of the body. Neither is it difficult to understand that this should be so, if we regard the exudations of acute rheumatism as being brought about by fibrous tissues acting as excreting organs, while an irritable heart assumes the position of a fatigued joint. Thus I have seen a case in which delirium was attributed to inflammation of the membranes of the brain, but in which the character of that delirium, together with an irritable state of the heart and want of clearness in the heart-sounds, without any positive *bruit*, induced me to venture upon a diagnosis of rheumatism, speedily confirmed by the supervention of pain and swelling of the joints.

The present inquiry being directed more particularly to treatment, I shall not do more than allude to Lehmann's ingenious theory of the possible existence of several (allotropic) varieties of fibrin, the physical or physiological relations of which to various fibrous tissues in the body may serve to determine the site of the exudations so common in rheumatic fever.

[The writer then proceeds to give shortly, but clearly, twenty-three cases of rheumatic fever of the acute fibrous kind, treated by various physicians. He observes that though this number is small, he has observed a far larger number of cases, an examination of which only strengthens the conclusions derived from this smaller number.]

In fifteen of these cases salts of potash were given, either at first or after the failure of other means. Where cardiac complications existed, these remedies were combined with calomel, antimony, and opium, and with blisters; while in some of the simple cases of joint-affection they were administered quite alone. In no case did they fail to effect a cure, which, even in severe cases, was sometimes attained very rapidly. The latter remark applies, indeed, to some recent acute cases treated with lemon-juice, but in many instances this remedy unfortunately proves ineffectual. The treatment with large doses of bicarbonate of soda not having, so far as I know, been employed at Guy's Hospital, I take this opportunity of giving the results of its use at the German Hospital, with the particulars of which my friend Dr. Hermann Weber has kindly furnished me. He has tried it in about sixteen cases. From six drachms to an ounce were given in twenty-four hours. In five cases the effect was very marked; the pain and swelling ceased within forty-eight hours; the pulse sunk within the same space of time from between 100 and 125 to between 65 and 55, the temperature of the body becoming likewise much lower than before the administration of the remedy. On the third or fourth day the pulse sank sometimes still more, in one case as low as 45. The profuse watery perspiration was less perceptible, the tongue became cleaner, the urine, of course, quite alkaline.

The diminished frequency of the pulse remained for some days after the remedy had been left off, and disappeared only by slow degrees.

In five or six of the other cases, the administration of the bicarbonate was followed by an alleviation of pain and a diminution of the frequency of the pulse, but by no means to the same extent as in the cases already alluded to. In the remaining cases, the remedy did not seem to exercise any influence at all on the prominent symptoms of the disease.

The cases in which the remedy was most useful were young persons, in whom the disease was attended with much pyrexia, considerable pain, marked swelling. increased perspiration, and the peculiarly furred tongue. They were, therefore, instances of the well-pronounced acute type, while the cases in which there was no effect were less acute—less according to the normal type of the disease.

We have now to inquire into the mode of action of some of the remedies employed in rheumatic fever. All that we know with any certainty of the effect of large doses of lemon-juice is, that it soon diminishes the frequency of the pulse. It has been well shown by Dr. G. O. Rees how it *may* serve to facilitate the conversion of lithic acid, introduced into the circulation in excess as a consequence of mal-assimilation, into urea. Now, the two chief indications in the treatment of rheumatic fever are

1. To prevent the formation of an undue amount of lithic acid, or to favour its conversion into urea.
2. To facilitate the elimination of the fibrin present in excess in the blood.

Even if we assume the reality of the action of lemon-juice suggested above, the latter indication remains unfulfilled. Much the same may be said of colchicum, which "abates pain, purges, and lowers the pulse, *i.e.* is cathartic and sedative," as it is not borne out by facts that this remedy augments the quantity of lithic acid in the urine, though it may tend to prevent its formation in the blood.

Alkalies, on the other hand, not only act as solvents for lithic acid, but also dissolve fibrin. In addition to this, their neutral salts, especially those of potash, are diuretic. The latter, therefore, appear to me to unite more completely than any other known remedy all the properties requisite in the treatment of an attack of rheumatic fever unaccompanied by any affection of the heart or other important organ. Even where cardiac or other complications indicate the use of mercury, opium, blisters, or local depletion, these are only to be regarded as accessories, the simultaneous employment of the alkaline treatment seeming to afford the best prospect of a certain and speedy cure.—*Guy's Hospital Reports*, 1859, *p.* 187.

DISEASES OF THE NERVOUS SYSTEM.

12.—ON THE NATURE, SEAT, AND RELATIONS OF NEURALGIA.

By Dr. C. Handfield Jones, F.R.S, Physician to St. Mary's Hospital.

[In most cases of neuralgia not arising from organic cause the accompanying debility or prostration is almost as marked a symptom as the pain.]

It is proved by experience that, unless this debility and prostration can be removed, and replaced by healthy vigour, no real progress can be made in the cure of neuralgia. The task is like that assigned to Sisyphus, the patient's and doctor's hope is worn out by ever-recurring relapses. The debility seems in a special manner to affect the nervous system. The brain is languid and dull, and inapt for mental labour; sometimes its function actually fails, and wandering or delirium occurs. Stimuli are beneficial, often very markedly so, though their effect is temporary. Fresh pure air, good food, sufficient repose alternating with exhilarating employment, supplemented or aided, if need be, by nerve tonics, are the real remedies, and just in proportion as they increase the general tone and strength does the patient attain complete recovery and immunity from relapses. On the other hand, just as surely do all causes of debility confirm, increase, and render inveterate the malady.

Now, it may be fairly argued that when the symptoms of debility, and especially of nerve debility, are so apparent, and have so distinct a relation to the particular symptom, this must be itself of like essential character. It can hardly be that the morbid state of the nerve affected can be greatly different from that which prevails so generally throughout the system, especially when we consider the means which avail for the cure of both. Romberg's metaphorical expression, speaking of anæmic hyperæsthesia (i.e., neuralgia), that "it seems as if pain were the prayer of the nerve for healthy blood," is, in all probability, exactly true. The nutrition of the nerve being ill performed, its structure undergoes some molecular alteration which conditionates pain. What is true of neuralgia from this cause I believe is true of all cases belonging to the non-organic class. Electrical disturbances, damp cold, malaria, seem to me all to act in the like way as far as we can judge—viz., by deranging the molecular nutritive actions of the nervous structure, and so impairing its function. There are several circumstances which seem to me strongly to support this view. One is the very frequent co-existence of numbness with the neuralgic pain, especially in highly sensitive parts, as the fingers and hands. One cannot say in what the condition producing numbness differs from that producing pain; but it is clear there is no opposition between them; both are often present together, and the numbness commonly remains as the more permanent condition in the intervals of the paroxysms of pain, and even after they have ceased to occur. Now, numbness is evidently a failure of functional action. Of the same import is the occurrence of various degrees of muscular paralysis, which is often associated with neuralgia, evidently as an analogous affection of the motor nerves. It yields to the same treatment. The phenomena of myalgia may also be referred to in illustration of the nature of neuralgia. Here we have a manifest instance of the relation of pain to debility; the sensory nerves of the muscles express pain because they are weak; whatever increases the debility increases the pain, and *vice versâ*. The relation of ague to neuralgia is worth considering in respect to this question. It is certain that neuralgia may be a manifestation of malarious influence just as much as ague, and that the two may replace each other. It may also be affirmed that in neuralgia (non-organic) from other causes, the pain-causing condition of the nerve must be the same as in malarious neuralgia. Now, in an ague fit there is no doubt that the vaso-motor nerves are in a paralytic state, consequently it is probable that in a neuralgic paroxysm the sensory nerves are similarly affected. Lastly, we may allude to the cure of neuralgia by Faradization as an illustration of its nature. The pain of a sensory nerve and the paralysis of a motor may both be removed by the stimulus of the interrupted current. This surely indicates that both states are similar.

Even in organic neuralgia, it seems to me a matter of much question whether the nerve affected is in a state of exalted excitability, or simply of deranged and disordered nutrition. In lead poisoning, the motor nerves of the muscles are certainly paralysed, the pains are diminished (Romberg) "by pressure and friction," and the whole phenomena are indicative of diminished, rather than of increased vital actions. The curative action of the sulphuret of potassium bath is only intelligible by regarding it as a peculiar stimulus to a great sensory surface, which is reflected from the nervous centres on the paralysed nerves and muscles. That it does produce muscular contraction, at least in some cases, is, I believe, certain. In gouty neuralgia, if we take colicky and spasmodic affections for examples, the disorder is much more of an asthenic than hyperæsthetic character. The pain and suffering attending a characteristic outbreak of gout in the foot have much more the features of hyperæsthesia than the colicky disorder. That a nerve which receives for nutrition blood poisoned by uric acid should be disordered in its acting, and thrown into a state conditionating pain, is very intelligible, but it can hardly be regarded as having its irritability exalted. On the other hand, the nerve lying in a focus of inflammation, by reason of the active hyperæmia, would seem really to be in a state of hyperæsthesia. Its condition is analogous to that of the nerves of one posterior limb in Brown-Séquard's experiment of transverse semi-division of the dorsal cord, where hyperæsthesia is produced in consequence of paralysis of the vasomotor nerves, and the resulting hyperæmia.

From the considerations which have been advanced, I am led to conclude, that in the majority of cases neuralgia essentially implies a lowering of the vital power and functional action of the nerve, not an increase. There are, however, certainly cases in which the painful parts are not manifestly hyperæmic, but are yet excessively tender, and intolerant of the least pressure. In these, it is clear that the excitability of the nervous apparatus is morbidly increased, yet I question whether the term hyperæsthesia is properly applied to them. In the state referred to, any, even the least, excitement brings on or aggravates the pain. This certainly implies an undue mobility of the nerve-structure, a readiness to be thrown into the pain-causing condition, but by no means a real increase of sensory power. It is by no means clear that a part in this state would appreciate two points as separate at a smaller distance from each other than it would when healthy. I should not regard such a condition as identical with that induced by partial division of the spinal cord, as in Brown-Séquard's experiments, or by strychnia poisoning I think it probable, that in these cases the morbid action is seated more peripherally toward, or in, the cutaneous terminations of the filaments; while in ordinary neuralgia the larger ramifications or the trunks are affected.

From the preceding discussion, we pass to the consideration of the question—What is the real seat of neuralgia—in the nerves or in the centres? Obviously, this is no easy question to answer. According to the law of eccentric phenomena, every sensation of which we are conscious is referred to the peripheral termination of the sensitive fibres (so Romberg writes). Bowman and Todd add that the sensation is referred to those parts, and to those only, to which the fibres irritated are distributed. According to this view, then, all appreciation of sensation as referred to any point in the course of the nerve is out of the question. An irritation, wherever set up, must be felt at the peripheral extremity of the fibres implicated, and never in any part of their intermediate course. But there are facts which are strongly opposed to this exclusive dogma, and which seem to prove that a sensation may be referred to various points in the course of the nerve-fibre. If we hit our funny-bone, although, no doubt, pain and tingling are felt at the peripheral distribution in the fingers, yet the chief agony is in the trunk of the ulnar nerve at the part struck, and certainly not merely in the skin covering it. The circumstance dwelt on by M. Valleix, that the specially painful points in nerves affected with neuralgia are always those where the nerve becomes superficial, is also a proof of a sensation being referred to other points besides the terminal. The same may be said of the pains which patients describe as shooting down along the track of a nerve, as the sciatic. These certainly are not located merely in the skin which covers in the nervous trunk.

From these considerations, I am led to admit the possibility of very numerous exceptions to the law of eccentric phenomena, and to believe that pain in a nerve may really indicate by its situation the seat of the irritation or other morbid action. This is a conclusion of some importance to the local treatment of neuralgia. It justifies our empirical habit of applying sedative remedies as near as possible to the seat of pain. But of course we cannot affirm, in any case of pain involving the trunk of a nerve, that the morbid action *may* not be central; the law of eccentric phenomena holds true so far as that central disorder may certainly give rise to peripheral sensation. The only means of certainty distinguishing the site of the pain-causing action is division of the affected nerve. If this arrests the neuralgia, we know the disorder is seated peripherally: if it fails to do so, we know we have to seek more centrally. In a very large number of cases, I fear it must remain problematical as to where the real seat of the disorder is. If—the pain being specially referred to some intermediate spot—injection of opium at that part (subcutaneous) should give more relief decidedly than the same dose at a distance, it would afford ground for believing that the cause of the neuralgia was localized in that spot. In the ordinary way of rubbing sedative liniments on the cutaneous surface over the seat of pain, we have no means whatever of proving a local action upon the suffering nerve, but rather the reverse. For take the case of the sciatic nerve, where pain is acutely felt at the back of the thigh, and notably between the ischiatic tuberosity and the great trochanter; if this is relieved by a sedative application to the covering cutaneous surface, we are sure that the chief action of the remedy must be on cutaneous ramifications of the glutæal, lesser sciatic nerves, and branches of the external cutaneous and other nerves on the front of the leg. These will convey impressions to the spinal centre, not far from the part where the roots of the sciatic are implanted; so that if the neuralgia were of central origin, it is very conceivable that the morbid action might in this way be beneficially modified. But, considering the depth at which the sciatic nerve lies from the surface, it seems quite impossible that the aconite, chloroform, &c., should penetrate so far through skin, fat, and fascia, or even muscles. There exists some evidence to show that any strong impression made on the centre (such as cauterizing the ear, galvanizing the columna nasi) through incident nerves may put a stop to some neuralgia.

The relations of neuralgia are of course very different according to the cause which gives rise to it. If, however, we take the commonest kind—which arises from cold, malaria, debility—we must allow that it manifests a very close affinity with non-febrile *rheumatism* Rheumatic and neuralgic pains are frequently so very similar, that they are only to be distinguished by the action of remedies. Iodide of potassium cures the rheumatic, quinine and iron the neuralgic; while often it occurs that in the same case, after having begun with the former, we have to resort to the latter to complete a cure. The beneficial action, noticed by several recent observers, of muriate of ammonia in neuralgia, can scarcely be dissociated from its remarkable and positive remedial action in muscular rheumatism. The interesting but obscure phenomenon of rheumatic paralysis is closely similar to, if not identical with, the paralysis or paresis of motor nerves which so often forms a part of neuralgia. *Catarrh* is allied to neuralgia by the similarity of its causes, the manifest implication (sometimes to a grave extent) of the cerebro-spinal nervous system, the resemblance of its inflammatory actions to those sometimes accompanying and depending on neuralgia, and in a large number of cases by its "juvantia." If exhaustion aggravates a neuralgia, so does it also a catarrhal flux; while rest and toning means have an opposite effect. The affinity between neuralgia and *ague* in malarious cases is strikingly apparent; the two disorders so evidently replace each other, that there can be little doubt that the difference is only one of situation: the sensory nerves being affected in one case, the sympathetic system in the other. The therapeutic effects of arsenic and of quinine in ague and in common neuralgia, *rapprochent* the two disorders not a little.—*Lancet, Sept.* 10, 1859, *p.* 258.

13.—ON EPILEPSY.

By Dr. Brown-Sequard.

[If you divide in a guinea-pig one-half of the spinal chord, in the dorsal region a small spot of skin in the face becomes extremely sensitive, and irritation of this causes an epileptic paroxysm. If this spot of skin be excised, no more fits can be produced. The same sort of thing occurs in the human subject, the spot of sensitive skin being analogous to the spot whence springs the aura epileptica.]

There are probably many of these cases in which a cure of the disease might be easily effected. These means consist in applying a ligature in those cases, which unfortunately are not the most numerous, in which you know there will be a fit, because it comes on after a general disturbance. The greatest difficulty is in those cases in which the fit is not preceded by any premonitory symptoms; yet the attempt to ascertain the seat of the peripheral irritation is not altogether hopeless. In cases known to be epileptic, but not having the perceptible aura, we may apply ligatures to all the limbs, alternately and successively, beginning at the upper part of each limb, and then by degrees shifting the compression downward, if necessary, even to the fingers and toes. There are cases of epilepsy, however, in which the patients will not submit to this or even any other mode of treatment. Sometimes we meet with persons in which a fit of epilepsy will be induced by simply touching a particular part of the body, susceptible of irritation. One such case, which came under my notice, was that of a young man, who lately killed himself, and who was a student of St. Bartholomew's Hospital, London. There was a particular spot on the top of his head that could not be touched without producing a complete epileptic fit; and his fellow-students very improperly used to amuse themselves by thus producing fits of epilepsy, or at least excessive vertigo, in this unfortunate man. I told him what was best to be done to effect a cure, but he would not allow it to be carried out, and he lately put an end to himself. The treatment of such a case consists in destroying the sensitive portion of the surface by cauterization; not by caustics, but by the actual cautery; and if cauterization does not succeed, a cure may still be effected by a section of the nerve or nerves going to the sensitive part of the skin.

There are cases of epilepsy which are not referable to a peripheral irritation; in these cases diligent inquiry should be made to ascertain whether they are not connected with irritating causes existing in some of the viscera, such as intestinal worms, urinary calculi, &c., when the treatment should be directed to the removal of such causes, or of the diseased conditions that may have led to them. There are also cases of idiopathic epilepsy not connected with any apparent cause. I think, from my short experience, that the remedies most powerful are *belladonna;* and after it, although much less powerful, *oxyde of zinc.* As regards *oxyde of zinc,* Dr. Odier, of Geneva, has published a book, narrating a great many cures by that remedy. The physicians in France, however, have not found so much good from the use of this remedy. There is this difference between the treatment used by Dr. Odier and the physicians of France and this country. Dr. Odier gives a far larger quantity than the latter venture to give. To be of any decided use, it requires to be given in much larger doses than are ordinarily administered. There are other means, of an external nature, which are very powerful in epilepsy; but it is to be regretted that they are too often left to be practised by empirics. These consist of cauterization of the nape of the neck by the hot iron, and sometimes the cauterization requires to be continued all along the spine. This remedy is specially of use in cases where there is laryngismus and spasms of the face and neck. Dr. Marshall Hall has greatly overrated the significance of laryngismus in epilepsy. The truth is, that it contributes to produce asphyxia, and hence tonic instead of clonic convulsions. —*Dublin Hosp. Gazette, Aug* 1, 1859, *p.* 234.

14.—*On Hysteria and Chorea.*—By Dr. **Brown-Sequard.**—Hysteria, chorea, and similar affections, may arise from external irritation; all of them may be due to disturbance in the nervous centre, in consequence of irritation of some part of the periphery. In hysteria—a disease which exists in man as well as in woman—we find several very interesting cases of this nature. A case is recorded of hysteria in a male, in which the attack was produced by a slight pressure on a tumor in the lobe of the ear; the tumor was originally caused by a wound, and the extirpation of the tumor completely removed all traces of the disease. I knew also of a case of a Parisian lady, in whom the pressure of one particular finger was sufficient to induce the hysterical paroxysm. She was not, however, under my treatment, and I do not know what was the conclusion of the case; but I imagine that with proper means the case was quite curable.

Paroxysm of hysteria may be produced by pressure on the ovaries. Frequently the fit is not a complete one; but some of the symptoms—such as colouring of the face, laughing and crying, &c.—are developed at various times. Various affections of the uterus induce the disease. In one case which I witnessed, the uterus was prolapsed, and the hysterical paroxysms were of the most violent character. On the uterus being replaced, the convulsions ceased. After a short time the prolapsus recurred, and the convulsions were renewed. This occurred several times. One of the most curious cases that I know of was that of a child, who one day, on getting out of bed, was suddenly attacked by a slight convulsion, and passed into a complete state of mania. The child was put to bed, and then the convulsion and other symptoms ceased, and he was surprised to see his father and friends in such a state of anxiety about his bed. He asked what was the matter, and knew nothing of what had taken place. He was allowed to get up again, and the fit returned, but ceased immediately on his being replaced in bed. The surgeon in attendance discovered that there was, on the inferior surface of one of the great toes, a slight swelling. Having put his finger on the swelling, while the child was in bed, the fit was produced; convulsion and a maniacal condition came on. When the pressure was removed, at once the child recovered its perfect condition of mind and body. The surgeon cut out the part, thinking there was some foreign body in it, which there was not. It appeared to consist merely of indurated cuticle; but the cure was complete; the child was able to run about, and so continued for several years. He was ultimately drowned; and it was supposed, when these maniacal attacks were remembered, that he was the cause of his own death.—*Dublin Hosp. Gazette, Aug.* 1, 1859, *p.* 234.

15.—ON A CASE OF REFLEX PARAPLEGIA IN WHICH STRYCHNIA WAS SUCCESSFULLY EXHIBITED.

By Dr. William Moore, Physician to the Hospital for Diseases of Children, &c., Dublin.

[The case was that of a porter, aged thirty. States that six weeks ago he was drenched to the skin, and remained in his wet clothes for thirteen hours. This was followed by a general feeling of *malaise*, for ten days, during which time he drank whisky freely. On the morning of the third day after the wetting, he awoke complaining of pain from his armpits downward.]

I first saw this patient on the 29th of August, when, with difficulty, he was placed in the prostrate position. I examined the spinal column, and failed to detect any evidence of organic disease. He had tenderness—not actual pain—on being tapped over the lumbar region, where the muscles were lax and flabby; he had perfect use of his arms and upper extremities, and, acting from a fixed point, could thus turn his whole body. The pulse was natural, and the tongue clean. In the early stage of the attack he passed his urine in small quantity, and with some uneasiness; that has passed off, and he now micturates freely; urine of a healthy character. In the absence of galvanism with wet sponges, I desired him to be extensively "dry cupped" over the dorsal and lumbar region.

On the following day, August 30th, there was no change of any note. The patient had been very efficiently dry cupped as desired. I now prescribed one grain of strychnia, and a few drops of rectified spirits, with bread sufficient to form sixteen pills, of which he was to take one three times in the day.

On the afternoon of the 31st, the man complained of twitchings and startings in the back, and muscles of the lower extremities generally; can turn himself in bed. To continue the pills as before.

Sept. 1st. The patient got out of bed without any assistance and walked down the ward. No doubt he occasionally faltered and was unsteady, but he never lost the perpendicular, and returned to bed independent of any support. He complains very much of the startings, particularly across the lumbar region, which, he says, awoke him out of sleep, and he is afraid to cough or take a deep inspiration lest he might induce them. The pills to be repeated.

On the 2nd of September this man turned out of bed at once, walked steadily, and I may add, briskly down the ward. As he is so much improved to-day, and complains of the jerkings in his back, I omitted one of the pills.

His recovery has since been confirmed in every particular, and, on the 6th, the man was sent out of hospital.

I think this is an instance of what might be termed "peripheral or circumferential reflex paralysis," as it had its origin evidently from standing in wet and cold for so many hours, there being no lesion of the bladder, kidneys, or other viscera to account for its reflection from any of them. I find a somewhat similar case related by Dr. Graves ('Clinical Medicine,' p. 503), where a man was seized with paralysis of the lower extremities by exposing his feet to cold and wet while bailing out water in a quarry. Speaking of the prognosis of such cases, Dr. Brown-Séquard says its gravity depends in a great measure on the severity of the disease which induced it. If it does not arise from organic disease or other cause which is in itself generally fatal, it will often admit of cure, and that, perhaps, very rapidly. The cases where recovery is so decided and rapid are very rare. I find Dr. Watson mentions a case of paraplegia from exposure to cold, in which complete recovery was effected in two days.

G. M——'s case goes to show what a valuable agent we possess in strychnia in paralytic affections, where we have no reason to suspect the presence of myelitis or other organic cerebro-spinal disease. On the treatment of such cases as the above, I am again tempted to quote Dr. Brown-Séquard, who says, if strychnia be administered in the *reflex* form it may be advantageous; but in cases of paraplegia consequent upon congestion or actual inflammation of the cord, if you give strychnia you will always aggravate the affection.

In the above case, after a careful examination, I felt satisfied there was no organic lesion present, and accordingly I prescribed strychnia, having first stimulated the muscles by "dry cupping." I should have preferred the use of galvanism with wet sponges, but, for reasons which it is needless to mention, this was not convenient. The result realized my most sanguine expectations, marked recovery having taken place on the second day. I may add that the "twitchings" were exclusively confined to the paralysed muscles; those of the upper extremities seemed proof against the therapeutical action of the strychnia.—*Lancet, Sept.* 17, 1859, *p.* 282.

16.—*Croton Oil as a Counter-irritant in Hydrocephalus.* By Dr. JOHN WATSON, Southampton.—[The author has on several occasions witnessed the successful application of croton oil to the scalp after the setting in of the most formidable symptoms of hydrocephalus. A strumous-looking child, aged two years, on the cure of an eczematous eruption of the scalp, with copious discharge, had an attack of acute hydrocephalus: the symptoms soon became very severe, the child being semi-comatose, and the pulse slow and irregular.]

Thinking it possible for his present attack to be connected with the previous condition of the scalp, as a means of best imitating the eczematous eruption, the croton oil suggested itself. With the sanction of the gentleman who was attending with me, I directed the croton oil liniment (croton oil, half a drachm; turpentine liniment, half an ounce) to be rubbed over the entire head every four hours till a plentiful crop of pustules should make their appearance; after which we soon had an amelioration of all the symptoms, and he gradually became convalescent, though he was unable to speak for several days, and could not stand alone for a considerable period.

It is now several years since the occurrence of this case, which made a deep impression upon me; for I did not remember to have seen a recovery under such unfavourable circumstances. In the same stage of the disease, whatever may have been its assumed cause, I have since adopted the same course, and as far as individual experience goes, bear testimony to its efficiency. It is not so objectionable to the little patient's friends as a blister, and is at the same time more manageable; and, from the extent of surface to which it is applied, decidedly more powerfully revulsive.—*British Med. Journal, July* 9, 1859, *p.* 540.

17.—ON THE TREATMENT OF TETANUS BY WOURALI POISON.

By Dr. GEORGE HARLEY, University College.

In "The Lancet" of Sept. 17th, it is stated that—"M. Vella, of Turin, arguing from the fact shown by M. Bernard, in 1850, that the woorara poison is a direct sedative of the motor nerves, undertook a series of experiments which clearly showed the antagonism between strychnine and woorara. Being appointed to the French Military Hospital, at Turin, during the late campaign, and seeing several cases of tetanus which had resisted opiates, ether, &c., M. Vella resolved to try woorara. The first trials were made upon two patients who had been suffering from tetanus for four and five days respectively, in consequence of gun-shot wounds. They were both in a semi-asphyxiated and desperate state. The woorara produced a general relaxation of the muscular system, whereupon the patients felt much relief: but they both died. The same treatment was, however, employed upon a third patient, who recovered. He was a sergeant, thirty-five years old, tetanic from a gun-shot wound of the foot. Two grains of woorara were dissolved in nine drachms of water, and compresses moistened with the solution were applied to the wound; the strength being gradually increased to fifteen grains in fourteen drachms of water. For the first four days the compresses were renewed every third hour; afterward every fifth hour, up to the twelfth day, when the changes were reduced to three and two in the twenty-four hours. In twenty-two days the patient could leave his bed, and returned to France thirty-six days after the first application of the woorara."

You may, perhaps, remember that in 1856 I pointed out, in the pages of your journal, the antagonistic action of wourali and strychnine—citing three experiments to show that these two substances have the power of reciprocally neutralizing the effects of each other, according as the one or the other poison is in excess. The conclusion I then drew from my experiments was, that wourali might be used as an antidote for strychnine. Since 1856 I have frequently repeated these experiments, and on several occasions have succeeded, by means of wourali, in saving the lives of animals to which I had administered strychnine in poisonous doses.

Two years ago, through the kindness of Professor Varnell, of the Royal Veterinary College, I had the opportunity of trying the effects of wourali on a horse laboring under a very severe attack of tetanus. Although I did not succeed in saving the life of this animal, I nevertheless saw enough to convince me of the value of the remedy. Indeed, I was so convinced of its beneficial effects that I would have tried it on a boy laboring under traumatic tetanus, whom I shortly afterward saw along with Dr. Madge, had the disease not yielded to other remedies.

Seeing the success that has attended the administration of wourali poison by M. Vella, and the results of my own experiments, I feel anxious that this substance should receive a fair trial at the hands of the profession. No doubt wourali is a dangerous poison, but in hands habituated to its use I believe it is not more to be feared than opium or any of the stronger drugs.—*Lancet, Oct.* 1, 1859, *p.* 345.

18.—ON COLD AFFUSION IN NARCOTIC POISONING.

By Dr. Reeves Jackson.

[Cold affusion is a convenient and most effectual remedy in cases of narcotic poisoning, but is not so highly appreciated by the profession as it should be. Often in these cases narcotics cannot be swallowed, the stomach-pump is not at hand, and galvanism, although a remedy of undoubted power, usually cannot be resorted to from want of the necessary apparatus. Three cases are then related, exemplifying the efficacy of this mode of treatment.]

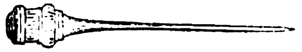
The first was a child eight months old, to which a large dose of Godfrey's cordial had been given five hours before the author saw it. Intense stupor and rapid sinking were present, and the case seemed hopeless. The head being turned downwards, a steady stream of cold water was poured from a coffee-pot over the occiput. When two or three gallons had thus been poured, the child made a long, gasping inspiration, and opened its eyes. They were soon closed again, but after the affusion had been continued a while longer, the breathing became more distinct, and the child uttered a feeble cry. Suspension of the affusion was attended with complete reproduction of the sopor, which, however, soon yielded on its resumption, and after a while the child having been got to cry lustily, vomiting was produced by means of an emetic and tickling the fauces. In two or three days the child had regained its usual health. 2. A lad, aged 19, suffering from facial neuralgia, drank a large tablespoonful of laudanum. The author was called to him seven hours after, and found him under the full poisonous effects of opium; the surface cold and clammy; the breathing irregular, slow, and stertorous; the respirations eight in the minute; the pulse full, slow and very irregular: the pupils very contracted and insensible to light, the countenance calm and pale. Various means of arousing and exciting him were tried in vain, when cold water was poured upon the head from a large pitcher, held at a height of about eighteen inches. The effect was almost magical in arousing his sensibility; and, after a while, violent vomiting ensued, all symptoms of drowsiness disappearing afterwards, under the use of a cup of strong coffee. 3. This was an example of poisoning by belladonna, occurring in a lady, to whom it had been administered in an enema for the relief of neuralgia of the rectum. She was found by the author completely insensible, with a swollen, flushed face, slow, unstertorous breathing, and a small, hard (130) pulse. Various means were employed to arouse her without any effect. A large enema of thin gruel was first administered, in order to clear out any of the poison that might remain in the bowel; and a steady stream of water was then poured upon the back of the head and neck. In about five minutes she made an attempt to articulate; and the use of the affusion was suspended, as the patient was cold. Placed in bed, and lightly covered, in ten or fifteen minutes her face became flushed, and she again fell into a deep sleep. The cold affusion was reapplied, and she soon regained consciousness. Although she continued drowsy for some hours, she recovered without the use of any other remedy, her vision remaining dim and confused for about three weeks.—*Med. Times and Gazette, Oct.* 8, 1859, *p.* 367.

19.—ON THE MODE OF EMPLOYING THE HYPODERMIC TREATMENT.

By Charles Hunter, Esq., late House-Surgeon to St. George's Hospital.

The Syringe for Injection.—The little instrument I use is made by Messrs. Whicker and Blaise, it is of the same make (but a little larger as regards the barrel) as their original *caustic syringe.* The barrel is of glass, with silver fittings, and contains a piston which works by a screw-rod, each half-turn of which expels half-a-minim, as a fine drop from the end of the pipe.

Two pipes belong to each syringe, the one larger and stronger than the other; the one here figured A is drawn the exact size of the smaller pipe, which will be found the best for general use; it screws on and off the barrel at pleasure, and is made of silver, with a hardened gold point. This point is sharp like a needle, and perforated on one side (as shown in the enlarged view, Fig. B) by the oblique opening through which the drops of the narcotic or other solution are expelled.



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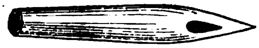

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No Incision is required with lancet, or other instrument, when this syringe is used, for the point of the pipe being very sharp and fine, is readily passed, with proper precaution, beneath the skin; no blood is shed, and the operation is no more than the prick of a needle.

The Employment of the Syringe.—Having charged the syringe with the narcotic fluid, hold it in the right hand at the junction of the barrel with the pipe, and with the left hand take up, between the finger and thumb, a fold of the skin of the patient, so as to make tense the part beyond your thumb, then the right hand being gently steadied, but not heavily pressed on the patient, let the point of the syringe, which is held at a right angle to the skin, touch the part which is tense, and, with a *quick but steady movement,* be passed through it; the point being well *through the skin,* the direction of the pipe may be altered so that it may run along in the loose cellular tissue beneath; * all this is the work of a moment; the pre-arranged number of drops are then introduced by so many turns of the piston, the pipe is then withdrawn, a finger making slight pressure as near as possible on the punctured spot, the object being both to steady the skin and prevent any drop of liquid escaping; and lastly, a narrow strip of plaster, cut beforehand and warmed, is placed on the spot.

The strip of plaster is generally a precautionary measure, but it becomes a necessity when the quantity injected is large, say twenty minims; but it is always useful to prevent the spot from being chafed. A broad piece of plaster is worse than none at all, it presses on the "little lump" which is caused for a few minutes by the presence of the injected fluid beneath the skin, and not at all perhaps on the punctured spot, and so it does more to press the fluid out than keep it in (I have seen a first injection in a case of delirium tremens fail for this very reason); *but a narrow strip just covers the punctured spot.*

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* In the Majority of cases the plan above described is best, especially with thin people; if, however, the patient is very fat, it is better to perforate vertically a portion of skin and subjacent fat, pinched up, and so made tense between the finger and thumb.

These directions may appear unnecessary, but the operation may fail, as just shown, for want of attention to these little points. If the introduction of the syringe be attempted, the skin of the patient being loose, or the syringe held at the further end, and consequently unsteadily, the patient may by these means be put to a great deal of pain, and the pipe of the syringe may be bent or broken from the socket; but when it is introduced with a quick steady movement, the skin being tense, the patient does frequently not even know when the point is introduced.

The Tissue to Inject.—The tissue injected is the cellular or areolar tissue of the body; it may not matter *much* whether the cellulo-adipose tissue, the panniculus adiposes, or the reticular tissue beneath it (not containing fat) be injected, but the latter is to be preferred; it is the looser of the two, fluid injected into it meets with no obstruction, and cannot easily escape from it, but if injected into the skin itself, as some think it is, or the conjoined cellulo-adipose tissue, it is apt to cause pain, it enters less readily, and is more apt to escape; nor does it seem to act quite so rapidly as when injected into the loose cellular tissue from which most probably absorption is the more rapid.

The Part of the Body to Inject.—When the object is to quiet the brain, or to produce a general effect, is it material whether the fluid be injected into the cellular tissue of the body or of an extremity? *No;* the non-necessity of localisation is the basis of this plan of treatment, and is the reason of its applicability in cerebro-spinal affections and general diseases. I need only refer to the various cases detailed in corroboration of this. The site which I, however, most commonly inject, is *the inner part of the arm.* The skin is here thin, easily made tense, and easily perforated; the cellular tissue beneath is loose, and readily receives the fluid; there are perhaps more veins here than in some other parts, but they are easily avoided.

The Quantity of Fluid to Inject.—It is as well to have the fluid of that strength that three or four turns of the piston shall be an ordinary injecting dose. Two or three turns can be made in a moment of time, and it is no small relief or surprise to the patient, who has been expecting, perhaps dreading, an operation, to find all over *in less than half-a-minute.*

The Dose.—Too much caution cannot be employed with regard to the *amount of the narcotic* injected. Two half turns, if your solution is strong, may double the dose, and the life of the patient, for want of due care, be placed in jeopardy; I would, therefore, urge attention to these points:—

1. Be certain of the exact strength of the fluid employed, and the exact value of each turn of the piston.
2. Concerning first injections, never use more than half the ordinary stomachic dose for males, nor more than a third for females.
3. Should a second injection be necessary, let it not be used too soon; nor in a full dose when the patient is partially under the influence of the narcotic.

These points are of practical importance, a *certain degree* of narcotism has to be reached for the benefit to accrue, and by the injection it can be reached in many cases by a very small quantity of the narcotic, because of the rapidity with which the effect is produced; what we have to avoid is *too great* an effect; what we try to produce is a *certain effect* with as *small a quantity* as possible. This leads me to a remark that men *bear narcotics much better than women.*

I was not aware to what extent this was the case until I had employed this treatment some little while; but I now think it may be looked on as a rule that men in general will bear with no ill effects, but be benefitted by, injected doses of narcotics, which doses would very strongly, if not seriously affect women; in fact, *this treatment is a test of the exact amount* of a narcotic necessary to produce a desired effect, when taken by direct means into the general circulation. For instance, you introduce beneath the skin the one-eighth of a grain of morphia, the effect which follows is the whole effect of the whole one-eighth; but you cannot be certain that the effect which follows the administration of one-eighth of a grain, firstly, by the skin; secondly, by the stomach; or, thirdly, by the rectum, is the effect of the whole one-eighth; but it is the whole effect of the quantity absorbed.

As by this method we get the *whole effect of the known quantity introduced*, which we are not sure of getting by the other modes, we have now a method as accurate as that of venous injection (without its dangers) for testing the precise effect of little-known medicines on animals, and the exact doses and effects of well-known medicines on man, of seeing the difference which the sex requires in the dose, and of ascertaining the minimum amount required to produce the desired effect.

It is impossible to say "what amount is to be injected" without knowing the particulars of the case, as well as the sex and age; but taking the acetate of morphia for an example, I think that first injections for adult females should vary from the one-eighth to a quarter or one-third of a grain; for adult males, from the one-sixth to half or three-quarters of a grain.

First injections should be small rather than large, and are good indicators of the amount necessary, should repetition be required. It is true that I have seen used and employed myself much larger quantities than those I have mentioned, for first injections; but the cases have been exceptional, and under close observation.

In the preceding papers on this subject I have shown the advantages of this mode of treatment over the endermic, enepidermic, and stomachic methods, which, requiring longer to act, are less certain and apt to fail completely. Before, however, bringing this paper to a close, I would allude to two other modes of medical administration, viz., by the tongue and by the rectum.

1. *Medicines administered by the Tongue.*—Dr. Wardrop has shown that there is a remarkable difference in point of time when medicines are absorbed from the stomach or from the mouth, absorption being most rapid from the latter, and the effect is more regular and more equable. Nor is it difficult to say why,—the medicine absorbed from the mouth is taken directly into the general circulation, but when absorbed from the stomach it has *en route* to pass through the portal system; absorbed from the tongue, the effect is more regular, because the medicine is more certainly absorbed *en masse*.

There is, then, much similarity between the hypodermic and the lingual modes. Rapidity of absorption is the great point in the *modus operandi* of each; and with regard to the effect they both have the advantages of rapidity, greater efficacy, regularity, and equability. Can the one method, then, replace the other? Are they applicable for the same cases and medicines? No; they both have their advantages. Dr. Wardrop's plan is best for the administration of *tasteless* medicines, for calomel, *et hoc genus omne*, but it cannot be used for those medicines which are nauseating and bitter, not, in fact, for narcotics generally, not for cases of delirium, patients refusing medicine, &c., which are the cases where the other plan is most desirable.

2. *Medicines administered by the Rectum.*—This mode of medicinal administration is of great value, and useful as a means both for local and general treatment; there can be no doubt that this method has advantages which the stomachic has not, viz,, of greater rapidity of action and greater effect, but the effect is *uncertain;* this uncertainty of action is *not* dependent on the mode of introduction, especially if the medicine be used in the liquid form, and employment be made of the gradual syringe invented by Dr, Spencer Wells to regulate the exact amount introduced; but is due to the want of *regularity of complete absorption*, which cannot be done away with. The rectal method is the more advantageous where the object is to administer the smaller doses of narcotics for affections of the intestinal canal, the rectum, and the parts adjacent supplied by the great sympathetic, but *most especially* for the speedy introduction of stimuli, and of nutriment in urgent cases, for liquids introduced by this plan have the advantage of being conveyed *simul-*

taneously into both the portal and systemic circulation: the hypodermic, on the other hand, is the more applicable for those cases where the part, requiring the narcotic, is supplied by the systemic circulation, and is under the influence of the cerebro-spinal nervous system.—*Med. Times and Gazette, Oct.* 8, 1859, *p.* 354.

DISEASES OF THE ORGANS OF CIRCULATION.

20.—ON SOME POINTS IN THE DIAGNOSIS AND TREATMENT OF HEART-DISEASE.

By Dr. G. H. Barlow, Physician to Guy's Hospital.

[The principal object of this paper is to show that a considerable proportion of disease of the organs of circulation in general have a tendency to range themselves into one or other of two large classes, according to which classification our treatment should be in great measure regulated. Dr. Barlow says:]

Now I would premise that, though much labour and ingenuity have been employed upon the semeiology of cardiac disease, and more particularly upon the interpretation of the signs furnished by auscultation, these signs have been too little connected with the general symptoms, an omission which has, I believe, somewhat impaired their value in a diagnostic, but still more in a therapeutic point of view. Now, in regard to the arterial pulsations, which we should naturally regard as next in importance to those of the heart itself, I cannot discover that much attention is paid to enabling the reader to connect the different varieties in the pulse at the wrist with different forms of heart disease, and the same may be said of the action of the lungs, the liver, and the kidneys. For instance, we are informed, in general terms, that the liver may be gorged and the urine scanty and turbid in heart-disease, but I think we may look a long while before we shall find it suggested that jaundice, from hepatic congestion or scanty and turbid urine, belongs more to one form of such disease than another.

Let us begin with the aortic sigmoids. When there is exudation of lymph upon the surface of these valves, or between the layers of endocardium of which they consist, there will be more or less narrowing of the orifice through which the blood passes into the aorta, and therefore there will be heard, upon auscultation, a bellows murmur accompanying the first sound, and traceable upward along the course of the aorta. It may happen, however, and generally does so, when the effusion is between the layers of the valves, that, by the contraction of the effused lymph, the valves become puckered, and consequently, inadequate to the closing of the orifice, and in this case there will be a double or see-saw murmur heard in the position thus pointed out. Now, lesion of these valves has a very characteristic influence upon the pulse at the wrist, which, in cases of obstruction, is sharp and compressible, rather defective in volume, but still more so in persistence, when compared with the impulse of the heart. When, however, there is regurgitation through these valves, whether from the above cause or from ulceration or laceration, it is large in volume and very compressible; it is, in fact, of the water-humour or splashing character. The rhythm of the heart, under these circumstances, is not disturbed, and the pulse neither intermitting nor irregular.

If we turn our attention to other organs, we shall find that in the earlier stages of disease of the aortic valves the circulation in the lungs is but little, if at all, disturbed, and, therefore, that, except under exertion, the respiration is easy; the liver is not gorged, and there is fair performance of its functions; and as the portal circulation, as well as the passage of the blood through the cava, is unobstructed, there is a free secretion of urine.

Let us now consider the symptoms of disease of the mitral valves. When such exists, whether it be obstructive or regurgitative, the auscultatory sign is generally supposed to be a systolic bellows-murmur, most distinct toward the axilla, though, in a case of obstruction, it perhaps extends more toward the right side than in that of regurgitation; the murmur is, moreover, to be distinguished from that arising from aortic by its not following the course of the aorta. I must, however, here express my belief that we ought not to regard the presence or absence of this murmur as deciding the question of the presence or absence of disease of the mitral valves, since its *immediate* connection with such disease is, to say the least, doubtful. In the case either of obstructive, or regurgitative disease there will be the symptoms of obstruction to the pulmonic circulation—dyspnœa, livor, and other signs of venous congestion, sometimes hæmoptysis—and a very small and often intermittent pulse. The right side of the heart will become gorged, and therefore the liver, the consequence of which will be a diminished secretion of bile, the effects of which will generally show themselves in the colour of the urine and often in that of the skin. From the obstruction to the portal circulation the urine becomes scanty, so much so, that on cooling it generally throws down some of its solid ingredients, especially the urates, and, besides its being tinged with bile, there is often a deposit of purpurine.

Hence, upon comparing these two forms of disease, we see in almost every symptom a remarkable contrast. The pulse, in disease of the aortic valves, is large, splashing, and regular; in mitral disease it is small and often intermittent. In aortic disease there is little or no dyspnœa, in that of the mitral valves is urgent. In the former disease the complexion is for a long time natural, in the latter it is, almost from the first, livid, and often icteric. In the former the urine is abundant and clear, in the latter it is scanty, high-coloured, and turbid. And, not to carry the contrast further, let us turn to the mode of death. When death occurs *immediately* from disease of the aortic valves, it is by syncope, when it is from disease of the mitral valve it is by apnœa.

I need hardly say that both the one form and the other of these diseases may be closely simulated by others, a remark which applies more particularly to disease of the mitral valve.

Thus, a dilated ventricle in an anæmic subject, with lax arteries, will present most, if not all, of the symptoms of disease of the aortic valves, and the same may be said of disease of the ascending aorta. Whereas I do not hesitate to say that other forms of pulmonic obstruction, as, for instance, long-continued capillary bronchitis, may present all the symptoms of diseased mitral valve, murmur inclusive. I have, however, adduced these forms of disease as typical of the two great classes into which most diseases of the heart—I had almost said of the circulatory system—seem to arrange themselves. I consider that the great difference between them consists in the obstruction, (for, after all, regurgitation is practically the same as obstruction) originating on one side or the other of that great barrier, the mitral valve. The tendency of disease of the heart is to propagate itself backward. that is to say, in the opposite direction to the current of the circulation. Now this tendency is, in the case of aortic disease, opposed, and often for a long time successfully opposed, by the perfect closure of the mitral valves, so that it is not till after the continued stress before the left ventricle has, by impairing its strength, led to dilatation, and rendered it unable to empty itself, and consequently to receive so readily the blood from the other side of the mitral valves, that the obstruction begins to propagate itself to the lungs, the right heart, lungs, &c. Whereas the closure of the tricuspid valve being physiologically imperfect, disease tergal to the mitral valve is rapidly propagated—the lungs, right heart. and liver becoming gorged in rapid succession—the effect being much the same whether the disease consists originally in obstruction or regurgitation at the mitral valves, or in impediment to the pulmonic circulation from severe bronchial obstruction.

I have already suggested that the great danger arising from disease of the aortic valves (in the first instance) is sudden death from syncope, an event which has occured not very unfrequently, whereas the next thing to be dreaded is gradual failure of the power of the left ventricle (generally in the form of dilatation, without compensating hypertrophy,) the consequence of which is that the patient is brought practically to the predicament of a person suffering from disease of the mitral valves, whereas, in the case of the latter disease, we have a rapid supervention of dangerous engorgement of vital organs; hence it follows that, barring the risk of sudden syncope, the danger is much more remote in disease of the aortic than in that of the mitral valves.

Again, as regards the principles of treatment, I may remark that, both for the purpose of obviating the danger of sudden syncope and counteracting the tendency to dilatation, a tonic and often stimulating plan of treatment is indicated, the object being to maintain the tone of the system, and, as much as possible, to prevent the dilatation and promote some amount of compensating atrophy. We must, at the same time, try to relieve the circulation by keeping up a free action of the excretory organs. It would occupy too much space were I to enter minutely into details, but I may mention that I have frequently found the sulphate of zinc useful as a tonic, in such cases. There is also a form of medicine which I have for many years been in the habit of prescribing, somewhat empirically, consisting of a combination of hyoscyamus, nitric æther and decoction of senega; and I may add, that where there is palpitation from diseased aortic valves, without any other serious complication, the benefit from this medicine is invariably great; and I may add that in other forms of cardiac, and I might also add of pulmonic disease, the more the pulse and other symptoms approach to those of disease of the aortic valves, the more confidence have I in the senega.

In disease of the mitral valves, on the other hand, although the pulse is much more feeble, I do not find the same advantage from stimulants as in disease of the aortic valves; by their use we often increase the palpitation without strengthening the pulse. In such cases we must look mainly to relieving the circulation through the portal system, and nothing will be found to give so much relief (notwithstanding the feebleness of the pulse) as free catharsis, after which we may often be enabled to establish a free action of the kidneys. Relieving the pulmonic circulation by free expectoration, where it can be effected, is also a valuable adjuvant; and here I would add, as in the former case, that the same rules of treatment apply to all affections of the circulatory system, in proportion as their symptoms approach to those of mitral disease. And I here believe likewise, that we might extend this remark to the opposite forms of subacute or asthenic bronchitis, which we not uncommonly meet with—I mean those forms of the disease in which there is in the one dyspnœa and wheezing owing to the walls of the tube being thickened and their calibre thereby diminished, and in the other excessive puriform secretion. In the former of these there is obstruction to the functions of the bronchial tubes by change in their structure, and the chief auscultatory sign is wheezing; in the latter, where the chief disturbance is from the great muco-puriform secretion, there are large rattles. In the former there is great lividity; in the latter there is, indeed, a slight livor of the lips, otherwise there is pallor. In the former there is a bloated countenance, and sometimes general œdema; in the latter there is emaciation. In the former the patient dies suffocated; in the latter he sinks exhausted.

I was once led, by the observation of the condition of the left ventricle in poisoning by digitalis, to try the effect of that drug in disease of the aortic valves, where death generally takes place with an opposite state of the ventricles. The result of my observations, however, was that my theory was perfectly wrong, whereupon I had recourse to it in what I consider the opposite form of disease, namely, that of the mitral, and in several cases, although the pulse was intermittent, with marked benefit. It is in such cases too, provided the urine is not albuminous, that a combination of a grain each of calomel, digitalis, and squill, so often successfully employed by my experienced colleague, Dr. Addison, will be found eminently useful.—*Guy's Hospital Reports*, 1859, *p.* 342.

DISEASES OF THE ORGANS OF RESPIRATION.

21.—ON ASTHENIC PNEUMONIA.

By Dr. J. Russell, Physician to the Birmingham General Hospital, and Lecturer on Pathology at Sydenham College.

[The two cases which form the text to this lecture are instances of pneumonia of an asthenic or typhoid type. After briefly mentioning them, the author makes the following remarks upon the disease.]

Perhaps no malady is attended with less uncertainty in its diagnosis than a genuine case of simple sthenic pneumonia: on the contrary, there are few more insidious than the same disease in its asthenic form. Where asthenic pneumonia exists as a secondary affection, it is not unfrequently so entirely destitute of special symptoms that its presence may be entirely overlooked; and were it not for our knowledge of the circumstances under which its occurrence mày be expected, our attention might never be directed tò the chest. In each of the cases at present under consideration, there was no reason whatever to suspect the existence of the disease prior to the physical examination of the lungs. In feeble or unhealthy subjects, again, pneumonia, although it be the sole or primary affection, is often deprived of some of its most characteristic signs.

When pneumonia occurs as a secondary affection, it is spoken of as intercurrent. It is a more or less frequent complication of many diseases, which are produced by the presence of a poison in the blood; thus it is met with in the course of typhus fever, in erysipelas and surgical fever, in scarlatina, measles, and influenza, in remittent and yellow fever, and, as we have also found, in acute rheumatism. It appears not improbable that it may sometimes exist as the direct consequence of a morbid poison where no other indications are afforded of the presence of that poison in the blood, just as in every epidemic of scarlet fever we find isolated cases in which the characteristic affection of the throat is the only symptom of the disease which is present. Thus, whilst erysipelas is prevailing in the wards of a hospital, and the subjects of wounds or of operations are liable to be seized with that malady, instances every now and then occur of patients similarly circumstanced suffering from asthenic pneumonia, which we have every reason to refer to the poison of erysipelas, although the usual cutaneous inflammation be absent.

But the disease is also met with where we have no reason to suspect the existence of any specific poison in the blood; it may occur after exposure to cold, or to the operation of any ordinary cause of simple inflammation, in enfeebled individuals who have been exhausted by overwork, by anxiety, or by other depressing agencies, or who have lowered their health by intemperance, or by unhealthy habits of life. Such appear to be instances of ordinary inflammation, occurring in a debilitated constitution, which imparts to them their asthenic type. Yet, in many of these cases the early development of delirium, or of diarrhœa, or, as I have seen more than once, of jaundice, together with a manifest and alarming tendency to sinking, affords reason for the suspicion that some poison has either preëxisted in the blood, or has been produced by the exciting cause of the disease; and this suspicion is strengthened by the observation, that in such cases the urgency of the symptoms bears no proportion to the extent of the inflammation; thus, I have seen these serious symptoms present, and hasten rapidly to a fatal termination, where the inflammation has been limited to one lung, and absent when it involved the entire lower lobe of both lungs.

Now, where pneumonia is present under any of the circumstances I have indicated, the signs of its existence may be obscure; the energies of the constitution may be so deeply depressed by the injurious influences under which they have been laboring, that the power of reaction is prostrated, and the indications of the disease are proportionally deficient. It is surprising what an amount of inflammation may be inflicted upon the body under certain circumstances, without its appearing sensible of its presence. This want of reaction will generally bear a proportion to the depressing nature of the poison, or of any other cause which has been previously in operation; thus, the poison of measles does not generally exert any very lowering influence upon the system, and the signs of pneuomonia, when it occurs, are sufficiently indicative. On the other hand, with the extreme prostration which attends the presence of the typhus poison, the greatest vigilance is necessary to render ourselves aware of the occurrence of the inflammation. Such differences arise, not from any diversity in the inflammation itself, but in the circumstances under which it takes place.

Nor is pneumonia singular in this respect; other inflammations, occurring under the like conditions, may be equally insidious. In puerperal fever, *e.g.*, we not unfrequently meet with instances in which inflammation of other solid organs, of the serous membranes, or of the joints, even runs on to suppuration, without its presence having been ascertained until it has been revealed by a *post mortem* examination.

I will now briefly compare the ordinary symptoms of pneumonia with those which are presented by the form now under our consideration. Even in the sthenic form, considerable allowance must be made for variations of symptoms in particular cases; but where the pneumonia has assumed an asthenic character, deviations from the normal type are much wider and more numerous.

The Inflammatory Fever. In cases where a specific poison exists in the blood, the inflammatory fever is, of course, merged in the peculiar fever which attends the operation of such poison, and is of a low type. Where no specific poison is present, the inflammation may set in with a rigor, and sometimes with a sharp feverish reaction; but the symptoms speedily assume an asthenic character, and may be attended, even at an early stage, by delirium and other serious symptoms; and the patient may manifest a marked tendency to sink. In all these particulars, however, there is great variety. In a case of uncomplicated pneumonia, which I saw the other day, the pulse sank below one hundred after the third day. In another similar case, in which *both* lungs were involved, the intellect was clear, and the pulse was below one hundred after the third day; whilst in two others, in each of which only a single lung was affected, the pulse was rapid; there was low delirium, and in one, retching, diarrhœa, and floccitation; and death occurred in both, at an early period, with every symptom of exhaustion.

Pain in the Side may be absent, even in ordinary pneumonia; it is more frequently so in the asthenic form; or, if present at first, may speedily sink into a very subordinate position. In some cases, the pain occupies a situation more or less remote from the seat of disease. Such deviation is by no means peculiar to the asthenic variety; but it becomes of greater importance in that form, on account of the absence of other indications, and the consequent danger of misleading the observer. In one case the pain was seated in the upper and anterior region of the chest, the lower lobe being affected; in another it occupied the region of the left hip; and so free from urgency were the other symptoms, that the patient had actually weathered the first stage when I saw him, and had come down stairs. His hurried breathing, with his cough, directed my attention to his chest, and I found the entire lower lobe of the lung consolidated. Pain in the flank, or iliac region, is sometimes an attendant of inflammation, probably of the base of the lung. In one remarkable instance, I believe the pain was seated in the pillars of the diaphragm, as it was chiefly occasioned by hiccough, which followed the presence of food in the stomach, and was so intense on any attempt to move the trunk, that it was next to impossible to examine the posterior region of the chest.

Cough and Expectoration are subject to much variation; the cough is not unfrequently slight. In one of our hospital patients we should hardly have paid attention to it, although there was no dulness of sensibility, and his intellect was perfectly acute. On the other hand, it may be remarkably violent; I have seen it almost spasmodic. It is important to know that expectoration is often scanty, and may be entirely absent. It was trifling in quantity in both of our patients. It was entirely absent in three cases which I call to mind, although in one of them, which was examined after death, the inflammation had effected both lungs. When present, the sputum is often composed of semitransparent mucus, of considerable adhesiveness; but without the peculiar gelatinous tenacity and the rusty color of the expectorated matters in the sthenic disease.

The Respiration presents, perhaps, the most unvarying physiological symptom. There may be little or no complaint of oppression; but, excepting in instances of great prostration, the breathing is much increased in frequency. This symptom will often be very evident by comparison with the pulse; whilst the latter may vary in the number of its beats day by day, the breathing continues steadily frequent, thirty-six, forty, or more respirations being performed in a minute.

The reason is evident. The amount of space available for the purposes of breathing being lessened, compensation is made by increase in the frequency with which the air is transmitted through the lungs. In asthenic pleurisy, also, this symptom is present. Cases of this disease, which sometimes present themselves in the out patient's room, in which the patient makes no complaint of the chest, often first arouse our suspicion by the unhealthy frequency of the breathing. I need hardly refer you, however, to one of our cases as obscuring even this system. In the girl in ward sixteen, frequent and irregular breathing was sufficiently accounted for by rheumatic affection of the intercostal muscles; and you may have noticed her distress on more than one occasion, when, on awaking from sleep, her irritable muscles were suddenly required to assume a state of greater activity.

In the obscurity which attends the general symptoms of asthenic pneumonia, our great dependence must be placed upon our knowledge of the circumstances under which the disease is most liable to occur; and it very fortunately happens that when our attention has been directed to the state of the chest, the clear indications which are afforded by the *physical signs* at once deprive the case of all uncertainty. One physical sign, however, usually present in the acute form of the disease, I have found absent in some cases. I allude to crepitation; bronchial breathing being the first abnormal sound I have discovered. It is possible that, from the obscurity of the general symptoms, the first stage of the disease may have passed before the examination has been made; but this explanation certainly does not apply to every case, and the scanty amount of the expectoration sometimes observed, affords corroborative evidence in favour of the statement I have just made. Sometimes the signs of consolidation are preceded by crepitation in the small bronchial tubes, the pneumonia occurring as a sequel of capillary bronchitis. From the absence of crepitation, especially when coinciding with deficient expectoration, it is possible to mistake the case for one of pleurisy. The diagnosis, however, is easily effected; for, besides that vocal vibrations are present, with abnormal intensity, on the affected side, the peculiar limit of the dulness forms a distinctive sign of great certainty, when the inflammation is seated on the lower lobe. The lower lobe of the lung occupies the entire posterior region of each side of the chest, excepting only the supraspinous fossa. The lateral and anterior regions correspond with the upper lobe on the left, with the upper and middle lobes on the right; consequently, in the cases I am now speaking of, while the chest is dull behind, even to the spine of the scapula, in the lateral space and in front—as was the case with both our patients—it is clear; the line of demarkation approaching a vertical direction—a direction of course impossible with free fluid in the chest. The only form of pleurisy which could afford a similar sign is that in which the fluid effusion is limited by adhesions.

You may, perhaps, expect me to assign the absence of the chlorides from the urine as an additional diagnostic sign of the presence of pneumonia; but since attention has been drawn to this subject by the observations of Dr. Beale and others, the symptom has been found to be by no means peculiar to inflammation of the lungs; but to be present in other forms of acute disease.

Asthenic pneumonia, like all low forms of disease, is more liable to irregularity in the manner of its occurrence than the asthenic form. It may present itself in the upper lobes of the lung, or in the central parts. In a case the other day, the patient, who had been depressed by severe anxiety and worry, was attacked with fever, pain in the chest, and rapid breathing (from thirty to forty); but no physical signs presented themselves, excepting some prolonged expiration in the lower lobe of the lung for five or six days, when the usual signs of consolidation were manifested, preceded for one day by crepitation and rhonchus. It is probable that in this case the inflammatory action commenced in the centre of the lung.

In forming your prognosis, there are two elements for your consideration; the inflammation, and the condition of system with which it is associated. Now of these two, the latter is by far the more important. Pneumonia, though a sufficiently serious disease, has, if uncomplicated, a strong tenacity to recovery; but this tendency may be overcome by the noxious influence of poison in the blood, or by a state of constitution in which the powers of the body have been prostrated by debilitating influences. Hence your judgment will be regulated rather by these symptoms which relate to the state of the patient's general health, than by those which indicate the extent of the inflammation. The same circumstances will influence the progress of recovery. A state of cachexia will not only retard this process, but may even turn it into a very unfavourable direction. It promotes degeneration rather than repair, and hence may create a tendency to suppuration and gangrene. We had a striking illustration of the truth of what I have said in the hospital the other day, (British Medical Journal, May 7th.)

There is one awful accident which sometimes happens in this disease, of which it is very necessary that you should be aware—the occurrence of sudden death by fainting. I have twice seen this unhappy termination to the low form of pneumonia; and once with acute bronchitis occurring with miliary tubercles, and with the heart in an early stage of fatty degeneration. Two of the three patients had suffered from much anxiety, and from overwork; of the third I have no information in this particular. I have also been told by a friend, of a case of subacute pleurisy, in which death took place in the same manner. One of my patients died as she was in a semi-recumbent posture, arranging her hair. Another literally passed from sleep to death, and was found in the attitude of quiet repose.

The subject of treatment would afford matter for an entire lecture. I shall content myself with simply indicating the principles by which you must be guided. I have already stated that pneumonia, when uncomplicated, has a natural tendency to recovery. The danger, therefore, arises more from the circumstances which attend it, than from the inflammation itself; and such circumstances must receive a considerable share of attention in the treatment. Not that I would lead you to disregard the inflammation; I only desire to place the other elements of this case in their proper position. The more accurate physiological knowledge we now possess, has defined with greater precision the position held by the blood vessels in the process of nutrition, and has taught us that changes in the circulation of one organ, must be explained by alterations in the nutritive condition of the organ affected, or of the system at large. We no longer regard inflammation as an entity of itself; but as one of a series of changes in the nutritive process; and these other changes often demand a still larger share of our attention. Impure states of the blood, depraved conditions of nutrition, the presence of tubercle or of cancerous deposits, often assume greater importance in our estimation than the local inflammation they may provoke.

Now in the disease in question, the concurrent circumstances indicate either that the cause itself of the inflammation seriously depresses the nutritive powers, or that it is allied with some other agency which tends to produce the same effect; and we have to do our best to prevent the favourable tendency of the inflammation from being overborne. On this account, sedatives, especially tartar emetic, which are of much use in sthenic pneumonia, are often quite inadmissible; and in favourable cases, can only be employed in the early stage, and in moderate doses.

Blisters, on the other hand, which are of questionable utility in the early stage of the sthenic form, are of much service. A few leeches also may afford much relief to the pain. Mercury has the high sanction of Dr. Watson. I would, however, venture to suggest that it is apt to be attended with the inconvenience of producing troublesome diarrhœa; and I have seen recovery imperilled by the occurrence.

In consequence of the tendency to depression which is so frequently prominent in these cases, stimulants will very often constitute most important remedies. Their quantity must be suited to the requirements of the particular case. In a case to which I have already referred, the patient did well with only a few doses of ammonia. Here, a stronger stimulant would have been unnecessary, and therefore injurious. On the other hand, in the pneumonia of typhus, or of erysipelas, or in the uncomplicated form, where delirium appears, with a rapid pulse, and other signs of prostration, wine and brandy must be given with a free hand. Between these extremes there will be every gradation. The object of stimulants is to keep up the energy of the circulation; and their employment must be regulated by the state of that function. On the other hand, wine and spirits do not contain nitrogen, and therefore cannot be appropriated by the tissues as food; they are no doubt entirely eliminated. It will, therefore, be desirable not to load the blood with an unnecessary quantity of excremental matter, especially when so important an emunctory for carbonaceous matter as the lungs, is diseased.

With these precautions, stimulants must hold a very high place among the remedies for asthenic pneumonia, especially as it presents itself among our town populations.

Opium, which so frequently accompanies stimulants in the treatment of disease, often proves a very valuable remedy. Regard must, however, be had to the tendency of the particular case. Some morbid poisons are narcotic in their action, and tend to produce stupor. This is sometimes the case with the typhus poison, for example; when such a tendency is observed, the employment of opium, at all times dangerous, would be doubly so with the respiratory surface seriously lessened. It is when the symptoms of asthenia exist without this complication, that this medicine lends its invaluable aid.

I need not say that due attention must be paid to the state of the secretions, and particularly to the alvine evacuations; and that while you secure the necessary relief to the action of the bowels, you must avoid weakening your patient by the undue administration of purgatives, particularly those of the more powerful class. Support, by light nutritious diet, is of great importance. By the judicious administration of beef tea, in small quantities, at short intervals, you may, in mild cases, even avert the necessity for stimulants, or at least for those of the stronger kind.—*Brit. Med. Journal, July* 2, 1859, *p.* 524.

22.—*Syphilitic Pneumonia.*—We well remember hearing Dr. Stokes describe a form of pneumonia common amongst drunkards, and which he called "drunkards' pneumonia." There is also an inflammatory consolidation of the lung which owes its origin to the poison of syphilis, and hence is well worthy of the appellation of "syphilitic pneumonia." At the Royal Free Hospital, on the 22nd ult., we were shown a well-marked case of the latter, under Dr. O'Connor's care; the patient, who was admitted about the middle of July, being thirty-five years of age. His syphilitic history was clear, and was associated with a papular eruption, some of the copper-coloured spots being visible up to the present time about the back and shoulders. On his admission, the physical signs of pneumonia were present, the dulness over both lungs was very considerable and extensive, and the vocal resonance was strong and distinct all over each. The dyspnœa, therefore, was urgent, but the breathing was not so embarrassed as in ordinary pneumonia. There was also frequent cough, without expectoration, associated with much wasting, and a small and quick pulse (100). His treatment consisted of blisterings all over the chest, five-grain doses of iodide of potassium from the 23rd to the 28th of July, and four grains of mercury with chalk and conium thrice a day. On the 2nd of August, a grain of iodide of mercury, with four grains of extract of conium, three times a day, were ordered, and continued till the mouth became sore; and a quarter of a grain of muriate of morphia every night. The gums are tender now; he is taking iodide of potassium with his cough mixture, and the disease is yielding. One of his testicles was much enlarged, of a pyriform shape, and indurated, principally depending upon enlargement of the epididymis. His voice is hoarse and husky.

This is one example in some six or seven which have been admitted into this hospital with the symptoms of inflammatory chest disease, clearly the result of syphilis. A case, in many respects similar to it, is under Dr. Willshire's care at the Charing-cross Hospital, differing only to this extent, that the bronchial tubes, trachea, and faucial mucous membrane have been affected, instead of the lung tissue. The patient is a middle-aged woman, whose history is obscure, but the ulcerations and other peculiarities point to syphilis as the cause of the disease. The secretion from the tubes is copious, and occasionally hemorrhagic. She has much improved under the use of the syrup of the iodide of iron.

We have seen cases in the Royal Free Hospital, under Dr. O'Connor's care, wherein the evidences of phthisis were present, with an absence of the physical signs of the disease, the symptoms depending upon constitutional syphilis, and readily yielding to the exhibition of mercury.—*Lancet*, *Sept.* 3, 1859, *p.* 238.

23.—*Treatment of Phthisis by Carbonate of Lead.* By M. Beau. According to M. Beau, it is extremely rare that a case of phthisis is found among workers in lead. The immunity which he had observed in this respect led him to try the efficacy of some of the preparations of this mineral in arresting the progress, and in entirely eradicating the tuberculous diathesis.

He gives the preference in his trials to the carbonate of lead, which preparation being insoluble, he considers as less liable to produce an unfavorable reaction upon the stomach. He administers it in increasing doses, from 10 to 80 centigrammes (2 to 16 grs.) each day, in pill, arresting or suspending the use of it as soon as the patient appears to be sufficiently impregnated, that is, as soon as the symptoms characterising the first degree of saturnine poisoning appear, such as arthralgia and gesia, lisere, and an icteroid tint.

M. Beau reports five cases, in four of which certain symptoms, especially the cough and the expectoration, seemed to be favourably modified by the use of the carbonate of lead. He does not, however, announce a complete cure, and adds, that as an auxiliary to this medication, it is necessary to support the patient by the best possible means; by nourishing food, wine, tonics, and causing him to observe all the rules of a rational hygiene.—*American Medical Monthly*, *Aug.* 1859, *p.* 115.

24.—*On the peculiar Efficacy of Sulphate of Copper in Exciting Vomiting in the Treatment of Croup.* By Dr. Missoux.—The importance of repeated vomiting in the treatment of croup is admitted by many practitioners, but the choice of an emetic is a point which has not hitherto been fully determined. Since vomiting has been considered by some as the mechanical act which induces the detachment of the false membranes, tartar emetic has been employed for the purpose. Others have preferred ipecacuanha, the dynamic action of which is less depressing than that of tartar emetic, but there its superiority ends. The sulphate of copper, in addition to its emetic action, possesses a very remarkable property of acting locally, and this peculiarity makes it superior to tartar emetic and ipecacuanha. With the latter substances, the patients derive benefit only from the mechanical act of vomiting, and when the false membranes are expelled, others are formed. The case is quite different with sulphate of copper, for when a solution of this salt is employed, the secreting surfaces are so modified, that no more false membranes are formed, or if they are formed, they no longer present the plasticity which renders them so adherent to the adjoining parts. Dr. Missoux, after a practice of eighteen years, states that the sulphate of copper has been in his hands the most successful emetic agent in the treatment of croup. Its purifying action appears to him the more valuable, because diphtherite (croup) at its commencement is often localised in the throat, and by applying remedies early, the extension of the false membranes to the larynx may be prevented. He wonders that this topical action of the copper salt has not been hitherto observed upon plastic exudations which are visible to the eye, such as cutaneous diphtheria, and that of the vulva, the throat, and the nose, for its effects in these complaints would have induced a speedy conviction of its utility. The dose in which Dr. Missoux administers the sulphate of copper is rather larger than that prescribed by other physicians. For young children he dissolves a quarter of a gramme of the salt in 125 grammes of distilled water, and orders a teaspoonful to be given every ten minutes, until vomiting is produced. After the age of puberty, and in adults, he increases the dose to one gramme, without his having ever witnessed any poisonous effects. The more the solution is concentrated, the more frequently the doses are given, and the earlier its administration is resorted to, the more prompt and certain are the effects of the treatment. Out of thirty diphtheritic cases, Dr. Missoux lost only two. This result may surprise some readers, but he assures the profession that he has determined the existence of croup only after actually observing the presence of the false membranes in the bronchi, trachea, and larynx.—*Brit. and Foreign Med. Chir. Review, July* 1859, *p.* 245.

25.—*Croup—Enlarged Tonsils.*—M. OTTENBOURG informs the Medical Society of Paris, that observation has led him to the conclusion, that permanent enlargement of the tonsils protects against croup. Children, exposed to the infection of croup, having enlarged tonsils escaped the disease, while others without hypertrophy of these organs contracted croup. This view of the matter has led M. Ottenbourg to this practical result. He advises the parents of children not to allow their enlarged tonsils to be removed.—*Med. Times and Gazette, May* 21, 1859, *p.* 517.

26.—*On the Employment of Veratria in Acute Diseases of the Chest.*—M. Aran has called the attention of practitioners to the remarkable effects produced by the internal use of veratria in febrile diseases, and especially pneumonia. In the Sardinian Medical Gazette an article has appeared, in which Dr. Ghiglia, without any knowledge of M. Aran's researches, recommends the use of veratria in the same circumstances, except that he never employs this alkaloid alone, but associates it almost always with opium, sometimes in the form of pill, sometimes as a syrup. The dose of veratria is five millegrammes (·077 of a Troy grain) in a pill with the same quantity of opium, and the number of pills to be taken in the twenty-four hours varies from six to seven, and even twelve, according to the circumstances. In this dose, according to M. Ghiglia, vomiting rarely occurs, but nausea and the other depressing effects of veratria are present. The results obtained by M. Ghiglia in certain cases of pneumonia, bronchitis, and broncho-pneumonia have been sometimes most remarkable, but have been occasionally unfavourable, and the following are the results arrived at by this author: "1. The inflammations of the respiratory organs, when they have arrived at such a period as to produce disorganization of the parts, are not improved by the use of veratria. 2. The action of this substance is the more favourable in proportion as the disease is more recent. 3. The tolerance is very various, according to individual habits, and perhaps also according to certain peculiarities which are not yet well understood. 4. The more easily the tolerance ceases the more marked is the depression. 5. Veratria is in many respects a preferable medicine to others which are more constant in their action but less easy to take. And 6. It is perhaps prudent, in severe inflammations of the respiratory organs, to order a few bleedings before prescribing the veratria.—*Brit. and For. Med.-Chir. Review, July* 1859, *p.* 242.

DISEASES OF THE ORGANS OF DIGESTION.

27.—ON DEFECTIVE ASSIMILATION IN INFANTS—ITS PREVENTION AND TREATMENT.

By Dr. Routh.

[The greater part of the mortality of infants is due to defective assimilation—the result of want of breast-milk, and the use of injudicious food. Diseases of defective assimilation are favoured by hereditary tubercular habit, exanthemata, bad air, and want of cleanliness. After death, when diarrhœa has been present, red patches or aphthæ are found on the alimentary mucous membrane, or a reddish-coloured intensely acid mucus is found exuded from it.]

The disease seems to be gradual, passing on to entire loss of *primary* assimilation; the secondary still persisting, although inactive from want of assimilable matters to take up. Albuminous, starchy, and oily matters were not digested.

The *treatment* consists in supplying fatty acids and already artificially digested animal and occasionally vegetable substances, especially human milk. If this could not be sucked, it should be collected in a cup and given by the spoon. Dr. Routh strongly animadverted here upon the absurd dogma, that it is wrong to mix human and cow's milk. He, on the contrary, believed the plan not only safe, but the very best practice in many cases, and the only means of saving an infant's life. Simple juice of meat, and this with vegeto-animal food, he had found most useful in fulfilling these indications. The remedies were of two kinds: 1st. Those calculated to increase cell growth and development. Phosphate of soda, producing an emulsion with fats, thus allowing of their assimilation; chloride of potassium, to dissolve carbonate of lime; phosphate of lime, to enable blood to take up more carbonic acid, and thus hold in solution more carbonate of lime; (these substances severally strengthening muscular and bony structure;) lime-water, to provide lime to blood. 2nd. These last also acted as some of the remedies calculated to allay local irritation of the alimentary canal. Carminatives were useful, such as dill, but especially cinnamon-powder, to correct flatus and to check diarrhœa. Anodynes were also (however objected to generally) strongly recommended by the author. For the diarrhœa, when present, nitrate of silver and sulphate of copper were the best remedies. Wine was also found very seviceable, even if given in large quantities. These remedies, however, it must be confessed, proved in most cases of no avail in the third stage, which was, he might say, almost incurable: but they acted very effectively in the second and first stages.—*Lancet, June,* 18, 1859, *p.* 613.

28.—CASES OF ASCITES, APPARENTLY ARISING FROM CHRONIC PERITONITIS.

By Dr. William Paley, Peterborough.

[This paper, which was read before the South Minland Branch of the British Medical Association, is an abstract of thirty-six cases of ascites, apparently arising from chronic peritonitis. It is noteworthy that in most of the complications in these cases, the strumous diathesis seemed strongly marked. The disease is essentially chronic, more liable to supervene in young persons, especially females; fourteen of the cases being males, and twenty-two females. In only one case, out of the five which died, was an examination permitted, and in this, the peritoneal surface was studded with tubercles, and a considerable quantity of sero-purulent fluid was contained in the peritoneal cavity. With regard to the treatment the author remarks:]

Treatment.—In the earlier cases I tried diuretics, purgatives, and counter irritants without much effect; then I used the iodides, principally the syrup of iodide of iron, and friction over the abdomen, with ointment of iodide of mercury, both, I am bound to say, with good effect; still the absorption of the fluid went on very slowly; a long course of treatment was required before it could be effected, and the patient's strength was very apt to give way in the meantime. At last I tried cod-liver oil, and found it so successful, that for the last eight years it has always formed the basis of my treatment. I generally give at the same time the syrup of iodide of iron, if there is no diarrhœa, and use friction over the abdomen with the iodide of mercury ointment. I lay great stress also upon keeping my patients as much as possible in the open air, giving a nourishing but unstimulating diet, carefully avoiding either invaliding them or reducing their strength by too active remedies. If there is diarrhœa or very much pain in the abdomen, I give small opiates, either with or without astringents, as the bowels may require. In two cases only, from the great distention of the abdomen, was the operation of tapping thought desirable; in one, a child, it gave no permanent relief, the body filling again in a few days: it was not therefore repeated. In the other case, that of a man, in which I suspected an abdominal tumour, it was performed four times, and seemed materially to assist the cure, the quantity of fluid diminishing after each operation.

In conclusion, and lest any one should be disappointed in not being able to effect a cure so rapidly as he could wish in similar cases, I will just mention the length of treatment required in the cases given; one, one week (died); four from two to three weeks; two from three to four weeks; two from four to five weeks; two from five to six weeks; four from six weeks to two months; eight from two to three months; four from three to four months; three from four to six months; three from six to nine months; two from nine to twelve months; one eighteen months.—*Brit. Med. Journal, July* 23, 1859, *p.* 587.

29.—ULCEROUS STOMATITIS IN THE ARMY.—CHLORATE OF POTASH.

Ulcerous stomatitis, which almost exclusively occurs in private practice among pauper children, and more especially when they are collected together in hospitals, asylums and workhouses, is also far from uncommon among our young soldiers. In consequence of exceptional circumstances caused by the Crimean war, several physicians attached to the civil hospitals were entrusted, in 1855, with medical wards in the military hospitals of Paris. M. Bergeron occupied one of these temporary situations, and to that circumstance we are indebted for an excellent memoir on ulcerous stomatitis in soldiers, and its identity with the peculiar stomatitis of children which has been called ulcero-membranous.

In his wards, at the Hopital du Roule, M. Bergeron was much struck with the frequency of ulcerous inflammation of the mouth among the garrison of Paris at that time, the disease chiefly attacking recruits. The development of the affection was clearly under the influence of hygienic circumstances—a fact proved by the privates being more subject to it than non-commissioned officers, and the latter far more than officers. As causes of the malady M. Bergeron indicates the want of variety of the food, the insufficiency of the regulation supply of spirits, crowding, and, as an agent of propagation, contagion. He also ascertained that suppuration within the alveoli constitutes a local predisposition to this specific form of stomatitis.

From M. Bergeron's interesting researches we also gather the following singular and unexpected fact—viz., that the disease is all but unknown in the navy, and that it has been observed neither as an epidemic nor as an epidemic affection in the armies of Great Britain, Austria, Denmark, Egypt, Holland, Naples, Prussia, Sardinia, Saxony, Sweden, Tunis, or Wurtemburg; that, moreover, it is almost as frequent in the Portuguese army as in the French land forces, and that it occasionally is met with in the Belgian troops.

Endemo-epidemic stomatitis is anatomically distinguished by ulceration of various form and extent, chiefly occupying the gums and inner aspect of the cheeks, but which may invade any part of the mucous surface of the mouth, and by enlargement of the submaxillary or cervical glands. These changes are invariably attended with much pain, abundant salivation, and extreme fetor of breath. Ulcerous stomatitis has nothing in common with dipththeria, and differs fundamentally from *grangrene of the mouth, phagedenic sores*, or scurvy. It is entirely identical with the buccal inflammation of children, which has received the denomination of *ulcero-membraneous stomatitis*.

When properly treated, this disorder terminates favourably in ten days; whereas, if abandoned to nature, it may last as much as three months, and is therefore deserving in the highest degree, of the attention of the profession.

The most rapidly efficacious method of treatment, the safest, and at the same time the least troublesome, consists, in the present state of science, in the exhibition of chlorate of potash, preceded or not by an emetic. The chlorate being, however, not very soluble in cold water, and being liable to deposit, if merely added to the mistura acaciæ, it occurred to M. Vial, chief apothecary to the hospital of the Roule, to prepare a warm solution at 1-20th, to be added to the excipient. Thus M. Bergeron's prescription consisted of—

℞. Mist. acaciæ..12 dr.
Solut. protassæ chloratis..........................16 dr.

to be taken in four doses, at intervals of three hours. When, after six or seven days treatment, and gradual improvement, the reparative action was suspended, the dose of the solution of chlorate was increased to three ounces, and if no beneficial results followed this augmentation, the exhibition of the chlorate of potash was temporarily interrupted, and the dry chlorate of lime substituted. In most instances, however, a speedy cure resulted from the use of the chlorate alone. The great benefit conferred upon the army by the introduction of this substance into medical practice in military hospitals, from M. Bergeron's felicitous applications, is obvious. Of course, in order to check the development of the disease, and rid our regiments of this endemo-epidemic affection, hygiene must interfere; and it is by improvements in the food, in the repartition of duty, and chiefly in barrack accommodation, that M. Bergeron deems this end may be attained. While looking forward to the realization of these reforms, perhaps it might be possible, by some simple measure, greatly to diminish the chances of ulcerous stomatitis. M. Bergeron expresses the opinion that soldiers should be compelled to submit to examination of the cavity of the mouth, and that when the presence of the disease is duly ascertained, the patients should be isolated, and placed under the influence of the chlorate of potash medication. M. Bergeron also suggests, that instead of merely requiring that a tooth-brush should form part of a soldier's necessaries (a good thing of course), he should be under the further obligation of using it with the same punctuality observed in the performance of ablutions upon the head and hands, which are at all times strictly enforced.—*Journal of Practical Medicine and Surgery,—Dublin Hosp. Gazette, Aug.* 1, 1859, *p.* 236.

30.—THE CEREBRO-SPINAL SYMPTOMATOLOGY OF WORMS, ESPECIALLY TAPE-WORMS.

By Dr. Thomas P. Heslop, late Senior Physician to the Queen's Hospital, and Professor of the Practice of Physic, Queen's College, Birmingham.

[Serious aberrations in the nervous functions are caused by the presence of intestinal worms. The author establishes this proposition by a reference to the whole of his recorded cases during the past seven years; observing that cases obtained from the entire experience of one observer over a period of many years are less likely to lead to error, than those observed by many different persons, none of whom may have taken the requisite precautions to exclude error.]

I have observed that the symptoms of the presence of worms have, in fact, a striking uniformity, and give a peculiar physiognomy to a case. Since the first part of this paper was in print I have seen a good illustration of this. A lady, about thirty-five years of age, of pale complexion, consulted me for most obstinate headache and general neuralgic phenomena. The senses were dulled; the skin dry; thirst considerable; urine much more copious than natural, and pale. Though doubtful of the real nature of the case previous to an examination of the urine, I yet ventured to hint that probably worms were at the bottom of her neuralgic complaints, and prescribed an anthelmintic draught. No worms were passed, and the urine was found, though of low specific gravity, to be free from any other peculiarity. The idea of worms was almost abandoned, yet, three or four weeks afterward, a very large lumbricus was passed into the clothes while going up stairs, and its expulsion was attended with a certain amount of relief. At first sight, this case seemed to warrant the fear that some chronic kidney affection was the cause of the symptoms, and I did not feel satisfied upon this head until I had enjoyed the opportunity of a careful examination of the urine. But it was not the only time that such doubts have entered my mind in investigating the symptomatology of worms. There is the strictest similitude between the cerebro-spinal symptoms of some forms of Bright's disease, especially of that form attended with the hard, contracted kidney, and those attendant upon worms, particularly the tape-worm. So close is this likeness, that in cases where I should have otherwise predicted the presence of worms, my mind has been suspended in doubt until after the urine had been subjected to scrutiny.

This point is too interesting, and offers too many valuable considerations for our notice, in speculating upon the conditions of these symptoms, to be passed over without a little further comment. What—looking at the observations which have been already adduced—may be fairly termed the phenomena of a representative case of vermination in the adult? They are about as follows:—A man, with a pale face, but fair *embonpoint*, presents himself to his attendant, complaining of dull headache, not limited to any particular region of the cranium, but perhaps more marked in the frontal region than elsewhere. This headache does not offer any marked exacerbations or remissions, but is almost constant and sufficiently severe, without being excruciating, to render life, if not a burthen, at least unhappy. Giddiness is so severe that he often staggers about like one intoxicated; and when this symptom is present to a less degree, there is still almost continuously a sense of confusion and insecurity, which renders walking a serious effort. The ears are affected with a constant buzzing, and a great variety of noises, described sometimes as being like that produced by the boiling of a tea-kettle, sometimes like the letting off of steam from a boiler, not rarely like the rumbling of thunder. The acuteness of the sense of hearing is, at the same time, unimpaired. His vision is dull. Fine webs seem to be constantly before the eyes, so that every object wears a hazy aspect; at other times, dark spots or brilliant flashes obstruct vision. Next to the headache and giddiness, it is the increasing imperfection of this sense which most alarms the patient. He finds that in seizing objects his hands are not so thoroughly under control as formerly. They and the arms are in a state of constant tremor; and the legs are similarly, but to a less degree affected. This symptom is sometimes so severe that he finds it difficult to hold objects firmly, unless he concentrates his attention upon his actions. He has a variety of unusual sensations about his body, all more or less referable to numbness. The stomach occasionally rejects its contents, and he is troubled with a general sense of uneasiness about the abdomen, sometimes with a sharp colicky pain. He has, perhaps, lately suffered a severe convulsive seizure, or been the subject of a transient stupor. If the patient be a woman, violent hysterical crises have occurred, or genuine epileptic attacks. The face is not merely pale; there is an aspect of languor and heaviness, much increased by a drooping of the upper lids in a severe case. Such is an abstract of the more common symptoms of worms, and I know of one disease only which presents a similar combination of morbid signs, and that disease is morbus Brightii. Fortunately the renal secretion is at hand to solve the difficulty and to exclude doubt.

This similarity of symptoms engendered by causes apparently so opposite as toxæmia on the one hand, and intestinal irritation on the other, has led me to doubt the generally received opinion, that the nervous phenomena of worms are connected with the mere results of these parasites, as irritants of the intestinal tract. There is no eccentric irritation known to us which produces like effects, and all analogy is in favour of the view that a generalized lesion of the nutritive function as indicated by a morbid blood-plasma, is the real source of these remarkable pathological states. This view is confirmed by the reflection that a profound lesion of the primary assimilating processes generally accompanies, and probably precedes, the generation and development of these worms. And in fact, it is difficult to explain their presence, except on the supposition that a suitable nidus has been previously prepared in the shape of morbid secretions, wherein the germs can fertilize. To these germs probably most persons, both in civilized and uncivilized life, are pretty frequently exposed, yet a large majority of mankind undoubtedly escape being infested by them. It is consonant with all our knowledge of natural history to suppose that those who escape are in a physiological condition, have their secretions in a healthy state, and consequently offer no resting-place in which the ova of entozoa can become developed. Hence, too, the greater frequency of these parasites among the ill-fed and half-fed denizens of our great towns, a fact which does not compel us to seek for its cause in a greater exposure to the germs under such conditions. Dr. Elliotson very justly observes, that "generally speaking, these worms prevail more especially in proportion as the patient is weak. We know that persons who are exhausted from fever will become the subject of vermin. In extreme debility of the constitution, a patient may be cleaned several times a day, and be covered with a fresh crop of vermin! So it is, in general, within. The more weakness there is of the body, the more the entozoa thrive. Bad air, bad food, and the want of sun, will contribute to their appearance. When rabbits are kept in a bad place, they become subject to hydatids; as do also sheep in wet pastures."

We find a further confirmation of these views in the circumstance that, though relief is speedy when the symptoms above related have been brought about by the presence of worms, it is by no means immediate, as it would be if a mere intestinal irritant were the sole condition of their existence. So far from being immediate, the relief consequent upon the expulsion of worms, in severe cases, is but slowly effected, even when we find no reason afterward to suspect the presence of others in the body. This is very manifest in many of the cases, and deserves notice in a practical point of view, as it serves to demonstrate the necessity for careful dietetic observances, and judiciously applied medicinal agents for some period after the end in view has been apparently gained. Here, too, is the source of the frequent disappointment of patients, who begin to despair of cure when they observe a constant recurrence of the symptoms and of the parasites, in spite of powerful anthelmintics periodically administered. The palliative is the vermicide;

the radical cure is to be found elsewhere, though medical doctrines lead to a reverse conclusion.

There are, confessedly, certain disorders engendered or maintained by worms, whose relief is strictly synchronous with their expulsion, whether effected by nature or art: such as hooping-cough, infantile convulsions; but these exceptions do in fact prove the rule. The nervous system in early life very quickly sympathises with intestinal irritation, and a crisis occurs, such as is well calculated to draw the attention of the physician to the possible presence of worms, long before they have materially impaired the general nutritive functions. The species of worm present in most of these cases, too, is such as forbids the belief that a very marked interference with those functions is likely to occur. Ascarides in the great majority of these cases, and lumbrici in the rest, are the worms with which we have to deal. The former seem to infest the large intestine almost exclusively, and its extreme portion in most cases; while the lumbrici, though invading, by preference, the small intestine, do not offer, unless accumulated in great numbers, the enormous bulk so common in tænia, and are, therefore, less likely to interfere with the normal functions.

In reference to the aggravation of hooping-cough in certain states of the economy, Dr. Whitehead, in his third Report of the Manchester Clinical Hospital, says:—"By a similar kind of sympathetic irritability, the presence of worms in the intestines will aggravate the symptoms, or prolong the duration of hooping-cough almost indefinitely; in several instances in which the symptoms continued unabated unduly long, and where it was found that worms existed, the expulsion of these parasites was immediately followed by mitigation of the symptoms, and speedy cure."

It is, however, to be observed, that these and similar symptoms are frequently to be noticed in tænia. Thus, Dr. Watson gives a case from Bremser, "of a child eleven years of age, afflicted with tænia, who had a troublesome dry cough. It was observed that her cough was suspended for two months, just after a very large portion of the worm had been brought away by anthelmintic medicines. This kind of coincidence happened not once only, but three or four times; and, at length, when the whole of the worm had come away, the cough was permanently cured."

Dr. Graves gives a case of a young lady who had fits of coughing of extraordinary intensity; the cough was dry, loud, and hollow, and repeated every five or six seconds, night and day, when she was asleep as well as when she was awake. In spite of this terrible agitation, she did not fall away proportionally. Every imaginable measure was employed, but no relief followed. The case was "given up in despair." Some time afterward Dr. Graves heard that she had been cured all at once by that ubiquitous medical practitioner, "an old woman," who had suggested a dose of turpentine and castor oil, for the purpose of relieving a sudden attack of colic; two or three hours afterward the young lady passed a large mass of tapeworm, and from that moment every symptom of pulmonary irritation disappeared.

It is hardly necessary to draw attention to the frequency with which "fits" of every variety are produced by worms. As I have before observed, this is abundantly recognized so far as regards infantile life, though certainly insufficiently in reference to the adult. It is remarkable, that wherever convulsive seizures are mentioned in connection with worms, either by Willis or Heberden, lumbrici are alone mentioned. This can hardly spring from ignorance of the distinction between round and flat worms—as it is undoubtedly as old as Hippocrates—but results, I believe, from observation of the superior liability to these seizures in persons invaded by this species. Their peculiar conformation and resistant head are favorable for attacks upon the wall of the intestines—Von Siebold believing that they are able to force asunder its fibres; and their tendency to wander to the anus or to the pharynx is one of their characteristics. Küchenmeister observes, that "if the worm be disquieted by any causes, it begins to wander about in the intestine which it inhabits, producing all kinds of disorders, which may even lead to death. According to the irritability of the individual, the number of the wanderers, the place to which they have wandered, and lastly, according to the power of the worms themselves of asserting their vital activity, so varies the danger to which these wanderers give rise." These causes of agitation are internal and external. The former, seated in the worm itself, are, perhaps, as Küchenmeister suggests, only to be found in the sexual actions: it must not be forgotten that these are not, like tænia, hermaphrodite. The latter are in relation with the ingesta, some of which must be more or less repugnant to the parasite.

The singular frequency with which lumbrici are found in the bodies of those dead of tetanus has not yet obtained the attention it deserves. It is probable, too, that a more thorough examination of the intestines, in such cases, would reveal their presence still more often. My friend, Mr. West, of the Queen's Hospital, tells me that a young woman, aged 17, was admitted into that institution, under the late Dr. Fife, in the year 1855. She was anemic in appearance, and the catamenia were irregular. A peculiar "tonic convulsive condition" of the muscles of the right arm was noted on admission, which had resulted, as she stated, from a blow she had given to the thumb of the right hand, while cleaning a pair of boots. She was believed, at first, to be suffering from hysteria, but she was seized with symptoms of trismus, and died in less than forty-eight hours. No appreciable lesion of the brain and spinal cord was discovered. All the thoracic and abdominal viscera were healthy. A lumbricus was found in the neighborhood of the ilio-cæcal valve.

A deeply interesting case of a similar nature is reported by Mr. Johnson, of Bedford. A boy, aged eight years, was brought to the Bedford Infirmary on the 24th of July, with a lacerated wound above the right elbow, caused by the wheel of a cart passing over the arm. Sloughing took place, and, on the 30th, a large sore, four inches by two and a half, was exposed. The boy went on extremely well until the 8th of August; his general health improved, and the wound presented a most healthy appearance. On that day symptoms of trismus made their appearance, and he was ordered two grains of calomel three times a day. On the 10th, paroxysms of opisthotonos occurred every three or four hours. On the 13th the gums had become slightly affected, and the paroxysms were not quite so violent. On the 15th he passed four lumbrici, five inches in length. From this time until the date of his death (August 25th), the paroxysms appeared to diminish very much in severity and frequency; but the length of time that these symptoms had lasted, viz., thirty-two days, had rendered him so weak and emaciated, that he died exhausted. The mucous lining of the upper part of the small intestine was found much inflamed, and in the upper part of the jejunum three masses of lumbrici were discovered, numbering thirty-six. Mr. Johnson believed that there was an intimate relationship between the tetanus and the presence of the lumbrici—a belief which appears to be justly based on the facts of the case.

Last summer a boy, about fourteen years of age, was admitted into the Queen's Hospital, under my care, laboring under the most violent tetanus. He had been affected by these symptoms about two days. There was an obscure history of an injury, as usual, but nothing could be found about the body to justify the statements made. A few hours after admission he died. The examination of the body was deemed very inconclusive. Softening of a small portion of the dorsal cord was observed; but there was some ground for the suspicion that the injuries accidentally occurring during the necropsy might account for this. Lumbrici were found in the intestinal canal.

It is within the limits of fair speculation to suppose that in these cases, and others of a like character, the tetanic phenomena were excited by the lumbricus in persons who already were in a condition favourable to the development of such phenomena, but who might have escaped had the worm not been present. The case reported by Mr. Johnson especially warrants this opinion.

Many of my own cases tell a history of fits in connection with worms—often the most violent hysterical cries were observed. Genuine epileptic seizures were also several times observed.

I have felt much hesitation in giving a name to some of the seizures produced by worms. If "excentric eclampsia" would appear the fitting term for some of the cases, especially those occurring in early life, and, for the most part, caused by lumbrici or ascarides, it would seem inapplicable to many others, which rather deserve the appellation of toxœmic eclampsia, and are assimilated to the well-known nervous lesions of Bright's disease, as before mentioned. But it is not so important to agree upon the precise nosological phrase wherewith to characterize these conditions as to thoroughly appreciate the diverse and often mixed character of these paroxysms. Intense hysteria, with the most violent jactitations, or, on the other hand, attended with an imperfectly developed cataleptic state and profound repose, sudden insensibility without convulsion, every variety of convulsive seizure attended with insensibility; or tonic spastic states, without unconsciousness, may constitute the particular expression of nervous sympathy, and yet acknowledge but one origin.

Few authors omit to place worms among the causes of cholera, although they are not named by Heberden in this relation, and Romberg gives but little countenance to the doctrine. He states that the disease can scarcely be shown to be produced by irritation. "Reflex irritation most frequently resides in the intestines and genital organs; but under this head a more important part has been ascribed to helminthiasis than it is found to merit on closer examination." Notwithstanding, this author gives one illustrative case. A boy, aged six years, in whose fæces the mother not unfrequently discovered traces of ascarides, rapidly recovered under the employment of calomel and jalap powders, which brought away a large quantity of the oxyuris vermicularis. I arrived at the same conclusion with Romberg some time ago, from observing the infrequency of worms in the history of cholera. This disorder, indeed, seems to belong to the diathetic neuroses, and to acknowledge but rarely intestinal or other irritation for its cause. The subject of the nineteenth case was suffering from cholera, and had passed pieces of flat-worm. But here there was a marked systolic bruit. In such a case the rheumatic diathesis, or, in others, an extreme nervous mobility, may be the predisposing condition, while the presence of worms, or a fright, may be the immediate excitant.

Krause is quoted as having seen a case of a young man, of robust constitution, aged 31, who had been attacked for many years with paroxysms of involuntary laughter. At every accession he experienced a malaise, which was relieved only by lying down flat on his belly. None of his previous advisers had questioned the patient about worms, which he admitted having passed some years before. The administration of efficient vermifuges immediately relieved him of all his disagreeable symptoms. Another young man, mentioned by Girandy, presented some very singular symptoms, one of which was that he found it impossible to walk over any substance, however small, even a sheet of paper. Every time he endeavored to conquer this difficulty he fell into a state of syncope. Vermifuges effected the evacuation of many worms, and a cure resulted. A case is quoted from Mœnnich, of a child between two and three years old, previously robust, who was suddenly attacked with paralysis of the lower extremities, strabismus, and distortion of the features. After the administration of vermifuges he passed eighteen ascarides, with a great quantity of glairy matter, and his health was reestablished.

Küchenmeister quotes from Seeger a valuable table of the comparative frequency of particular symptoms, from a statistical table of 100 patients with tape-worm, which yields a widely different result from that arrived at by Louis; and far nearer, though still with important differences, that which my own experience would warrant. "Sixty-eight times there were cerebrospinal affections, and partial or general convulsions (for example, epilepsy, hysteria, melancholy, hypochondriasis, abdominal spasms, dyspnœa, and convulsive coughing), which may even rise to maniacal attacks, and mental weakness; forty-nine times, nausea, even with vomiting and fainting; forty-two times, various pains in the abdomen; thirty-three times, disordered digestion and irregular evacuations; thirty-one times, irregular appetite and voracity; nineteen times, periodical, habitual, headache, usually on one side; seventeen times, sudden cholic; sixteen times, undulatory movements in the abdomen, up to the chest; fifteen times, dizziness or delusions in the senses, and defects in the speech; and eleven times, shifting pains in various parts of the body." Küchenmeister refuses to ascribe these symptoms to the worm; and his reason is, that even when it has been expelled, they do not disappear. He also quotes, half-approvingly, the Abyssinian belief, that tape-worm only thrives in a healthy intestine. I have already observed that the expulsion of the tape-worm may not take away the verminous malady; and that the vermicide must be held as preliminary only to a course of measures directed against the condition which maintains the existence of the parasite. Such arguments, therefore, as those of Küchenmeister must be held as inconclusive.

Dr. Wood, of Philadelphia, has given an excellent description of the symptoms produced by each variety of intestinal worm, and more thoroughly appreciates the serious nature and frequency of the nervous symptoms than any recent author with whom I am acquainted. He, like M. Grisolle, quotes the results arrived at by M. Wawruch, of Vienna, and which are too valuable to be passed over. During a period of twenty years the professor had seen 206 cases of tape-worm—a number sufficiently numerous to warrant positive results. The following is his summary of the symptoms:—Dull pain in the forehead, giddiness, buzzing in the ears; dulness of the eyes, which are surrounded by a dark circle; œdematous eyelids, dilated pupils, frequent and spasmodic movements of the eyes; alternate paleness and flushing of the face, palenes of the lips, peculiar movements of the nose and mouth; emaciation, alternate loss and excess of appetite, cravings for particular articles of food; offensive breath, furred tongue, spitting and vomiting of thin mucus in the mornings; itching at the nose, anus, and vulva; grinding of the teeth, especially during sleep; constriction of the throat, swelling of the belly, gurgling, shooting pains, and a sense of pinching about the umbilicus; a feeling in the morning as of a foreign body moving in the bowels; amelioration of all the symptoms under the use of farinaceous food, hot bread, and coffee; finally, depression of spirits, and a train of nervous derangements in protracted cases. "Sometimes," adds Dr. Wood, "the nervous disturbance amounts to convulsive movements of an epileptiform or hysterical character; and when these occur in a man with signs of an abdominal disorder, the possible existence of the worm should be suspected."

This latter remark well deserves to be kept in recollection by the clinical student. In a case presenting the symptoms alluded to, in the absence of the signs of any special brain disorder, the existence of even a few ill-defined disturbances in the digestive functions should arouse suspicion and inquiries relative to the existence of worms.

I now, in conclusion, place before the reader, in a summary manner, the conclusions at which I have arrived, and which appear to me fairly drawn from my cases.

1. That, in a great majority of cases of tape-worm, and, though with lesser frequency, in cases of other intestinal worms, more or less serious and peculiar nervous disturbances are apt to arise.

2. That the most frequent of these are headache, giddiness, various troubles of the special senses, especially singing in the ears, flashes and dark spots before the eyes, imperfect amaurosis, and trembling of the limbs.

3. That various anæsthetic, and, on the contrary, neuralgic phenemena, are very frequent, usually connected with general lassitude and sense of muscular feebleness.

That, though less frequent than those previously cited, convulsive seizures, partaking of the nature of epilepsy or acute eclampsia, or sudden attacks of insensibility, mixed with syncope, and, in the female sex, severe forms of hysteria, are also often directly traceable to worms.

5. That the last symptoms, (No. 4) are more common in childhood, and the

earlier periods of life, than afterward; and are more frequently caused by the round and thread-worm than by the tape-worm.

6. That chorea does not appear to be often excited by the irritation of worms.

7. That a feeble state of the general health generally accompanies the presence of worms; often, in cases of tænia, proceeding to marked anemia, so as even to lead to the suspicion of the possible existence of Bright's disease.

8. That the irritation phenomena of the digestive tube, even when associated with various symptoms referred to the functions of that tract, do not warrant the diagnosis of the presence of tænia; and that their absence does not absolutely indicate the absence of the parasite.

9. That the frequent appearance of the nervous symptoms above related, without a well-marked relation to any special lesion of the nervous system, especially if alternating with periods of perfect or nearly perfect freedom, should engender the suspicion that worms are present. If to those symptoms are added various ill defined disturbances of the functions of assimilation, including occasional colicky pains, without marked vomiting, pain after food, or decided emaciation—it is in the highest degree probable that worms are the source of the symptoms, and steps should be taken to obtain assurance of their existence, or the contrary.

10. That it is probable that many of the sympathetic phenomena of vermination are connected, not with their direct irritation of the mucous membrane, with which they are in relation, but with a general disorder of the system, partly resulting from the parasites, and partly the cause of their maintenance and development in the intestinal tract.—*Dublin Quarterly Journal, Aug.*, 1859, *p.* 133.

31.—*Tapeworm induced by eating Raw Bacon.*—Recent investigations have done much to elucidate the hitherto mysterious question as to how certain parasites find their way into the human organism. Respecting the common tapeworm or tænia solium we now know for certain that its scolex, or immature form, is the cysticercus cellulosæ, and that the latter abounds in pork. We know, further, as a fact in the social history of different nations that this kind of tapeworm is found in almost exactly proportionate frequency to the use of pork as an article of diet. Wherever, as in Poland, Hungary, and our own country, the breeding of pigs flourishes, there the tænia solium abounds; and *vice versâ*, wherever, as in Iceland, there are very few pigs, there it is almost unknown. A sound physio-pathological reason may be given for the religious ordinance of circumcision among the Jews; and here we seem to have a parallel one offered for the prohibition of the flesh of swine. It is asserted by Küchenmeister that this worm "is almost entirely unknown among those Jews and Mohammedans who live strictly according to their religious precepts, and who are not deprived of the opportunity of procuring their meat from 'clean' butchers' shops in which pork is never sold."—*Med. Times and Gazette, May* 28, 1859, *p.* 550.

32.—HEPATITIS TREATED BY CHLOROFORM.

By Dr. Kidd, Physician to the Metropolitan Dispensary.

In a case of acute liver disease, which came under the notice of Dr. Kidd, of the Metropolitan Dispensary, within the last month, the action of chloroform exhibited internally was quite magical. The patient, a delicate female, had been suffering for some days under what was described and believed to be acute inflammation of the liver with obstruction of the gall ducts; for this antiphlogistic measures were had recourse to with little or no avail; the tenderness over the liver as of hepatitis, was excessive, still the jaundiced colour of skin was unaltered. On seeing the patient with this history, Dr. Kidd, on carefully watching the nature of the pain (though in everything else it stimulated the pain of inflammation of the peritoneal coverings of the liver itself), yet he detected that the pain had decided remissions. "Oh, I feel the pain coming if you touch me," the patient cried out more than once; the least touch, or examination of the part, in fact, brought it on, nor did it seem to be limited alone to the hepatic region, but to the entire of the right side below the nipple. "No hepatitis here," said Dr. Kidd, "but possibly gall stones and hysteria," and so it proved to be; the patient had been twenty-four hours in high fever and constant agony. Venesection, *ad deliquium*, calomel, to the saturation of the system, such as would delight Mr. Ranald Martin or his school, leeches and cupping, all loomed hazily in the distance; the case, in a word, was very similar to that of Mr. Augustus Stafford, who, Dr. Snow, in his book says, would have been rescued from death by a drachm of chloroform. The treatment no doubt, in both cases would depend on a correct diagnosis. Dr. Addison and Mr. Skey are in the habit of remarking, where in inflammations of the peritoneum or elsewhere, you have very excessive tenderness on pressure take care that it is inflammation at all. The remark is a good one, and convinced by it, Dr. Kidd having rubbed two grains of calomel to the patient's tongue, commenced the treatment with ten drops of chloroform internally every half hour till the pain seemed to alter. The effect was magical; the pain subsided after three doses, allowing a free examination of the parts and free fomentations with hot flannel; the bowels, which had not been open for some time previously, were soon after acted on; the menstrual function, which had been delayed ten days beyond its time, and was beginning to cause some alarm, returned also quite naturally.

Two days subsequently when the medicine was altered the pain again returned, but yielded again to the chloroform and the application of four leeches over the site of the gall ducts. The patient after this recovered perfectly.

The rationale of the treatment in such cases as this is to be found in the fact, that chloroform has a decided or specific action on muscular tissues and semi-muscular canals like the urethra or gall ducts. Dr. Gull, of Guy's Hospital, has also published cases of gall stones, where chloroform gave very manifest relief, and Dr. Snow, as already stated, refers to the well known case of Mr. Stafford, where chloroform unfortunately in a miserable village could not be procured, though the physician in attendance sent for some. Opium in overpowering doses having been then administered and large bleeding tried the patient was lost.—*Medical Circular, July* 13, 1859, *p.* 20.

33.—ON JAUNDICE.

By Dr. Stephen H. Ward, Physician to the Seamen's Hospital "Dreadnought," &c.

[The author, after citing cases of jaundice from the different functional and structural derangements of the liver, observes],

I cannot but allude with some satisfaction to the light which morbid action is ever throwing upon healthy function. Chemists are still undecided as to the exact composition and uses of bile; but the phenomena of disease, in cases such as I have narrated, have given us nearly all the insight we possess into the real purposes of the liver. From the comparative torpor of the brain, and the effort with which it performs its functions in some cases, and the more serious cerebral symptoms in others, where there is no secretion of bile by its usual channel, and imperfect elimination, perhaps, by the kidneys. we learn that this fluid contains materials, the separation of which form the blood is essential to health. And analyzing the cases a little more closely, we find that the evil day of cerebral implication more immediately impends where the actual secretion is arrested than in those in which the fluid has been separated from the blood, but has been prevented by obstruction in the ducts from passing off in its usual course. The anæmia, passive hemorrhages, &c., which ensue when the secretion of bile has been interfered with for any length of time, show the effect upon the blood itself. One symptom seems to result from the absence of bile, for any lengthened period, from the intestine, and that is emaciation, or, at any rate, non-renewal of adipose tissue. It confirms the views of chemists, that bile contains a soapy kind of material, which effects the solution and consequent absorption of the fatty portions of the chyme. Recent experiments would seem to show that the pancreatic juice is adequate to the solution of fatty matters; but my own observation of the consequences which ensue where the bile is absent, leads to the conclusion stated. The constipation which attends the deficiency or absence of bile, and the diarrhœa consequent upon its excess, prove that the purpose of part of its ingredients is to stimulate the peristaltic action of the bowels, and promote the removal of excrement. Further, the offensive character of evacuations devoid of bile points conclusively to its antiseptic properties.

In conclusion, I have one or two observations to make in reference to the treatment of jaundice. There is one principle to be borne steadily in mind in all cases, whatever their cause, and that is to promote in every way the functions of those organs by which compensatory elimination of bile is effected. To carry out the principle, we must avail ourselves of warm and vapour baths, saline purgatives, and the various kinds of diuretics. In case 5, I exemplified the fatal results which followed suspension of the function of the kidneys through the action of a blister. Acting upon the experience derived from the case in question, I would advise, under similar circumstances, that recourse should be had to some other form of counter-irritation than blistering—such as strong liquor of ammonia, mustard-plaster, &c. The strong blistering fluid, which produces vesication quickly, would be less likely to be absorbed into the blood.

In jaundice from acute congestion of the liver, leeches, cupping, (either with or without the scarificator), fomentations, &c., over the region of the liver, and saline purgatives to unload the gorged portal system, are the curative measures most likely to be followed by relief. When the congestion is primary, due to spirit-drinking, and such as may go on to inflammation of the adhesive character, mercury pushed to slight specific action, and followed by iodide of potassium, would appear by the cases cited to be indicated. In cases of closure of duct, mercury can do no good; here we can only carry out the principle of elimination by other channels. In jaundice from suppression of bile consequent upon mental or moral causes, the treatment consists in cholagogue doses of mercury, saline purgatives, diuretics, warm or vapour baths, and above all, in the removal of the exciting or sustaining cause. When bile once appears in due quantity in the alvine evacuations, we must not go on pushing our remedies simply because the skin continues jaundiced; for, as Dr. Budd, who lays great stress upon this point of practice, observes, some time must elapse before skin can regain its normal colour.

Threatenings of cerebral implication are to be met by drastic purging, counter-irritation to the nape of the neck and calves of the legs, and free action on the kidneys; and, as we have seen, may frequently be met successfully.—*Lancet, July*, 16, 1859, *p.* 54.

34.—CASE OF ANÆMIA LYMPHATICA, A NEW DISEASE CHARACTERIZED BY ENLARGEMENT OF THE LYMPHATIC GLANDS AND SPLEEN.

(Under the care of Dr. PAVY, Guy's Hospital.)

Much interest was excited amongst the profession by the announcement of Dr. Wilks, at one of the meetings of the Pathological Society during the past session, that the morbid specimens which he exhibited were taken from a patient in Guy's Hospital, whose disease was new, and hitherto unnamed. The essential features of the disease are the most extreme pallor of anæmia, enlargement of one or more of the various groups of lymphatic glands, either internal or external to the body, and a peculiar morbid condition, with occasional enlargement, of the spleen; the last depending upon the deposition of an opaque, white, lardaceous material, in isolated masses, or diffused throughout the substance of the organ, and resembling bacon-rind. The malady is so striking, and yet so peculiar, that when carefully studied it is almost impossible to mistake its identity. Six cases are detailed in the second volume (third series) of Guy's Hospital Reports, in a paper by Dr. Wilks, "Cases of Lardaceous Disease and some Allied Affections." All of them proved fatal, as well as those which have since come under our notice. The peculiarities noticeable in these cases were as follows:—

Case 40.—Enlargement of the lumbar and posterior mediastinal lymphatic glands, forming a chain of tumours along the whole length of the spine upon each side of the aorta; spleen enlarged, opaque white, deposits through it; age 24.

Case 41.—Lumbar glands much enlarged, and accompanying the aorta along the spine to the pelvis; mesenteric and bronchial glands enlarged; spleen large, with a number of ovoid white bodies; age 9 years.

Case 42.—Cervical, mediastinal, bronchial, and lumber glands enlarged; spleen four times larger than natural, three-fourths of it resembling opaque white tallow; age 10 years.

Case 43.—Lymphatic glands of neck, groin, and around the great vessels in the chest and abdomen, enlarged; spleen had a few white tubercles; aged 16 years.

Case 44.—Great enlargement of the absorbent glands of the neck, axilla, and groin; spleen enlarged and an infinite number of small, white, opaque deposits; aged 50.

Case 45.—(Dr. Markham, 4th vol. Translations of the Pathological Society.)—Enlargement of the anterior and posterior mediastinal glands, encircling the arch of the aorta; spleen enlarged, with small yellow masses throughout; age 30.

Some other instances might be added to these; but it will be sufficient to append the following, shown to the Pathological Society in the course of its last session:—

"Enlargement of the cervical, mediastinal, and lumbar glands; the spleen much enlarged, with white deposits throughout; age 22."

The enlargement of the lymphatic glands, which thus seems the peculiar feature of this malady, is remarkable for the lingering form of fatal cachexia which it produces. The extreme pallor of the patient—as we have witnessed at this hospital—at once attracts the attention of the observer.

In relation to the six cases we have briefly noticed, Dr. Wilks observes, in regard to the symptoms during life and the appearances after death—"Their uniformity is too considerable to constitute merely a coincidence of disease between the glands and the spleen, and therefore there is, without doubt, a peculiar form of affection involving these organs, accompanied by an anæmic cachexia, prostration, and death. I say a peculiar affection: for though allied to the tubercular, I believe it to be one not yet recognized under the ordinary forms of disease."

This affection has been mistaken for scrofula, especially where the glands in the neck of weakly children have commenced to enlarge. It occurs to persons of all ages. It may gradually extend over a period of two or more years, when the thoracic and abdominal glands become involved, and slow prostration precedes death.

The intimate structure of the enlarged glands is a fibro-nucleated tissue, and this is not to be distinguished from ordinary fibro-plastic growth. Dr. Hodgkin described a case of this kind in the seventeenth volume of the Medico-Chirurgical Transactions, in which he refers to its connection with a peculiar affection of the spleen; but he affixed no name to it. Dr. Wilks correctly styles it *anæmia lymphatica*, which is a very distinctive appellation, the anæmia being the most important result, and tending to the fatal issue. Moreover, it is a simple and good name for it, as he thinks it indicates the most important condition of the malady, and the one often only recognizable when the enlargement of the glands is entirely within. There is no excess of white corpuscles in this disease similar to that observed in the *leucocythæmia splenica* of Bennett, but rather a deficiency of the red, as was observed in the following case, for the notes of which we are indebted to Mr. Hugh Bennett, clinical clerk to the hospital. In this instance, the duration of the disease was three years and a half, and the extreme whiteness of the skin resembled the anæmia of females who have lost much blood. The anæmic bruit was also present.

William B., aged 27 years, was admitted, on June the 8th last, into Job ward. He was a single man, by occupation a gunmaker, and residing in Fleet-street; states that his health had been good up to three years and a half ago, excepting having had an attack of inflammation of the bowels fifteen years since, and three attacks of gonorrhœa four years ago; he had a chancre on penis, no sore throat, no bubo, nor eruption; six months afterwards had enlarged glands in the left groin, which have increased in size ever since; he was never of dissipated habits, and always kept good hours. His parents and brothers are healthy; no history of scrofula in the family. Three years and a half ago, he noticed a small lump in the left groin, accompanied with a slight pain. It was the pain, and not the lump which chiefly attracted his attention. The pain he described as dull and aching, descending the left thigh as low down as the knee, the thigh being at the same time slightly swollen, and also ascending obliquely backwards towards the small of his back. Twelve months ago he was an inmate of St. George's Hospital, under the care of Mr. Hawkins, who treated him with iodide of potassium and cod-liver oil internally, and tincture of iodine locally. He was presented by Mr. Hawkins, at the expiration of eight months, slightly relieved. He resumed his original occupation, and followed it for a short time, until he became so weak that he was compelled to give it up.

The patient is a man of middle stature, light complexion, grey eyes, and light hair; presenting all the appearances of having lost an abundance of blood—suffice it to say, however, that he has not lost any; skin of his body generally extremely white, hot, and pungent; conjunctivæ watery. Chest well formed, mobile during respiration, and resonant on percussion; equally resonant posteriorly. Lungs healthy. On listening to the sounds of the heart, there could be heard indistinctly a systolic bruit in the course of the aorta, ("anæmic bruit diagnosed.") He never had rheumatism or pain in his limbs. His voice is strong and clear; tongue moist and clean; has evidently an enlarged spleen, bulging out of the left hypochondriac region; it can be distinctly felt through the abdominal walls; has enlarged glands in the left groin, about the size of a goose's egg; no other glands perceptibly enlarged; has general anasarca; skin pits on pressure in every part of the body and extremities; urine healthy, specific gravity 1017; bowels open; motions of a clay colour, "pale."

Mr. Stocker, the apothecary, saw the patient on the 8th of June, and ordered the following medicine: two grains of iodide of potassium, in an ounce of julep of ammonia, three times a day.

June 9th. He had a good night. His blood was examined microscopically this morning, and was found to contain an excess of white corpuscles, "comparatively speaking;" but, in reality, there seemed to be a deficiency of the red corpuscles, rather than an excess of the white.

11th. Dr. Pavy prescribed five grains of the citrate of iron, with quinine, thrice a day.

13th. Has a severe headache this morning, and a troublesome cough is coming on, without any expectoration. To have five grains of extract of conium night and morning; also, five ounces of wine daily.

16th. Feels better since he has had the wine; cough much about the same. Ordered, lactate of iron, five grains; iodide of potassium, two grains; syrup of poppies, half a drachm; water, an ounce: three times a day.

18th. Expresses himself as being better; lower extremities still very œdematous; coughs a good deal at night.

July 4th. Thirst excessive; appetite lost; expectoration more abundant, of a bluish gray, slightly frothy character, and strongly adherent to the bottom of the utensil.

8th. The patient evidently seems much worse; lies prostrate in bed; is not able to sit up for five minutes together; mouth and tongue very dry, the latter being brown in the centre, and white along the margin.

9th. Had a very restless night; respiration became hurried; pulse quick and feeble; eyes turned upwards; mouth wide open, and dry. Ordered, eight ounces of wine; ammonia and serpentaria.

10th. Unconscious; lies on his back, with his head thrown backwards; pulse rapid, and extremely feeble.

11th. Expired at six, a. m. Died quietly.

Post-mortem examination thirty-three hours afterward.—On opening the thoracic cavity, it was found to contain a larger quantity of fluid than is usually met with in health, and an excess of fluid was also found in the pericardium. Lungs free from adhesions; patches of softening were here and there found on cutting into them. Liver, kidneys, and heart, healthy; the latter contained no clot, except a very small one in the left ventricle; blood being remarkably thin, like port wine and water mixed. Spleen enlarged; weighed twenty-four ounces and a half; full of white tubercles. Lumbar glands greatly enlarged; inguinal glands also enlarged.—*Lancet, August* 27, 1859, *p.* 213.

DISEASES OF THE URINARY ORGANS.

35.—ON EXCESS OR DEFICIENCY OF UREA IN THE URINE.

By Dr. Lionel Beale, F.R.S., Physician to King's College Hospital.

[When a specimen of urine is said to contain excess of urea, it is understood that on adding an equal bulk of nitric acid to it, when unconcentrated, a nitrate of urea forms, and crystallizes just as if the urine had been concentrated by evaporation.]

This result may be brought about in several ways. In cases in which but a small quantity of fluid is taken in proportion to the urea to be removed—when an unusually large amount of water escapes by the skin and other emunctories—and in cases in which an unusual amount of urea is formed in the organism, we shall frequently find excess of urea.

There is another class of cases in which the urine often contains this excess of urea; and it is difficult to offer a satisfactory explanation of the excess. The patient is weak, and grows thin, in spite of taking a considerable quantity of the most nutritious food. He feels languid, and indisposed to take active exercise. In some cases, digestion is impaired; in others, the patient eats well, experiences no pain or uneasiness after food, and perhaps has a good appetite. Sometimes there is lumbar pain. It would seem that much of the albuminous substances in the blood, instead of being applied to the nutrition of the tissues, becomes too rapidly converted into urea, and is excreted. The waste of the tissues is not properly repaired, and the patient gets very thin. To refer these symptoms to the existence of a particular diathesis, appears to me no explanation of the nature of the case. The pathology of these remarkable cases has not yet been satisfactorily investigated. Mineral acids, rest, shower-baths, and good air, often do good; but some of these patients are not in the least benefited by remedies, and they continue for years very thin, passing large quantities of highly concentrated urine, while the appetite remains good, and they digest a considerable quantity of nitrogenous foods. I am now trying, in one of these cases, which has resisted the usual plans of treatment. the effect of pepsine, with diminished quantity of meat, and a larger amount of farinaceous food. The condition often lasts for some years, and then the patient's health improves, and he gets quite well.

Dr. Golding Bird has drawn attention to the frequency of the occurrence of excess of urea with oxalate of lime. The quantity of oxalate of lime, however, is in all cases so very small that it is hardly possible to believe that the formation of this substance can be very important. We shall see that oxalate is one of the commonest urinary deposits; that it results from decomposition; that there is no reason for believing it to be indicative of any peculiar diathesis or habit of body. Excess of urea affords no explanation of the presence of oxalate of lime, nor this latter of urea. Each condition may exist without the other. *Cæteris paribus*, we should expect to find oxalate of lime most frequently present in specimens of highly concentrated urine.

Excess of urea is frequently found in the urine of persons suffering from acute febrile attacks. It is very common in cases of acute rheumatism, and is often met with in pneumonia and acute febrile conditions generally. In England, we meet with many of these cases; but, on the continent, they appear to be so rare that many authorities seem to doubt the truth of what English observers have stated with regard to this point. Lehmann, I think, states that he had not seen a case in which crystals of nitrate of urea were thrown down upon the addition of nitric acid, without previous concentration.

The amount of urea excreted is often very great. Vogel mentions a case of pyæmia in which 1235 grains of urea were removed in the course of twenty-four hours. Dr. Parkes obtained as much as 885 grains in a case of typhoid fever. These quantities are very great, if the patients were of the average weight of adult men; but, unfortunately, their weight was not recorded.

Urine containing excess of urea is generally perfectly clear, of rather a dark yellow colour, and of a strong urinous smell. Its specific gravity is about 1030, and it contains generally 50 or 60 grains, or more, of solid matter per 1000. At ordinary temperatures, an aqueous solution must contain at least 60 grains of urea per 1000, to form crystals of the nitrate upon the addition of nitric acid without previous evaporation; 50 grains of urea per 1000 hardly gave the slightest precipitate after the lapse of a considerable time. It would seem that the salts, extractive matters, &c., in urine, cause the crystalization of the nitrate when even a smaller quantity of urea is present. It should be mentioned, that the above experiments were performed in the summer, in very hot weather. In one case, in which the urea readily crystalized on the addition of the nitric acid, the urine had a specific gravity of 1028, and contained—

Analysis.

Water	940·18
Solid Matter	59·82
Organic Matter	50·57
Fixed Salts	9·25

Much has already been said with regard to the circumstances which cause a diminished quantity of urea in health. I may remark here, that Dr. E. Smith holds that tea and coffee excite respiration and increase the quantity of carbonic acid; that tea increases waste, and excites every function in the body; and that it is, therefore, injurious to those who are not well fed. These conclusions are at variance with the results of the laborious investigations of Dr. Böcker, who found that tea caused a diminution in the quantity of perspiration, urea, and fæces. He states that it does not influence the amount of carbonic acid formed, nor the frequency of the pulse or respiration; and that, when the diet was insufficient, tea prevented the loss of weight being so great as it would have been otherwise.

In chronic disease of the kidney, the urine is of very low specific gravity, and but a very small proportion of urea is excreted in the twenty-four hours. This arises from the alteration in the gland-structure, and the amount of urea separated may be regarded as a rough indication of the extent of the organ involved. In some cases, the morbid condition affects the whole structure; but in others the greater part of the kidney remains healthy. In the latter case, a fair amount of urea will be excreted; and, although the urine contains albumen, the case may be looked upon as a hopeful one.—*British Med. Journal, Oct.* 8, 1859, *p.* 809.

36.—CASES OF DIABETES.

By Dr. John M. Camplin, F.L.S.

[Before perusing the following cases, it would be better to refer to Dr. Camplin's description of the bran bread which he recommends, vol. xxxv., p. 303. *Case* 1.—August, 1858. Mrs. ——, aged 44, the mother of several children, rather a large woman, supposes that her complaint has been brought on by anxiety—sweats profusely; much debility. Sp. gr, of urine 1·040, liq. potassæ turning the sample to a claret colour. Almost eight pints were passed in twenty-four hours.]

When I first saw her the diabetic symptoms were rather aggravated than otherwise, the liq. potassæ rendering the urine extremely dark.

I prescribed

℞. Acid. sulph. dilut. ʒ iij.; liq. gent. compos. ℥ iiss. M. cap. coch. parv. ter die ex aquæ cyatho.

℞. Aloes. barb, ext. Saponis āā xxiv.; pulv. ipecac. gr. iv.; ext. nucis vom. gr, iij. M. et div. in pil. xij. cap. j. vel. plures h. s. ut opus f.; and enjoined a diet, of bran, cake, meat, and a free use of vegetables.

Aug. 31. Urine diminished to four pints, and now only brown, with liq. potass.; thirst nearly gone, and in every respect better.

Sept. 15. The sample of urine sent up for inspection contained scarcely a trace of sugar; the sp. gr. 1·016 (quantity not mentioned); and it was difficult to believe that it could be from the same person.

Nov. 1. I received a note stating that she had quite recovered; and on April 19 of this year her husband writes: "I am happy to say that Mrs. —— is now quite well; she has resumed her usual diet long since."

Case 3.—Dec. 1856. ——, Esq., upwards of 70, attributes his attack to late hours in Parliament, which had been "too much for him at his advanced time of life."

The general health of this patient was not so much lowered as might have been expected, the disease having existed for some months; but his son (an eminent surgeon) considered him now fast declining; he complained much of weakness of the limbs, and had a little puffiness on the tibia; had taken gallic acid and other remedies by the advice of his medical attendant in the country; and had also been put on a partially restricted diet; the quantity of urine was now about 80 oz., the sq. gr. not much above 1·030, but it contained a considerable quantity of sugar.

I prescribed

℞, Ammon. sesquicarb. ℈ij.; infus. aur. co ℥ viii.; m.f. cap. coch. ij. ter die; and directed a more rigid diet, with a little brandy and water instead of wine.

This prescription was occasionally varied, with ammonia as the general basis, and he took from time to time, small doses of pil. hydr. and pulv. ipecac. compos, and gradually recovered.

August 5, 1857. He writes, "I am glad to be able to say that for some months past I have been in excellent health, and my friends tell me I look as well as I ever did."

July 21, 1858. I had a note from his son, who says: "He has latterly been well in health, and when I last examined the urine, no sugar could be detected, but there was a large amount of lithates. He lives pretty much as usual, temperately always as regards alcoholic liquors, but does not eat much bread—the bran cake his servants managed very well, and he takes it occasionally: he has lately suffered the operation of reclination on a cataract in his right eye, and the result I am thankful to say seems likely to be satisfactory."

On a recent occasion, when I accidentally met this gentleman, I had the satisfaction of hearing that my patient, though in his 78th year and feeble, had not required any restriction in diet for eighteen months or more.

Case 3.—The Rev. Mr. ———, nearly 64. Sept. 1857.—The history of this case written by himself is exceedingly graphic; and only the fear of occupying too much of your valuable space prevents me from transcribing it at length. The disease had been slowly undermining his system several months, and having been in the habit for some years of resorting to a hydropathic establishment when out of health, he had recourse to that in the first instance. Contrary to what had been usual with him, instead of recovering strength, he continued to get weaker, and the nature of his disease not being ascertained, the proprietor considered him nervous, and recommended him to go to the seaside. After remaining there for some weeks, still losing ground, he came to consult an eminent physician in town, who discovered the nature of his case, prescribed for him, and put him on a partially restricted diet. When he placed himself under my care, he was somewhat better, but highly nervous and dyspeptic, the urine still abnormal in quantity, and containing sugar. I advised that the diet should be still further restricted —that he should have weak brandy and water instead of claret. Substituted ext. humuli at night for the opiate, as he was too wakeful to do without anything, and prescribed the following mixture:

℞. Magnes. carbon. ℈iv.; palv. acaciæ ʒij.; sp. ammon. arom. ʒij.; infus. gent. compos. ad ℥viii. m. f. mist.

I find this prescription occasionally varied, as Dec. 16.

℞. Potass. bicarbon. ʒiij. ℈j.; magnes. carbon. ℈iv.; pulv. acaciæ ʒij.; tinct, hyoscy. ʒi.; infus. calumbæ, ad ℥viii. m. f. mist cap. coch. ij. ter die.

At one time small doses of tinct. nucis vom. were ordered to the alkaline mixture, but soon laid aside again; he gradually recovered and resumed his clerical duties, which he had long been incapable of performing.

June, 1858. He writes:—"I am now gradually returning to ordinary diet. I substitute brown bread for the bran cake. I have now more fear of lithic acid, than of sugar."

March 23, 1859. "I have no return of diabetes, and am decidedly stouter, and more vigorous, Now take three glasses of port-wine daily, two at dinner, one mulled at night, or at 8 o'clock in the evening; brown bread and greens, meat three times a-day, but not much."

Case 4.—August, 1858. ——— Esq., banker, aged upward of 60, consulted Dr. Babington, who prescribed for him, and kindly recommended him to me for instructions as to diet. Dr. Babington's prescription was five grains of ammon. sesq. in infus. gent. c. three times a-day. Writing to me shortly after, this patient says:—"It is quite astonishing how little water I part with in comparison—it was immediate when I paid strict attention to diet."

I had not heard of this gentleman for many months—in answer to an inquiry as to his state, he writes, (April 23, 1859:)—"I do not make more than one quart per day; sometimes there is a little red sediment at the bottom. I sleep well, and eat well."

The above are specimens of a few cases, in which a return to the ordinary diet has been practicable: in the majority the disposition to the formation of sugar has appeared too strong to render this advisable: and I have thought it better for the patient to be satisfied with comparative health and comfort, with the diabetic diet, than to run the risk of a relapse, particularly where, from the distance and other circumstances, I could not examine the urine from time to time.

The remedies prescribed in the above cases were in the first acid, in the others alkaline, in each the indications seemed obvious, and the result was most satisfactory. In cases now under my care (especially five or six from 30 to 35 years of age,) I am testing the value of various remedies, and hope by classifying them, as much as practicable, to obtain definite results.—*Med. Times and Gazette, May* 28, 1859, *p.* 547.

37.—CHRONIC DIURESIS IN A MAN AGED FORTY; EMPLOYMENT OF BELLADONNA.

(Under the care of Dr. Willshire, Charing Cross Hospital.)

When a patient comes under the notice of the physician with a dry skin, great thirst, and extreme diuresis, a suspicion is entertained that the malady may be diabetes, and an examination of the urine is instituted to clear up the matter. If the specific gravity ranges from 1035 to 1045, the probability is that a large quantity of sugar is present, which will be determined in the usual manner. If, again, the specific gravity is very low, say 1002 or 1005, it may turn out to be an instance of what was formerly called diabetes insipidus, and now commonly known as *chronic diuresis* (the hydruria of Willis, and polyuria of Elliottson.) Not a trace of sugar is present in the urine in this affection, although it has been most rigorously searched for in the case which we now place upon record, wherein several gallons of urine were evaporated to a small bulk for the purpose of such inquiry. The similarity between diabetes and chronic diuresis in the large quantity of urine passed, and the presence of thirst and dry skin, renders an examination of that fluid necessary to diagnose between the two.

As regards the immediate locality involved in chronic diuresis, the views of Bowman, Golding Bird and others, would seem to place it in the Malpighian corpuscles, which thus secrete this large quantity of urine; and supposing there is no organic disease of the kidney itself, and that it is merely an exaggerated functional activity, and is allowed to go on, sooner or later organic mischief is sure to follow. It becomes incumbent, therefore, to arrest this morbid process, which in its reflex, or reactional effect on the system, (as evidenced by the constitutional symptoms of fever, thirst, general irritation, &c.,) will become of serious import to the patient. The little information we possess of this disease, and the feeble hold we have upon it by treatment, render it one of peculiar interest. In the case related by Dr. Watson, in his "Lectures on the practice of Physic," the duration of the disease was three years, and after death tubercles were found in the brain and lungs of the patient (a boy). It may last for a much longer period, however, as in the case quoted by Willis, of a man of fifty-five in the Hotel Dieu at Paris, who had been affected with it since the age of five years, and who had consumed daily since he was sixteen two bucketfuls of water, and discharged a commensurate quantity of urine. Like diabetes, diuresis is apt to end in phthisis; but it is a noteworthy fact that sometimes it is followed the former affection or mellituria.

A somewhat similar case to the subjoined was under Dr. Wiltshire's care in the same hospital about two years ago.

C. S——, aged forty, a hawker, was admitted on the 7th of June. He was in good health up to the 12th of May last, when he caught cold, and suffered thirst so that he drank from four pints to a gallon of water daily, besides beer and tea. He soon began to void from ten to twelve pints of urine in the twenty-four hours; and although his thirst was great, his appetite was bad. He was unable to sleep at night, being obliged to rise half a dozen times to micturate. When admitted, he passed fifteen pints of urine per diem, of a very pale straw color, quite clear, specific gravity 1002, and free from sugar by the ordinary tests. From that time to the 27th June, the treatment consisted of warm baths, Dover's powder, opium, tincture of the sesquichloride of iron, and quinine. By this, the urine was reduced to seven pints daily, with the specific gravity varying from 1001 to 1007, and containing no albumen. On the 27th, he was ordered a sixth of a grain of extract of belladonna, with two grains of quinine, three times a day. He now passed, in the twenty-four hours, six pints and a half of urine, of specific gravity 1006.

From the 30th June to the 7th of July the belladonna and quinine were continued, the former being gradually increased, until some symptoms of atropism made their appearance, when the dose was diminished. He now passed eight pints of urine during the day, of specific gravity 1002. He stated, however, that he felt much better; his mouth was not so dry, and he was not so thirsty. His bowels always remained costive. He went out on the 14th ultimo, when the specific gravity of his urine was 1006.

During the time of his stay in the hospital, the weather being very warm, he was ordered to walk in the sun to induce perspiration. The reason for giving belladonna was, that as it is one of the best remedies for allaying the irritability of the lower urinary organs, it might have some influence on the higher organs,—namely the kidneys themselves. While the patient was in hospital, six gallons of his urine were collected for analysis by Mr. R. V. Tuson, the teacher of chemistry, who had some idea that a *minute* quantity of sugar might perhaps exist in such cases as the present. This quantity was evaporated down to a very small bulk, filtered, and most carefully and repeatedly tested for sugar, both before and after boiling, with dilute sulphuric acid, but no indication of the presence of saccharine matter was obtained. Another portion of fresh urine was allowed to stand in a warm place, in order that torulæ might become developed if any sugar were present, but none was observed.—*Lancet, Sept.* 3, 1859, *p.* 237.

38.—*On the Employment of Tannin in large doses in Albuminous Anasarca.* By Dr. P. Garnier.—Although the internal use of tannic acid is still very limited in France, its employment in large doses has been much recommended lately in other countries, and has been extended to numerous cases which, while proving its innoxious character, appear to exhibit it as possessing some totally new properties. It has been shown to be useful in all cases where it is required to arrest hemorrhages, to give tone to the organism, or to remedy morbid secretions. It has been employed, for example, with great benefit in albuminuria, diabetes, and serous infiltrations.

From these considerations, Dr. Garnier has been induced to employ tannic acid in the albuminous anasarca consecutive to scarlatina; and he adduces several cases illustrative of this mode of treatment, drawn from his own experience and from cases recorded by other physicians. The cases all prove that in the general serous infiltration of the tissues complicated with albuminous urine, there is a rapid and simultaneous disappearance of these two morbid phenomena under the influence of tannin alone, administered in a large dose. The conclusions drawn by Dr. Garnier are that tannin, employed in doses of two to four grammes a day (ʒ ss to ʒ j), cures anasarca or œdema developed passively and occurring simultaneously with albuminous urine; that its curative action is manifested by abundant urine, gradually resuming its physiological characters, by perspiration, easy alvine evacuations, return of appetite, &c.; that these signs appear from the second day of the administration of tannin; that given in solution in doses of twenty to fifty centigrammes at a time, tannin causes no unfavorable symptoms affecting the digestive passages; and lastly, that the action of tannin appears to be exerted primarily upon the fluids of the economy, the albuminous principles of which it coagulates and renders plastic, and that its consecutive action on the solids appears to be tonic and astringent.—*Brit. and For. Med.-Chir. Review, July,* 1859, *p.* 241.

39.—ON MORBUS ADDISONII.

By Dr. Samuel Wilks, Assistant Physician to Guy's Hospital.

[After detailing the last example of this interesting affection, which has occurred at Guy's Hospital, speaking of the scepticism which has prevailed as to the conclusions arrived at by Dr. Addison, as to the fatality and etiology of the disease, Dr. Wilks remarks:]

It has been said that supra-renal disease has been discovered independently of any alteration in the colour of the skin, also that a pigmental discoloration may constantly occur without any morbid affections of these organs; moreover, that they may be found diseased when the patient has died of some acute malady; and even a fourth argument is added, that they may be removed from the lower animals with impunity.

Before considering these objections, let us for a moment look at the original statements put forth by the author. And first of all remember, that the discoloration of the skin, although a striking feature of the complaint, was not the main one insisted on by him; but since it is that which can be pourtrayed in a drawing, and consequently apt to strike the eye on turning over the pages of the monograph, it is not surprising that it was at once regarded as the most remarkable part of the complaint, and would therefore be especially dwelt upon in our ordinary mode of communicating facts to one another, until at last the erroneous opinion would be reached that Addison's disease and discoloration of the skin were interchangeable terms. Now, it is stated by the author himself that he was led to the discovery by a very different method than by studying the changes in the skin; that it was owing to a peculiar interest which he took in the class of cases styled simple and fatal anæmia that this allied affection came under notice, and that it was whilst watching such cases the fact of discoloration was also observed, and subsequently on post-mortem examination its connection with disease of the supra-renal capsules. The extreme prostration of all muscular power was the most remarkable circumstance attending these cases, and indeed in some forms of supra-renal affection no discoloration had occurred at all. This was the case in the instance of a man who died in the hospital about two years ago, in whom there was no discoloration of the skin, the only symptom being the most utter prostration of strength, and yet after death these organs were found completely diseased. The most prominent symptoms, then, of the affection are due to this asthenic condition, denoted by the loss of muscular power, weakness of pulse, breathlessness upon exertion, dimness of sight, weakness of stomach, &c.; and if the case has been of long duration, in addition to these, a discoloration of the skin. A sufficient number of cases have now been observed to prove that the change in the skin depends on the chronicity of the disease. This is Dr. Addison's opinion, and the fact has been verified by Mr. Hutchinson, who has taken the trouble to collect all the cases recorded; and therefore it may be said that in the acute form of the disease no discoloration of the skin may be expected, but merely symptoms due to asthenia.

As regards the character of the colour when it occurs, the observation of several instances since the publication of the original memoir has shown that very great similarity has existed in all of them, both as regards the hue itself and its method of affecting the body by a uniform implication of the whole surface. It is true that a case is represented where the body is covered with patches of colour, checkered with white; but here no post-mortem examination took place to verify the diagnosis, and thus the fact is left as we state, that in the first-related cases, as well as in all subsequent ones, there has existed a uniform discoloration of the whole integument. So remarkable has this been, that the body has presented the appearance of a person with dark blood rather than that of a European, and therefore the answer which Dr. Addison continually makes to interrogatories respecting the peculiarity of the discoloration is, that he regards only those cases as characteristic where the surface of the body is seen to be gradually approaching in colour that of an inhabitant of some southern nation. The similarity is proved by examination of the integument itself, which, if placed beneath the microscope, is seen to contain a layer of pigment in the rete mucosum just as in the dark races of mankind; so that no difference, as far as we are aware, can be found between them. Moreover, as in the latter, certain parts, as the axillæ and pubes, are darker than the rest of the body, so in this morbid discoloration these same parts are those most affected. Without, therefore, denying that the colour may sometimes occur in patches, we think we are correct in saying that all experience has hitherto shown that the discoloration has been uniform over the whole surface of the body. The exact hue is difficult to describe, but it may be said to resemble that of a mulatto's skin, and therefore is of a brownish cast, having sometimes an olive-green tinge, and thus the term bronzing of the skin has come into use, or it has often what our artist calls a walnut-juice shade. To say, however, in short, that the appearance produced is exactly that of a person of dark blood, is to speak as accurately as description will allow. The anatomical characters are probably not very peculiar, and resemble those of many other pigmental changes, arising from various causes, since the true seat of colouring matter is in the cells of the rete mucosum; this is its position in dark races, and also when produced by morbid conditions. An examination of a portion of skin, therefore, would not determine the nature of the case; although, if the colour should be universal, there would be a strong suspicion of supra-renal disease. It is well, however, to remember that the pigment is situated beneath the epidermis and thus is distinguishable from pityriasis, and many other conditions with which Addison's disease has been confounded. It would seem scarcely needful to say that it is impossible to confound it with jaundice, had the mistake not been often made; indeed, in nearly all instances of the affection which have come before us, an hepatic disease has been the diagnosis; but a moment's observation would be sufficient to show that no bile is circulating through the body—the urine contains none, nor is the conjunctiva yellow, which is the first part affected in jaundice, but here, on the contrary, is remarkably pale and anæmic. It is true, however, that in jaundice the yellowness of the skin is due to a deposition of biliary pigment in the rete mucosum. A remarkable circumstance observed by Mr. Hutchinson in connexion with the discoloration is that in some instances the surface of the body has evolved a peculiar odour, resembling that perceived in dark races of mankind.

The symptoms, then, of Addison's disease are characterised by those of asthenia, and in chronic cases by discoloration of the skin. And now as regards the post-mortem appearances. These may be exclusively confined to the supra-renal capsules, and the disease affecting them is of a remarkably uniform character, differing merely according to the stage of the affection. The disease, without doubt, is closely allied to tubercle, since in several instances tubercles have been met with in the lungs and other parts of the body, or a scrofulous material in some lymphatic glands, closely resembling the deposit in the supra-renal bodies. When the disease is acute the organ is somewhat enlarged, and changed into a material which is semi-translucent, of a gray colour, softish, homogeneous, without structure, or sometimes slightly fibrillated, or contains a few abortive nuclei or cells. This lardaceous material is the first deposited, and resembles what is often seen in the early stage of scrofulous enlargement of lymphatic glands. Subsequently it undergoes a decay or degeneration, as in them, and changes into an opaque, yellowish substance, and thus the two materials are constantly found associated. At a still later period, as in a scrofulous gland, it may soften into a putty-like matter, or it may dry up, leaving the mineral part as a chalky deposit, scattered through the organs. These, then, are the changes—first the deposition of a translucent, softish, homogeneous, substance; subsequently, the degeneration of this into a yellowish-white, opaque matter; and afterwards a softening or drying up into a chalky mass. It is clear, therefore, that as the discoloration of the skin occurs only in very chronic cases, that it must have been found always associated with those later stages of the supra-renal disease, and that when the skin has been not altered in colour, the earlier conditions have been met with; and this has been the case also in those instances where associated with general tuberculosis. Occasionally, also, some fibrous tissue may be found around the organ, being the products of an inflammation which has united them to the kidney, liver, and adjacent parts. It is not sufficiently remembered that some years are necessary for the changes to occur in the deposit of which we have spoken; and therefore, if chalky substance be found, there can be no doubt that the disease has existed for a considerable period. This corresponds with our knowledge of the duration of the symptoms in some of the best-marked cases which have come before us, and should compel us to keep under notice suspected instances of the disease for several years.

Now, to consider the objections which have been raised as to the correctness of these observations. It has been said, in the first place, that animals have survived long after removal of the supra-renal bodies; now, should this be so (although further experiments are required to corroborate the statement,) it would not militate against the fact that disease of these organs will sooner or later, lead to a fatal result, since similar arguments might be used with reference to other organs which can be removed with impunity, and yet, when subject to morbid conditions, lead to fatal consequences. For example, the spleen is said to be capable of removal without injury to the animal, and this can be well believed, seeing how extremely atrophied it often becomes in old people; and yet we know, at the same time, that when hypertrophied it is sufficient to kill. Moreover, as just now remarked, it does not seem sufficiently remembered that the disease of these organs is naturally slow, and that in all probability they are almost totally disorganised before death takes place; that, owing to some unknown cause whereby their function may be otherwise performed, or be for a time in abeyance, the patient may probably live for years, and then at last die suddenly, in a somewhat analogous way as degeneration of the kidneys may persist for many years, although the patient is always on the brink of a precipice, and ready on any day to die off unexpectedly; but in such a case the importance of the disease might be doubted if the patient had died from other cause, and the renal affection been met with accidently.

As regards the objection that the discoloration offers no anatomical peculiarity, since the pigment is merely deposited in its usual position, it may be answered that this is true with respect to this one fact, but not true as to the mode in which the body is affected, since we are not aware that at present any case has been brought forward where the whole cutaneous surface has been discoloured so as to resemble the mulatto in the manner mentioned other than that of supra-renal disease. Partial discolorations, as in ephelis, are common enough.

With respect to a third objection, that disease of these bodies may exist without discoloration, this has already been explained as a point admitted when the disease is acute.

Then as regards the accidental discovery of supra-renal disease in the body after death, when not suspected: this, we think, has no greater importance than the discovery of disease of other organs when not anticipated. If found alone it would be sufficient to indicate the mortal nature of the malady, and if associated with disease of other organs it would be difficult to say to which to attribute the symptoms; thus, a general tuberculosis, involving a large number of important parts of the body, may include the supra-renal organs as well, and in such a case there may have been no distinctive marks of the existence of their morbid condition during life, but then the same may be said with respect to all the other organs affected. Again, even should an acute disease carry off the patient, and disease of these organs be found, still (in our present limited knowledge of the subject) it cannot be said that the one affection did not predispose to the other, since in one of Dr. Addison's best-marked cases the patient died of an acute pericarditis.

All these objections which have been raised we consider to be of little value compared with the great facts which remain—that at the present time several cases are recorded where persons have died with no other apparent disease in their body than that of the supra-renal bodies, sometimes associated with discoloration of the skin and sometimes without; facts which show incontrovertibly that their disease is connected directly or indirectly with the death of the patient. Moreover, we cannot bring our mind to conceive how, when in several of such cases a right explanation has been given as to the cause of the symptoms during months or years prior to the patient's disease, this diagnosis was due to a fortunate guess rather than to a rigid scientific deduction.

For the satisfaction of those of our readers who have not had an opportunity of perusing the original monograph, we extract the following important passages from Dr. Addison's work:

"For a long period I have from time to time met with a very remarkable form of general anæmia, occurring without any discoverable cause whatever; cases in which there had been no previous loss of blood, no exhausting diarrhœa, no chlorosis, no purpura, no renal, splenic miasmata, glandular, strumous, or malignant disease. Accordingly, in speaking of this form of anæmia in clinical lecture, I, perhaps with little propriety, applied to it the term *idiopathic*, to distinguish it from cases in which there existed more or less evidence of some of the usual causes or concomitants of the anæmic state."

"It was whilst seeking in vain to throw some additional light upon this form of anæmia, that I stumbled upon the curious facts which it is my more immediate object now to make known to the profession."

"The leading and characteristic features of the morbid state to which I would direct attention are anæmia, general languor and debility, remarkable feebleness of the heart's action, irritability of the stomach, and a peculiar change of colour in the skin occurring in connexion with a diseased condition of the supra-renal capsules."

"The patient becomes languid, weak, indisposed to either bodily or mental exertion, the appetite is impaired or entirely lost, the whites of the eyes become pearly, the pulse small and feeble, or perhaps somewhat large, but excessively soft and compressible; the body wastes, without, however, presenting the dry and shrivelled skin and extreme emaciation usually attendant on protracted malignant disease; slight pain or uneasiness is from time to time referred to the region of the stomach, and there is occasionally actually vomiting, which in one instance was both urgent and distressing, and it is by no means uncommon for the patient to manifest indications of disturbed cerebral circulation. Notwithstanding these unequivocal signs of feeble circulation, anæmia, and general prostration, neither the most diligent inquiry nor the most careful physical examination tends to throw the slightest gleam of light upon the precise nature of the patient's malady, nor do we succeed in fixing upon any special lesion as the cause of this gradual and extraordinary constitutional change."

In cases of supra-renal disease—

"With more or less of these symptoms we discover a most remarkable, and as far as I know, characteristic discoloration, taking place in the skin; sufficiently marked, indeed, as generally to have attracted the attention of the patient himself or the patient's friends. The discoloration pervades the whole surface of the body, but is commonly most strongly manifested on the face, neck, superior extremities, penis and scrotum, and in the flextures fo the axillæ and around the navel. It may be said to present a dingy or smoky appearance, or various shades of deep amber or chestnut-brown, and in one instance the skin was so universally and so deeply darkened that, but for the features, the patient might have been mistaken for a mulatto. In some cases this discoloration occurs in patches, or perhaps rather, certain parts are so much darker than others as to impart to the surface a mottled or somewhat chequered appearance."—*Guy's Hosp. Reports*, *Oct.*, 1859, *p.* 93.

SURGERY.

AFFECTIONS OF THE BONES AND JOINTS, AMPUTATIONS, &c.

40.—ON RESECTION OF THE ANKLE-JOINT.

By Henry Hancock, Esq., Senior Surgeon to the Charing Cross Hospital.

[In that large and important class of cases wherein disease is restricted to the ankle-joint, or its immediate neighbourhood, the rest of the foot being healthy, for the most part Syme's or Pirogoff's operations are resorted to, the otherwise sound and healthy foot being sacrificed. But in reality these operations are comparatively rarely called for. Only in those cases are they really necessary where there is so large an amount of disease or mischief that all hope of preserving a good and useful foot is precluded.]

The operation which I would advise you to select, in the cases under consideration, is that of Resection or Excision of the Ankle-joint.

The operation was first performed by Moreau, and subsequently by Jäger and others, abroad; but I believe I am justified in stating that, with the exception of those which I have done myself, there is not a single instance upon record in which excision of the ankle-joint has been performed in this country for disease. Why, in this age of conservative surgery and joint resection, the solitary exception should be made in the case of the ankle-joint, and so useful a member as the foot needlessly sacrificed, is an anomaly which I confess I do not understand. We have seen that, in Syme's operations, independently of the entire loss of the foot, there is danger of sloughing or bagging the matter; and in both Syme's and Pirogoff's operations, inflammation, sloughing, and suppuration in the course of the divided tendons. In excisions of the ankle-joint these dangers do not exist. I have now performed this operation four times—three times successfully, once unsuccessfully, the patient dying some six months after the operation, from long disease, the result of a dissipated life. In no instance has there been sloughing; there need not be a single tendon artery divided; there is, afterwards, very little if any deformity—comparatively little shortening; the foot is preserved, and, as you will see by the cases I here relate, the patients are able to walk and run about with scarcely any perceptible limp.

You may bear in mind, however, that the success of the operation depends upon leaving the anterior and posterior tibial arteries intact. If these vessels are injured, there will not be sufficient blood supplied to nourish the part, or power to heal the wound; sloughing will ensue, and your operation fail. Your object also should be, to preserve everything as intact as possible, and on no account to open into the sheaths of the tendons.

The plan I have found to answer best is the following:—Commence the incision about two inches above and behind the external malleolus, and carry it across the instep to about two inches above and behind the internal malleolus. Take care that this incision merely divides the skin, and does not penetrate beyond the fascia. Reflect the flap so made, and next cut down upon the external malleolus, carrying your knife close to the edge of the bone, both behind and below the process; dislodge the peronei tendons, and divide the external lateral ligaments of the joint. Having done this, with the bone nippers cut through the fibula, about an inch above the malleolus; remove this piece of bone, dividing the inferior tibio-fibular ligament, and then turn the leg and foot on the outside. Now carefully dissect the tendons of the tibialis posticus and flexor communis digitorum from behind the internal malleolus. Carry your knife close round the edge of this process, and detach the internal lateral ligament; then, grasping the heel with one hand, and the front of the foot with the other, forcibly turn the sole of the foot downwards, by which the lower end of the tibia is dislocated and protruded through the wound. This done, remove the diseased end of the tibia with the common amputating saw, and afterwards, with a small metacarpal saw placed upon the back of the upper articulating process of the astragalus, between that process and the tendo-Achillis, remove the former by cutting from behind forward. Replace the parts *in sitú;* close the wound carefully on the inner side and front of the ankle, but leave the outside open, that there may be a free exit for discharge; apply water dressing, place the limb on its outer side on a splint, and the operation is completed.

You observe that the only parts cut through are the skin, the external and internal lateral ligaments, and the bone. Neither the extensor nor flexor tendons, the anterior nor posterior tibial arteries, are injured; consequently you have not to tie any vessels. The patient should be placed in bed, with the leg lying on its outer side; and you should be careful that there are openings in the splint-pad and oil-silk corresponding to the wound, otherwise the pressure of the pad causes the matter to be retained, and will, as I have seen it do, give rise to severe constitutional disturbance.

Case 1.—*Resection of ankle-joint; cure.*—J. H., aged eight years, of strumous diathesis, admitted January 30th, 1851, into Charing-cross Hospital, under my care, with disease of the ankle-joint. He had been in a delicate state of health for some time, and about three years before his admission a boy threw a stone, striking him on the left instep, from which period the joint became affected. On admission, the part presented a glossy, shapeless appearance; hot, and extremely painful on the least movement. It would admit of little, if any flexion, the child being quite unable to bring his heel to the ground. There were two fistulous openings anteriorly to the external malleolus, through which a tube could readily be passed into the joint. Shortly after his admission he was attacked with scarlatina, from which he soon recovered; the joint, however, becoming more painful, and the sinuses discharging thin offensive matter. It was deemed advisable to excise the ankle-joint in this case, the disease appearing to be confined to that part, and I accordingly performed the operation on Feb. 17th, 1851. There was some pain for the next three or four days, caused principally by inflammation of the absorbents of the leg and thigh. The inflammation, however, completely subsided in the course of a week. From this time he continued to improve until he left the hospital in the following May, cured.

I saw this boy about two years ago; he had become tall and stout, and he told me he could walk, run, and jump without any inconvenience. He wore a thick sole to his boot, and there was scarcely any perceptible limp in his walk.

Case 2.—*Excision of the ankle-joint; death in six or seven months.*—M. A. G., admitted under my care into Charing-cross Hospital in September, 1857. About four years previously a swelling, not preceded nor accompanied by pain, commenced in front of the ankle-joint. A wound soon after appeared behind the external malleolus, which remained open and discharging for about a month, when it healed, and she felt nothing more of it for about eighteen months, when the joint again became swollen and very painful; but she continued to walk about until fourteen months before admission, when the symptoms became so aggravated, that she could not walk at all, whilst for the last five months she could not put her foot to the ground.

When admitted, the joint was much swollen; there were three openings in front of the internal malleolus, which communicated with the joint. On October 5th I excised the ankle-joint. She did not suffer much afterward, but at the same time she did not progress so rapidly as the other cases have done. She became impatient, and left the hospital without my consent before the part had healed, and I subsequently learned that she died of lung disease some six or seven months after the operation.

Case 3.—*Excision of the ankle-joint.*—W. R., aged twenty-five, admitted under my care into Charing-cross Hospital on August 27th, 1857, with disease of the ankle-joint. When three years old he slipped off the pavement, and sprained his ankle, which became much swollen and very painful. Matter formed, which was let out with temporary relief; but he shortly became worse, and was taken to the late Sir Astley Cooper, who advised amputation of the limb; but his friends objected, and Sir Astley ordered him a wooden leg, to wear from the knee. This he did for six months, but without benefit. Having friends in Killarney he was sent there for change of air, and was attended by a medical man, who, he says, "worked wonders" with him. His treatment was peculiar. He ordered off the wooden leg, and then desired him to put his foot to the ground, and walk in the meadows barefooted every morning, whilst the dew was on the ground. At night he gave him oil to be rubbed into the ankle before the fire. At the end of six months he had improved so much, that he could walk without the assistance of a stick. The joint continued to get stronger every year, but remained stiff. In June, 1855, he came under my care with an ulcer on the side of the ankle. This healed in a fortnight, but the stiffness remained. He was now persuaded to place himself under the care of a quack, who pronounced the ankle to be out of joint. After oiling it well for three weeks with neat's-foot oil, he (according to the patient's statement,) "snapped the bone into its place;" after which he lost the stiffness, and could walk "as well as he could wish." Eight months after this he slipped again, and again consulted the quack, who, he says, "put the bone in a second time." Again, a third time he fell, and a third time he consulted his friend, who this time failed to accomplish his object, in consequence, as he said, of the "nerve resisting him so much." The ankle now began to swell and inflame; matter formed, and he consulted Mr. Tucker, who kindly sent him to me. I need scarcely tell you that when I attended him for the ulcer, there was no dislocation present. "The snapping and putting in the bone" was, doubtless, the disruption of adhesions, and, in all probability, caused the aggravation of mischief necessitating the operation.

At his admission his ankle was much swollen, inflamed, and very painful; there was an open wound over the external malleolus, communicating with diseased bone, and discharging offensive matter. Soon after, the inner side became swollen and intensely painful. An incision was made to relieve the tension, and afforded much ease. He, however, gradually became worse, his health began to give way, and, therefore, on Sept. 10th, I excised the joint. The tibia, fibula, and astragalus were all found diseased. He went on very well. He experienced some little drawback from a small spicula of bone coming away, but he left the hospital cured.

This patient who is an exceedingly ingenious fellow, has invented a boot, which appears to answer admirably. In the description which he has sent to me, he says, "Finding the cork sole did not give me any spring, I began to consider what I could substitute for the cork, so as to give me greater facility in walking. The result of my meditations was, a steel spiral spring, fixed in the heel of the boot, the cork, of course, being cut away. Then I have the cork sole down to the toes. On the top of the spiral spring is a circular piece of thick leather, and I derive great benefit from the invention."

He also wears the usual side irons for weak ankles. I met him the other day walking down Hampstead Hill, and he certainly showed no signs of having undergone so serious an operation.

Case 4.—*Excision of the ankle-joint and removal of a considerable portion of the os calcis, for disease.*—J. T., aged six years, residing at Hounslow, was sent to Charing-cross Hospital, under my care, in Sept., 1858, by my friend, Dr. Chapman, having for the previous four or five months suffered pain in the left ankle. On the 26th of Aug. he leaped from the top of a wall five feet high, and so hurt the joint that he had to be carried into the house. Considerable swelling and suppuration ensued, and, when admitted, he was very ill, weak, and feverish, his countenance being anxious and indicative of a t suffering. There was an unhealthy looking wound in front of the internal malleolus, discharging a large quantity of offensive matter, and a probe readily penetrated the joint, which was found extensively diseased. His friends having consented, I excised the joint on the 9th of October, in the manner I have described to you; but after removing the upper portion of the astragalus, which was carious, I found the disease extended through and beneath this bone, involving the os calcis to a considerable extent. The convex tibial articular surface, and the whole of the body of the astragalus behind the interosseous calcaneo-astragaloid ligament, were removed, as well as the remains of the corresponding articular surface on the upper part of the os calcis; whilst the interior of that bone, behind the interosseous notch was carefully gouged out before the disease could be got rid of: so that little more than the shell of bone remained in that situation. No arteries were tied. The boy suffered very little constitutional disturbance, his general health and appetite having been uniformly good, and his progress satisfactory.

March 10th, 1859. He is now cured, he can stand upon his foot and walk without pain. The wound is entirely healed.

This case shows in a very marked manner the value of the operation. Had Pirogoff's method been employed, it would inevitably have failed, from the condition of the os calcis. It also shows how extremely valuable is conservative surgery—in young people, at all events. The amount of disease and the state of the os calcis almost made me doubt the result; but yet we find the child going through the cure with scarcely any constitutional disturbance, the cavity in the os calcis filling up and becoming sound, and a perfect cure taking place in five months from the time of operation.

With these cases before you, I feel quite justified in advising you to adopt this method wherever you are called upon to operate, provided the disease appears confined to the ankle-joint. In gun-shot wounds and other injuries, your proceedings must be guided by the extent of implication of the soft parts.—*Lancet, Oct.* 1, 1859, *p.* 331.

41.—ON DISEASES OF THE TARSUS.

By John Ericbson, Esq., Surgeon to University College Hospital.

In no region of the body have the good effects of modern conservative surgery been more distinctly shown than in the tarsus. In the "good old times" of surgery, if a person had a white swelling of the bones of the foot, or a diseased tarsus, he was at once condemned to amputation of the limb. It was enough for a patient to have "disease of the tarsus" for him to have his limb removed; no distinction being made between disease of the different parts of the foot, nor any attempt to save the sound by the sacrifice of the diseased part.

Until a comparatively recent period, indeed, "diseased tarsus" was described as a whole. Surgeons did not endeavour to make out the exact extent and amount of the disease, and any case described as "diseased tarsus" was looked upon as requiring amputation of the leg. The rule of practice then observed was, amongst the wealthier classes—those who could afford the expense of a "cork leg"—to amputate a little above the ankle; but amongst the poorer classes, to remove the leg about a couple of inches below the knee, so as to give the patient a stump which, when bent, would fit into the socket of a wooden pin. Thus, in the latter case especially, not only was the leg, itself perfectly sound, sacrificed, but the patient was exposed to great additional danger; for if there be one point more than another which has been indisputably proved by surgical statistics, it is, that the mortality after amputation increases, *cœteris paribus*, in exact proportion as we approach the trunk, every additional inch which we remove augmenting the danger to the patient. This practice continued to prevail until M. Chopart drew some distinctions between the treatment to be pursued, according as the disease affected the anterior or the posterior tarsal bones and articulations. He showed that when the anterior articulations only were affected, amputation at the junction of the astragalus and calcaneum with the schaphoid and cuboid—an operation which goes by the name of "Chopart's amputation"—ought to be performed; thus removing the whole of the disease, and the patient recovering with a shortened foot, but, the heel being preserved, one on which he could bear the weight of his body, and which would be highly useful to him.

The next step in the conservative surgery of the lower extremity, in cases of diseased foot, was the operation introduced by Mr. Syme,—that of disarticulation at the ankle joint. This was certainly a great advance, for the flap being taken from the heel, the patient has a stump on which he can bear. The operation is also a very safe one. I do not know the precise statistics of all recorded cases; but this I know, that I have performed it nine times without a death, and this, in the lower extremity, is extremely satisfactory.

Since the introduction of anæsthetic agents, conservative surgery has taken great strides, and I think you may look upon conservatism in surgery as the necessary result of anæsthesia. For although operations of this kind were performed years ago by the Moreaus, Park, and others, and their utility demonstrated, yet the operations of gougings, scrapings, and partial resections were so horribly painful to the patient, and occupied so much time in their performance, that surgeons dreaded to undertake them. Of late years surgeons have learned to discriminate disease of one part of the tarsus from another, and to apply a different, but appropriate, treatment to each.

Looking at the subject in a diagnostic point of view—and the treatment is most intimately connected with the diagnosis—we find that the pathology of diseases of the tarsus is closely connected with its healthy anatomy. Composed, as it is, of seven bones, it presents four distinct articulations. By the term "articulation," applied to the tarsus, I do not mean merely the connection of contiguous bones with each other, but distinct synovial sacs shut off from communication with other synovial sacs in the foot.

The *posterior calcaneo-astragaloid* is the first of these; next comes the *anterior calcaneo-astragaloid*, the synovial membrane here serving also for the *astragalo-scaphoid*; the *calcaneo-cuboid* is the third; and the *anterior tarsal synovial membrane* is the fourth and largest of all, and the most important in a surgical aspect. It extends between the scaphoid and the three cuneiform and cuboid bones, between the cuneiform bones themselves, between the two outer cuneiforms, and the bases of the second and third metatarsal bones, and also between the external cuneiform and the cuboid.

In the vast majority of cases, so far as my experience goes, it is the osseous structures, and not the articulations, which are primarily diseased. The bones, being cancellous, far removed from the centre of circulation, and exposed to alternations of temperature, readily become the seat of congestion, and caries, rarely, however, of necrosis; and in strumous subjects not unfrequently fall into a tuberculous condition. Caries, whether simple or tuberculous, once set up in the bones, speedily implicates the articulations secondarily.

Now you can easily conceive, on casting an eye on the arrangement of the tarsal synovial membranes, that the extent of disease will, in a great measure, depend upon its seat. Thus, a person may have disease in the os calcis, extending even to the cuboid, with very little likelihood of its proceeding farther for a length of time. Such disease will be limited to the outer part of the foot, does not involve its integrity, and readily admits of removal by operation. But let him have disease springing up in the scaphoid, or in one of the cuneiform bones, or in the bases of the second or third metatarsal bones, then the morbid action will rapidly spread through the whole of the anterior and inner part of the tarsus, and, in all probability, no resection operation can be advantageously employed. So that the seat of disease influences materially its amount, extent, and the kind of operation required for its removal.

Let us now consider the various bones of the tarsus separately, as primary centres of disease.

The *os calcis* is diseased more frequently than any other bone of the foot, being, from its exposed situation, liable to injuries of all kinds, receiving the weight of the body when alighting on the feet in jumping, and having strong muscles inserted into it. Caries is the disease usually attacking the calcaneum; necrosis very seldom, although we sometimes find a piece of necrosed bone in the centre of a carious cavity. When this bone is diseased, the posterior part of the foot is swollen, and perforated by one or several fistulous openings, through which a probe passes down to, and sinks into, carious bone. On further examination, we find that the rest of the foot is healthy. Having thus limited the disease to the os calcis, what course is open to us in the way of curing the patient of the disease? Why, we may, of course, lay open the sinuses freely by means of a T-shaped incision, and gouge away the diseased osseous structures. This may always be done with success, however extensively the cancellous structure of the bone is involved, provided an external sound shell exists. You have often seen a little girl, who now occasionally attends here amongst the out-patients, upon whom I performed this operation twice, the disease having recurred after the first gouging. So much of the calcaneum was taken away in that case, that a mere shell of bone only remained; and yet the removed bone has been replaced by fibroid tissue, which will, in time, no doubt, ossify. She has a perfectly useful foot, and the only sign of any operation having been performed is a small, depressed cicatrix on the outer side. Indeed, where you have disease limited to the os calcis, such an operation as I have just mentioned will generally be attended with an excellent result, and it is but very seldom indeed that complete excision will be required. But in some cases you will find that the morbid action originating in the os calcis has not only involved the whole bone, but has extended somewhat beyond it, implicating the calcaneo-astragaloid, or the calcaneo-cuboid articulations, or both. Then you must proceed as I did in the case of a girl who was in the hospital last summer—namely, perform complete excision of the whole os calcis, and gouge away any diseased bone that may be met within the astragalus or cuboid. The girl made an excellent recovery; the heel continues somewhat flattened, it is true, but she has a sound and perfectly useful foot.

The *astragalus* is situated in a position of great surgical importance. Articulating, as it does with the malleolar arch above, with the calcaneum below, and with the scaphoid in front—forming, as it were, the key-stone of the foot—it is perfectly evident that any morbid action commencing in it is very likely to spread to and invole all the more important structures of th foot. Seldom, indeed, does disease originating here long remain confined to this bone; and, so far as my experience goes, gouging operations, even if performed at an early period, are rarely of much benefit, the morbid action continuing to extend notwithstanding their employment. Indeed, in diseased astragalus, I believe that excision ought, as a rule, to be practised in preference to gouging, contrary to what is the case in the calcaneum. In these cases you find swelling just in front of the malleolar arch, with fistulous openings leading down to the diseased astragalus; the anterior part of the foot and the heel being quite sound. You may have disease of the ankle-joint itself, depending upon primary disease of the astragalus for its origin. and then the laxity, grating, &c., symptomatic of diseased articulations are present. The treatment in such cases consists generally in removing the astragalus from its bed, and gouging away any diseased bone which may exist either on the upper surface of the calcaneum or under surface of the malleolar arch. Very large portions of bone may be removed from this situation. I have taken away the whole of the malleolar arch and astragalus, and gouged out the upper surface of the os calcis very freely, and yet the patient has recovered with a strong and *movable* foot, but very little shortened and deformed.

The *scaphoid bone* stands next in importance to the astragalus in its power of implicating a great extent of the foot when diseased. The morbid action may extend either backwards, and affect the astragalus—in which case you will act much as you would do in disease of the latter bone; or it may pass forwards, and then the whole anterior tarsal synovial membrane becomes affected. A bulbous swelling of the anterior part of the foot, perforated by fistulous openings leading to diseased bone—the heel, astragalus, and ankle-joint being free—indicate the existence of the condition which I have just named.

How, then, are we to treat disease of the scaphoid extending to the large anterior tarsal synovial membrane? Resection in such cases is, I believe, useless. I have never seen nor heard of that operation being done, and I should imagine that if the scaphoid were excised, the operation would be followed by total disorganization of the foot, requiring amputation. In these cases Chopart's operation is usually the only resource, and should be performed, except in certain instances, where, from the very extensive disorganization of the soft parts, we may require to go farther back.

When the *cuneiform bones* are the seat of caries, you will generally find that the middle cuneiform is the bone primarily affected. Thence the disease extends to the lateral ones, or to the basis of the second and third metatarsal bones. In such cases the anterior tarsal synovial membrane usually becomes extensively implicated, and Chopart's amputation will be required. But if the morbid action continues to be limited to the middle cuneiform and the contiguous metatarsal bones, and the patient's general health is good, removal of the diseased osseous structures by the gouge, with extraction of the carious cuneiform, may be attended by successful results.

The *cuboid* is seldom primarily diseased. I have had two such cases, one of which was successfully treated by gouging; but in the other—that of a man named J., whom some of you will recollect—Chopart's amputation became necessary, in consequence of implication of the anterior tarsal synovial membrane.

In many cases of diseased tarsus, as in that forming the text of these remarks, the morbid process is not confined to one, but spreads to several other bones. Here you must be guided in your treatment by the seat and extent of the disease. In one such case—in a lad about sixteen, who was sent to us from Staines—I removed the lower two inches of the fibula, some of the under surface of the tibia, and greater part of the astragalus, os calcis, and cuboid, and yet complete recovery took place; and in the man upon whom I operated last week, although nearly the whole of the outer side of the foot was gouged away, I should expect an excellent result were it not that the articulation between the cuboid and external cuneiform bone has been opened, and that (and now you will see the force of the remark I made at the commencement of the lecture), in consequence of this the disease is likely to extend across the foot, disorganisation of the whole foot to take place, and amputation to become necessary.

In conclusion, I must warn you not to be in too great a hurry to operate on very young children. You will find that in infants, and in children under five years of age, caries of the tarsal bones with abscess may frequently be recovered from by proper constitutional and local treatment, conducted on ordinary principles, without the necessity for operative interference.—*Lancet, June* 18, 1859, *p.* 603.

42.—ON PIROGOFF'S OPERATION.

By Dr. Eben Watson, Lecturer on Physiology in Anderson's University, Glasgow, and Surgeon to the Glasgow Infirmary.

[James M'G——, aged fourteen, received a severe injury to the foot whilst cleaning machinery. The whole foot, with the exception of the heel, was completely "smashed," as it is familiarly but expressively denominated.]

There could be no difference of opinion as to the propriety of amputation in such a case. The only question was where it should be performed. The parts were much too severely and extensively injured to admit of Chopart's amputation through the tarsus being practised with any hope of success. I might have performed Mr. Syme's amputation through the ankle-joint; but, from the shattered state of the foot, the dissection of the heel-flap would, I think, have been difficult. It seemed to me that this was a favourable case for performing the amputation recommended by M. Pirogoff; for the skin and bone of the heel seemed to have been uninjured, while no other portion of the foot was in a condition to be saved. My colleagues agreed with me in this opinion, and therefore, the patient having been put under chloroform, I proceeded with the operation as follows:—

With a straight, pointed, and strong bistoury I divided all the tissues down to the os calcis, from a few lines in front of the inner malleolus, to the same point on the outside of the ankle, and I connected the extremities of this incision by another across the front of the ankle. I then opened the joint, and cut the lateral ligaments carefully, especially the inner one, keeping close to the bone, that I might not injure the posterior tibial artery. I next sawed through the os calcis, from above downwards, close to the posterior margin of the astragalus; and, having cleared the articular ends of the tibia and fibula, I sawed off a thin film of bone from them, of course taking away with it the two malleoli. Some of the tendons that had escaped the knife were now shortened, and then the plantar arteries and the anterior tibial were tied. I now found that when I brought up the cut surface of the heel bone to the cut surface of the tibia and fibula, they were easily placed in exact apposition, and were as easily retained there by three silver sutures in the front of the stump, bringing the ligatures out by the sides of the wound, which were left open, so as to admit of a free discharge. The stump was surrounded by wet lint enveloped in oiled silk paper, and when the boy had been replaced in bed, it was laid on a pillow. After recovering from the chloroform, he took twenty drops of laudanum, and slept pretty well during the night. For a few days he was feverish, and required low diet, laxatives, and even an antimonial mixture; but this passed off when suppuration became established in the wound. His health was at no other time affected during his stay in the hospital.

Locally, the treatment consisted in having the leg bandaged pretty firmly in a hollow posterior splint of pasteboard padded with cotton, and cut away at each side of the heel, so as to allow the discharge to run away freely. The wound was kept clean by changing the dressings every day, but the splint was only changed on the second, third, or fourth day, as it seemed to require. The ligatures came away on the sixth or seventh day after the operation, but the sutures were allowed to remain in twice as long. On their being removed, a strip of plaster was placed so as to keep the wound together, but I do not think it was either very effective or very necessary, and it was soon dispensed with. The posterior splint, and a turn of bandage brought round the point of the stump as well as laterally, were, I think, the means of keeping the parts in due apposition during the healing process. Water dressings at first, and afterwards lint dipped in olive oil, were the only other applications used.

About three weeks after the operation, a small abscess formed above the inner ankle. It was freely opened, and healed readily. The original wound, too, had united healthily by the beginning of October, and the boy was then allowed to walk about on crutches, still, however, wearing the splint for the sake of greater security against injury to the stump. About the same time I observed that the calcis had become united to the tibia and fibula, so as to be nearly immoveable. I did not, however, allow this to be very severely tested.

On the 17th of December, I showed this boy to the clinical class at the Royal Infirmary. He could then walk with perfect freedom on the stump, and without any lameness. As he stood before the class there was no apparent deformity, but when the limbs were compared from the knees downwards, the left (or one operated on) was about half an inch shorter than the other. The stump was as perfect as can be imagined. It was difficult to make out the cicatrix in front. There was complete osseous union between the bones involved in the operation, so that they formed an united support for the body; and the skin of the heel, tough, strong, and smooth, formed a very hardy covering for its extremity.

In regard to the performance of Pirogoff's operation, I would offer two remarks:—

1st. It is much easier to avoid cutting the posterior tibial artery behind the inner malleolus in this operation than in that proposed by Mr. Syme. For every one who has performed the latter knows that he is not the least likely to cut the artery so long as he is attending to it—viz., while he is separating it from its attachments. It is when he is doing something else, and especially when he is dissecting back the skin of the heel, that his knife is apt to slip upon the vessel before he is aware. Now, in Pirogoff's operation, after the artery has been detached along with the skin below the inner malleolus, the operator cuts no more in that direction at all. He has only to cut the internal lateral ligament of the ankle-joint, keeping his knife close to and parallel with the astragalus, and then to use the saw; the soft parts being held back for him by his assistant, by means of a blunt hook or with his fingers.

2nd. M. Pirogoff, in his memoir describing his new operation, writes as follows:—"I separate the short anterior flap from the two malleoli, and saw through them at the same time close to their base." And again: "I turn this flap (the posterior) forwards, and bring the cut surface of the os calcis *in apposition* with the *articular surface* of the tibia. If *the latter be diseased*, it is sometimes necessary also to saw off from it a thin slice with the malleoli."

I wish to draw your attention particularly to these directions, because I venture to differ from M. Pirogoff in regard to them. I even think that the unfortunate issue of some of his cases may be attributed to his following that plan of operating. In my opinion, you ought always to saw off the articular extremities of the bones of the leg; for then you have a clean surface of cancellar bone on either hand—viz., at the anterior part of the os calcis, and at the inferior extremities of the tibia and fibula. Such surfaces are the best adapted for speedy osseous union; whereas, if the articular surface of the tibia is left untouched, as I understand M. Pirogoff recommends to be always done when it is not diseased, the synovial membrane and cartilage must inflame and suppurate, and be partly absorbed, partly discharged, before osseous union can take place between the tibia and calcaneum. The position of matters is very different when soft parts are to be applied to the surface of a joint, as in amputation through the wrist-joint, which you saw me perform about three weeks ago, and which has healed without one of those untoward circumstances that used to be dreaded by surgeons in such cases. If, however, the operation of M. Pirogoff be performed without excising the articular surface of the tibia, we should have a state of matters more analogous to the excision of one surface of a joint, which few surgeons would recommend. The bringing up of the os calcis, and not a soft flap, upon the articular surface of the tibia makes the greatest differences in the processes pursued by nature before healing is permitted. In the one case it is generally simple adhesive inflammation; in the other, it is, as I have stated above, a more lengthened and complicated, and therefore more dangerous, process. Hence it is that I should recommend the surgeon in all cases to saw off a thin layer of the articular surface of the tibia along with both malleoli, before he brings up his posterior flap.

But if this be done, it may be asked why disarticulate at all? In thinking of this question, I planned the following procedure, which, I think, will be found easier than M. Pirogoff's. The leg is placed on its side, the operator holding the front part of the foot to be amputated in his left hand. He then makes an incision with a bistoury across the side of the foot, from the tip of one malleolus to that of the other, carrying it right down to the os calcis. He then applies the serrated edge of a small amputating saw in the wound so as to divide the os calcis at such an angle as will enable him to avoid touching the malleoli. The assistant ought to steady the os calcis by grasping the heel between his finger and thumb, while the operator is using the saw. The latter then resumes his knife, and placing it between the divided surfaces of the bone, cuts a little upwards, till he gets fairly behind the upper portion of the ankle-joint. The posterior flap is now formed, and should be turned upwards on the back of the leg by the assistant, so as to keep it out of the way. The knife should next be carried in a circular manner round the anterior aspect of the joint, dividing the skin in such a way as to unite the points of the former incision by this transverse one in front. The skin in front should then be pulled up a little, and the tendons and other structures should be divided down to the tibia and fibula, just above the ankle-joint. Lastly, these bones are sawn through in a slanting manner, by directing the saw from before backwards and downwards. The posterior flap is now brought up, and it will be found that the cut surfaces of the tibia and fibula on the one hand, and of the os calcis on the other, will fit each other exactly. The skin in front is united by wire sutures, and the operation is finished.

I am not at all anxious to claim originality in regard to this modification. My aim is not to rob M. Pirogoff of any share of that honour which is justly his due, but to assist in perfecting and establishing his operation amongst the resources of surgery. Nor am I singular in thinking that the operation, as proposed by him, admits of improvement. Many surgeons in this country and on the Continent have suggested variations in its performance, and I only ask that the above method of operating, *without disarticulating*, which is its sole distinctive feature, may be carefully considered, as I have no doubt the other proposals have been.

I may remark that I do not think the slant-cutting of the os calcis an improvement in itself, though it has been proposed by M. Sédillot so long ago as 1855, and again by Mr. Busk, of the Seaman's Hospital, in 1858. I have merely adopted it in my modification for the purpose of avoiding collision with the malleoli in sawing through the os calcis; but I believe that the less slanting the longer will be the limb, and the greater the ease of keeping the ends of the bone in apposition.

The method which I have proposed occupies less time than that of M. Pirogoff; the risk to the posterior tibial artery in disarticulating the foot, and the trouble of the additional dissection are avoided, while an equally good stump is made in the end. Besides, it will be found that, in performing M. Pirogoff's operation for some injuries of the foot, in which the greater part of it has been destroyed or lacerated, one of the surgeon's chief difficulties will be, the want of purchase in steadying the foot while he is sawing through the calcaneum after disarticulation. He can only hold it by the broken and lacerated front part; whereas, if he applies the saw in the way I have proposed, the attachments of the ankle-joint, and the possibility of the assistant's seizing the projecting part of the heel, make this work much easier. Again, in sawing off the articular ends of the tibia and fibula, he has the astragalus to hold by, instead of the slippery ends of the malleoli.

After I had performed this operation twice on the dead subject, and was convinced of its suitableness for the accomplishment of the object in view, I employed it in the following case.

Thomas M'C., aged thirty-three, carter; admitted on the 4th January, 1859. "This afternoon a loaded railway waggon knocked the patient down, and passed over his right foot, producing great laceration of the soft parts, and fracture of many of the bones of the foot. The tissues on both sides of the ankle and foot are very much separated from the bones. Patient does not labour under any shock."

At a quarter-past nine P.M. a consultation was called on this man's case. I then found his foot completely smashed except the heel, and, as stated above, the skin and soft parts were separated back to the very malleoli. Indeed, so bad was the laceration that some of my colleagues recommended amputation at the lower third of the leg. On careful examination, however, I found that I could perform the operation above described; and I accordingly did so, making as good a stump as in the former case, with much greater ease and expedition. The patient was feverish for a few days after the operation, and received gentle antiphlogistic treatment. He afterwards progressed slowly but uninterruptedly in his amendment. Just as in the preceding case, the limb was placed in a posterior splint of pasteboard, the stump was covered with water dressings, and the whole was supported by a bandage. The dressings were, of course, changed from time to time, but no adhesive plaster was applied to the wound.

He was dismissed cured on the 12th of March. His stump was sound, and the union of the bones perfect. He had been for some days accustomed to walk on crutches.

M. Pirogoff seems to dread the occurrence of two evils after this operation. These are—

1st. The death of the os calcis. Now, I can hardly think that there is any very great danger of this occurrence, especially if the posterior tibial artery is not divided too high up. And even if the performance of the operation is thus marred, still the branches from the posterior peroneal artery to the outside of the calcaneum would, I think, be sufficient to maintain its vitality. At all events, there is no greater risk of death of the os calcis than of the posterior flap in Mr. Syme's operation. The same precautions are requisite in both cases, and will be equally efficacious in both.

2nd. Abscesses in the sheaths of the tendons are greatly feared by M. Pirogoff; and it cannot be denied that they are likely to occur in some of these cases. He recommends that the tendons be not cut too short in the formation of the flaps; otherwise, when the muscles contract, the sheaths will be left empty towards the wound, and, in his experience, more liable to suppuration. It is very proper to attend to this advice, but surely it is seldom that these abscesses, supposing them to have occurred, are so very dangerous as he represents. The abscess is in most cases limited by exudative matter to a small part of the sheath, and, if freely opened when pus has formed, it generally proceeds no further, but heals kindly and readily. Such an abscess formed in the first of the cases which I have reported above, and it hardly retarded the progress of the case for a single day. It will, moreover, be obvious that this is a danger which is apt to present itself in all cases of amputation through parts supplied with long tendons, as at the ankle or in the forearm, but it has never been considered so very formidable by other surgeons.

It has occurred to me, that the splint, which I kept steadily applied in these cases, may have operated favourably in preventing the formation of abscesses in the sheaths of the tendons. This apparatus, no doubt, kept the whole limb quiet, permitted no jerking of the muscles, and prevented, to a certain extent, their contracting and pulling the tendons up from the cut extremities of the sheaths.

The chief advantages of M. Pirogoff's operation are—1st, that the length of the limbs is preserved as nearly equal as possible under the circumstances. M. Pirogoff's own statement is thoroughly borne out by my experience of his operation:—"The leg," says he, "after my operation, appears an inch and a half (sometimes more) longer than in the three operations (Syme, Baudens, Roux), because the remnant of the os calcis left in the flap, as it unites with the inferior extremities of the tibia and fibula, lengthens them by an inch and a half." In the case of the boy M'G., the left leg is only two-fifths of an inch shorter than its uninjured fellow; and in M'C.'s case it is not more than an inch and a half. This great difference, in these two cases, is easily accounted for—(1.) Everybody knows that there is great inequality in the length of the os calcis in different persons; some are more spur-heeled than others, and these persons, however clumsy their feet may have been before, would obviously make the best subjects for Pirogoff's operation. (2.) In M'C.'s case, the soft parts round the ankle were much lacerated, as formerly stated; and, in paring my anterior flap, I had to cut rather higher up than was desirable. The saw was also applied fully high up, so that a good half inch of the tibia was cut off. Had it not been for this accidental circumstance, his limb might have been nearly half an inch longer.

2nd. The skin and areolar tissue of the heel are stronger and sounder in the stump after Pirogoff's operation than they could be if dissected off the heel, and applied to the ends of the bones of the leg, as must be done in any form of amputation through the ankle-joint. The support for the body is thus much better in the former than in the latter case, and the patient is sooner able to use it in walking. The boy M'G., on whom I first operated, began to walk on his stump as early as six or eight weeks after the operation, and, in less than four months after its date, he could use it with perfect freedom. He still continues to do so, and his defect is hardly observable, whether in walking or standing, although he wears a very clumsy artificial foot. The other patient, M'C., was, for some time, timid in using his stump, but by the beginning of May, when he last showed himself at the hospital, he had quite overcome that feeling. He had obtained a very good light artificial foot, of such simple construction that it only cost a guinea. He could walk without a stick, and it was remarked by every one who saw him, that no stranger could discover from his manner of walking that he had lost his foot.

In conclusion, I think that M. Pirogoff's operation is a great improvement in surgery, and I am astonished to learn that he has himself departed from it; for so it was reported officially by Messrs. Mouatt and Wyatt to Sir John Hall (Fergusson's Surgery, 4th edit., p. 487). For my part, not only can I see no good reason for abandoning the operation, but I think its proposer deserves much credit. It seems to me preferable to any other form of amputation at the ankle-joint when the heel is sound; and I shall even go further, and maintain that it is, *in some cases*, preferable to Chopart's amputation through the tarsus. I refer to cases of injury of the foot in which the latter operation is sometimes attempted, though it may be impossible, owing to the laceration to procure a sound covering of soft parts for the astragalus. Now, when this is not done, the face of the stump is apt either not to close at all for a long time, or to ulcerate whenever an attempt is made to use it. It thus remains painful and useless for a length of time, during which the muscles of the calf of the leg contract and pull up the heel, thus increasing the mischief both as to pain in walking and deformity of the injured limb. Illustrations of these remarks must have occurred in the experience of every practical surgeon, and two cases lately came under observation in our own hospital; the patients having sought relief because of their painful and ulcerated, and therefore useless, stumps. In the stump which remains after Pirogoff's operation, such a state of matters could never occur, both from its shape and from the fact that the operation wound is *fixed* high up in front, where it is in no danger of being hurt in walking.

I have said nothing as yet about the choice of cases for this operation, and, indeed, very little need be said at all. It is obvious that the heel bone must be sound, otherwise the case is not suited for Pirogoff's operation. When the tarsal bones are diseased, the os calcis is seldom free from the morbid affection; hence, in such cases, Syme's operation is generally more applicable; whereas, in accidental injuries of the front part of the foot, if neither Hey's nor Chopart's operation can be performed with a good covering of soft parts in front, then an admirable stump may generally be procured by adopting the procedure of M. Pirogoff.—*Lancet, June* 11, 1859, *p.* 577.

43.—ON SURGICAL OPENINGS INTO THE KNEE-JOINT.

By John Adams, Esq., Surgeon to the London Hospital.

[Is it desirable or necessary to open by incision so large and complicated a joint as that of the knee? Some years ago a man was brought into the London Hospital who had been bitten in the knee by a tiger. Suppuration occurred in the joint, and the limb was amputated. Instead of the amputation a free incision ought to have been made into the knee-joint, to give exit to the matter. There are now four cases under the author's care, in which he has thought it necessary to open the knee-joint. They are all different in many very important respects.]

The first is that of a man about 45 years of age, who was admitted into the hospital in consequence of a compound fracture of the patella, which necessarily caused an opening into the knee-joint. The treatment pursued in the outset of this case was, in my opinion, quite judicious, as a laudable attempt to save the limb was made; and, the parts being accurately adjusted, the limb was kept in an extended position, and the usual antiphlogistic means being had recourse to, the wound over the patella soon healed. But the case did not progress satisfactorily; for, inflammation occurring, abscesses formed in various directions, some superficial and others very deep, so that I was compelled to cut through the muscles of the calf to remove some dead fascia which kept up the discharge. After this, pus formed in the interior of the knee-joint, and I was compelled to make incisions on either side of the patella at different times, to give exit to the matter. After struggling on for a length of time, anchylosis of the knee took place, at an angle certainly not quite convenient for freedom of progression; but I hope the limb will eventually become useful, as all discharge has ceased for a considerable time.

The next is the case of a man, aged about sixty, who was admitted in consequence of some obscure inflammation of the leg after an injury. The case was progressing favourably, when a deep-seated swelling formed in the ham, and, after a few days, deep fluctuation was detected, which led me to make a very cautious opening, through which a large quantity of pus was evacuated. The opening was enlarged, and it was found that the matter came from between the heads of the gastrocnemius externus, and close to the large bloodvessels in the popliteal space at the back of the posterior ligament of Winslow. Nothing untoward happened for some days, when the abscesses ceased to discharge, and the knee-joint began to swell. The swelling increased day by day, and evidently indicated suppuration in the knee-joint. Now this was very remarkable, because it appeared as if the matter had made its way from the popliteal space into the interior of the joint through the posterior ligament; at least, it appeared so to me. However, severe constitutional irritation occurred, and it became requisite, from the tension of the joint, to do something for the patients relief. I had some idea of amputating the limb, but it was determined, at a consultation with one of my colleagues, to make an attempt to save the limb, and to effect this to make a free incision into the joint. I therefore opened the joint, first on the inside, and, at intervals of ten days or a fortnight, on the outside, and gave exit to a large quantity of sero-purulent fluid on both occasions. The man has struggled through, with a constitution evidently damaged by hard living, and severe labour, and is progressing favourably now; but the opening still remains patent on the outside of the knee, and the fungus nature of the granulations points at some deep-seated disease, probably of the bone itself. I cannot speak very favourably of the case, but I think enough has occurred to show me that, if his constitution were sound, he would make a fair recovery, with an anchylosed joint. I have now directed him to be sent into the country for the benefit of his health. The case is remarkable, from the peculiar course which the abscess appeared to take, having made its way from the popliteal space into the joint itself. I feel satisfied in my own mind that amputation of the thigh would have been attended with a fatal result, from the defect in the man's constitution; and, therefore, although the cure is as yet imperfect, I am under the impression that no other treatment could feasibly have been adopted than the one I pursued; at any rate, it proves that you can open the knee-joint without destroying life or limb.

The third case is one of a lad about twelve or thirteen years old, who was admitted in a very advanced stage of low fever. He had also considerable swelling of the left leg, consequent on inflammation of the subcutaneous cellular tissue. A large abscess formed over the head of the tibia, which I opened. After this, matter formed very deeply along the outside of the knee-joint, and this I opened. Soon afterwards the joint itself became much swollen and painful, and was tense with fluid. I suspected matter in the joint, and made a free incision into it. I found a large collection of pus, and the articular cartilages extensively eroded. I could not advise amputation, as he was still laboring under symptoms of low typhus fever; and he sank, after a few days, with signs of general pyæmia.

In this case it was apparent from the beginning, that no operation was advisable except that of opening the joint to give exit to the matter. It is of no use amputating in such cases, as the constitutional disease, (probably the cause, rather than the result, of the local mischief) could not be benefitted by so violent a procedure. And I cannot help thinking, that operations performed under such conditions, fatal as they always are, bring discredit upon surgery, inasmuch as they are always and unexceptionally attended with the loss of life.

Now, the last case of this series to which I shall direct attention, is one now under treatment, and is that of a man whose limb I amputated in consequence of extensive injury to the ankle-joint. This case is interesting, as showing how much the constitution can bear under very unfavorable circumstances, and under a most complicated series of affections. The man's condition has been, no doubt, materially injured by a habit he acquired in China, of eating opium to a large extent.

He was admitted three or four months ago, in consequence of extensive injury to the left ankle, from the falling of a heavy weight on his leg and foot, by which he sustained a fracture of the tibia above the malleolus; and the fracture being exposed, it may be termed a compound fracture. Sloughing of the skin over the inside of the leg and ankle laid bare a large portion of the tibia, which protruded to a great extent, being detached from the lower fragment, which remained in connection with the astragalus; this, however, becoming subsequently detached, was removed by the dresser. About four inches of the protruding tibia was dead; the consequence was, that extensive suppuration was going on, and there was no possibility of repairing the mischief without amputating the leg or removing the dead bone. On consultation, it was determined to saw off the dead bone. There was no difficulty in doing this; and there appeared to be a fair prospect of success, as granulations of a very healthy character shot up from every part of the exposed surface. However, after three or four weeks, the man got materially worse, and the granulations assumed an unhealthy aspect, and, as his constitution seemed evidently suffering from the long-continued drain, I thought it right to amputate below the knee. The operation was performed very high, just below the knee, on the principle advised by Mr. Teale in amputation of the thigh. The skin of the upper surface of the stump sloughed to a slight extent. During the progress of the case, hemorrhage to an alarming degree occurred from the popliteal artery, and this was secured by Mr. Ward, who laid open the stump to reach it. His progress was satisfactory until inflammation attacked the knee-joint, and a large quantity of fluid was poured out, and which presented itself towards the inner side, as is usually the case. I opened this freely, and let out a large quantity of sero-purulent fluid, by which the patient was relieved. The only additional circumstance I need mention in this case, is the fact that, the opening on the inside of the knee becoming closed, a large abscess has formed in the bursa beneath the tendon of the rectus, which frequently communicates with the joint itself. There is every prospect of cure.

Thus, you perceive, that at the same time I have had four cases under my care, in which I have thought it advisable to open the knee-joint. And I may ask the simple question, why should you fear to open this joint? You may be quite certain of this, that where suppuration has taken place, it is absolutely necessary that you should adopt this procedure, as there is no chance whatever that the pus can be absorbed under the circumstances mentioned in the preceding cases. I think the custom of freely opening the joint is due to the writings and practice of Sir Benjamin Brodie, and I am sure that you cannot commit a more fatal error than to leave patients unrelieved of abscess in the interior of the joint, under the impression that you are likely to add to the mischief already existing by opening so large a joint as that of the knee. Let me give you this piece of advice: if you determine to cut into the joint, let your incision be free, and do not trouble yourself by squeezing the matter out with your hands, but lay a flannel, soaked in warm water, over the part, and allow the cyst itself, by its inherent faculty of contraction, to force out its contents. In most cases, it is desirable to leave the opening patent, and not to trouble yourselves to bring the edges of the wound together by strapping and bandaging, although this practice may sometimes succeed. You have seen cases in the hospital where the constitution has been infected with very fatal blood-poison to which the term pyæmia is given; and, in many of these cases abscesses form in the various joints of the body. In such cases, I would especially advise you to make free incisions into these abscesses, and the result of my experience is that by such treatment many lives are saved.—*Lancet, August* 13, 1859, *p.* 155.

44.—ON ACUTE PERIOSTITIS.

By T. B. Curling, Esq., Surgeon to the London Hospital.

There are few operations more frequently performed in the hospitals of London than those required for the removal of necrosed bone, and they are certainly more common now than they were in former years. The results show that these operations are essentially of a conservative character,—that tedious sinuses, always discharging and liable to inflame, have been enabled to close, and that useful limbs have been saved by the removal of a constant source of irritation. These operations have become more frequent, then, because surgeons have become more strongly impressed with the advantage of extracting locked-up or impacted dead bone—have become less afraid of ill consequences from the necessary disturbance of parts, and have been emboldened to undertake tedious and troublesome operations, knowing that they can be rendered painless by chloroform. Our museums are rich in specimens of encased bone taken from amputated limbs; but such preparations are rarely added now, because it is seldom that a limb is removed for such a disease until the attempt to save it has been made by extracting the incarcerated bone. But so numerous are cases of necrosis in hospital practice that it is well to inquire whether we cannot, in many instances, prevent this serious result. The more common cause of necrosis in the long bones is acute periostitis; consequent upon injury.

When we consider that the whole, or greater part, of a bone may be destroyed by acute periostitis—that the inflammation may extend to the adjoining articulations, imperilling the safety of a limb, and that patients sometimes sink under the constitutional fever attending it, I need not urge the importance of an early diagnosis of the disease, in order that right and prompt measures may be taken for its removal. The complaint for which acute periostitis is most liable to be mistaken is acute rheumatism; and it is a mistake which, I fear, is not unfrequently made in practice. Indeed, some care and nice observation are required to make the diagnosis. In rheumatism, as in periostitis, there is high imflammatory fever, with swelling of the limb, and great pain, increased by pressure, so that the patient is nearly helpless, and he shrinks from the touch of the surgeon, in dread of the torture which an examination may cause him. In periostitis, say of the femur or tibia, the swelling is diffused. It is not limited to the larger joints—to the ankle or to the knee, but occupies a wider range, and is œdematous in character. But the chief diagnostic mark is the seat of pain. In periostitis little or no pain is caused by pressure, unless it be made over, or in the course of, the affected bone. You may, in the early stage, move the limb at the knee or the ankle, and press the ligaments and tendons without producing pain, but the slightest pressure on the bone excites intense suffering. If you press over the tibia or the muscles of the thigh around the femur in rheumatism, you rarely cause much pain; but in acute periostitis such pressure cannot be tolerated for a moment. The conclusion in favor of periostitis will be much strengthened if it be found that the attack of inflammation succeded an injury.

The treatment commonly recommended in acute periostitis is local depletion with calomel and opium. Just at the onset of an attack, in a superficial bone like the tibia, this treatment may be of service, but in periostitis of a deep-seated bone, or if the inflammation do not speedily subside, such measures are not to be relied on. After matter has formed beneath the membrane, they are worse than useless. They weaken the patient without exerting any influence on the disease. There is then no way of averting serious mischief but by a free incision of the inflamed periosteum.—*Lancet, Sept.* 3, 1859, *p.* 231.

45.—ON PAINFUL CICATRIX, AND IRRITABLE STUMP.

By Henry Hancock, Esq., Senior Surgeon to Charing-Cross Hospital.

[Painful cicatrix occurs for the most part in situations where the skin is naturally in close contact with the periosteum, as over the lower portion of the fibula and inner surface of the tibia. The pain frequently comes on only after the cicatrix is formed, the previous wound not being so.]

This is a point of some practical importance, and led me to pursue the treatment adopted in the following case—viz, "subcutaneous separation of the cicatrix from the periosteum," and the prevention of adhesion again taking place. These cicatrices have usually been dissected out; but the operation has proved very unsatisfactory, whilst the wound remains open, and being lost when it closes; and we may readily understand why this is the case when we recall the fact, that the wound made in this operation must be filled up by granulations springing from the periosteum or bone, as the case may be,—when we also remember the contraction which takes place in all cicatrices so formed, how the delicate periosteal nerves must be implicated, and how this contracting or contracted cicatrix must be continually dragged upon and irritated by every movement of the limb or muscles of the part. The following case illustrates the foregoing observations:—

Painful Cicatrix.—Mrs. B., when about thirty years of age, suffered from suppression of the catamenia, for which she was on several occasions bled in the leg. After the last bleeding, pain having occurred in the spot, leeches and lotions were employed, but without any beneficial result, the pain being much increased, and very severe. This continued for three years, during which she was treated by most of the first surgeons of the day for disease of the vein. She next consulted the late Mr. Liston, who at once excised the painful spot. The wound healed, and she remained free from pain for nearly fourteen years, when it returned precisely in the same spot, and continued for several weeks very severe, and not relived by treatment; there was neither swelling nor redness. Another surgeon of great eminence was then consulted. He proposed to remove the cicatrix, which was done with benefit for sixteen months, when the pain returned. The cicatrix was again removed, but the relief afforded only lasted six months. Removal of the cicatrix was again recommended; but the patient desiring another opinion, Mr. Chapman, of Hounslow, under whose care she was, brought her to me.

Her sufferings at this time were so great that she was willing to undergo anything that held out a probability of cure. She could not sleep at night, and appeared quite worn out with pain. Upon hearing the history of her case, I was struck with the fact, that after the last two excisions, she remained free from pain so long only as the resulting wounds were open and unhealed, but that directly the cicatrix was completed the pain returned; and when, upon examination of the part, I found that the skin, or rather the cicatrix, was adherent to the periosteum, and perfectly immovable, I concluded that her sufferings were due to this cause, and that they would not be alleviated until the parts were separated, and the new skin as far as possible placed in the same position as that of the surrounding integument. I, therefore, proposed that the skin should be separated from the periosteum by a subcutaneous incision, and that a reunion should be prevented by moving the skin backwards and forwards from day to day as might be deemed necessary. This was agreed to, and accordingly, assisted by Mr. Chapman, on Sept. 15th, 1857, I performed the operation with the common tenotomy knife, the part cut through being very hard, like cartilage. Mr. Chapman, who attended the case afterwards, informs me that some little inflammation followed, but that it was readily subdued; that the skin was prevented re-adhering, and that up to the present time the patient has remained in good health, and perfectly free from pain.

You will find the same treatment of service in cases of irritable and painful stump after amputation. This malady has been ascribed to various causes, as, for instance, the flaps being made too small in the flap, or the bone being left too long in the circular, operation; retraction of the muscles and soft parts; implication of the nerve in the cicatrix; undue development of the bulb at the cut extremity of the nerve; exfoliation of bone, and adhesion of the cicatrix to the bone, &c., &c. Where the integuments have been cut too short, or where there has been undue retraction of the soft parts, you have what is termed a conical stump, which you cannot mistake; where also there is exfoliation of bone, you may reasonably suspect its existence from the swelling and induration of the stump, whilst there will usually be redness and an opening with pouting granulation, marking the track to the exfoliating bone; but in other cases there is no sign of suffering for some time after the stump has healed, and, although the pain is almost unbearable, you will frequently be unable to detect anything abnormal either in the touch, colour, or quantity of soft parts. The character of the pain almost always points to implication of the nerve in some way or other, and accordingly operative surgery has been chiefly directed to this point; division of the nerve, excision of the bulb and a portion of the nerve, and secondary amputation being the plans adopted.

Excision of the bulb of the nerve, however, does not always succeed. I have done it myself in some two or three cases, but with only temporary benefit; and, from what I have observed, I am inclined to believe that in many instances the suffering is not so much induced by the nerve or its bulb as by the adhesion and connexion of the cicatrix by firm, unyielding cartilaginous structure to the periosteum or bone.

You will observe in the following case that this suffering occurs even though the cicatrix is not in immediate contact with the bone, but attached to it by an intervening mass or band; whilst the skin around the point of cicatrix corresponding to this mass is puckered in, there is a total absence of subcutaneous cellular and adipose tissue, present at other parts of the stump.

Painful Stump.—M. H., aged thirty, admitted into Charing-cross Hospital Nov. 30th, 1858. Had disease of the left knee-joint at ten years of age. At fourteen, the knee, being much swollen and very painful, was punctured, and a considerable quantity of blood escaped, but no matter. At sixteen the catamenia first appeared; they left her for two years, and then returned, but with irregularity. At seventeen years of age she fell, and so much injured her knee that she went into the Royal Free Hospital, where the leg was amputated. The stump healed rapidly; but, accidentally falling upon the floor, she hurt the stump so much that it reopened, and the bone protruded through the wound, which would not heal; the pain was intense, and subsequently about two inches of bone were removed. After this she recovered, and remained well until about four years ago, when she felt as though the limb was entire—as if the blood were rushing to every part below the amputation, accompanied with great pain in the nerves. The pain gradually increased, and ten weeks since it became more violent than ever, and was almost unbearable; so much so, indeed, that she begged me to amputate the leg higher up.

Upon her admission, on the 30th November, I carefully examined the stump, and found that the cicatrix at one point was tied down, as it were, to the end of the bone by a dense band about three-quarters of an inch long, and that any pressure upon this point increased her sufferings to a great degree. The end of the nerve, enlarged into a considerable bulb, could easily be distinguished, attached by this band to the bone also, thus accounting for the pain which she experienced in the course of the nerve. I had upon previous occasions, in other cases, dissected out these bulbs, but with so little success that I was convinced that the sufferings could not depend so much upon them as was usually supposed; whilst the result of the case which I have just related to you led me to expect that if the cicatrix were released from the bone so as to permit free movement, the patient would be relieved from pain without another amputation. Accordingly, on the 11th December, the cicatrix was separated from the bone by a subcutaneous incision, the connecting medium being so dense as to resemble cartilage. The soft parts were moved gently over the bone for a short time every day until the wound was healed and all trace of tenderness had ceased. The stump, which had previously been puckered and baggy, became round and plump; the pain entirely ceased; and she left the hospital, cured, on the 14th January, 1859. —*Lancet, July* 23, 1859, *p.* 79.

46.—*Simple Extension in Contraction from Burns.*—We lately had the opportunity of observing the treatment of a case of deformity arising from an old burn in a little boy, nine years of age, under Mr. Coote's care at St. Bartholemew's Hospital, which is worthy of notice. It is a plan in use, we believe, at the Orthopædic Hospital, and consists in the proper application of simple extension, perseveringly carried out. The boy was admitted on the 7th of April, with his lips and mouth drawn downwards from a burn in the neck when an infant. The cicatrix possessed the usual characters of hardness and thickening. By suitable appliances the head and chin were kept extended, with the effect of bringing back the lower lips and jaw to their natural position, and getting rid of the extreme deformity which had heretofore existed. The mouth can now be closed.

The effect of extension is to cause the absorption of the adventitious material present in the cicatrix, and thus permit the latter not only to become soft and extended, but permanently to remain so.—*Lancet, Aug.* 13, 1859, *p.* 162.

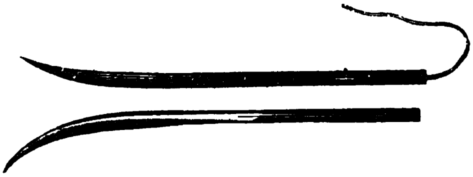
47.—ON THE USE OF VERATRUM VIRIDE IN SURGICAL FEVER.

By Dr. J. Y. SIMPSON, Professor of Medicine and Midwifery in the University of Edinburgh.

Some of our American brethren have latterly been using extensively, in febrile, inflammatory, and nervous diseases, a new and potent drug belonging to this class, viz., the veratrum viride. Its effects have been described by Dr. Osgood, Dr. Norwood, Dr. Barker, and other practitioners. They tell us that they find that by exhibiting in repeated doses a concentrated tincture of the veratrum viride, they can reduce the pulse and keep it reduced with a certainty and to a degree which can be effected by no other drug. Dr. Barker and others have published cases where they have thus brought down the febrile pulse in a few hours from 140 beats to 80, 60, or less in the minute, and kept it at will at this lower standard. It is a drug altogether which is certainly entitled to the strong attention of European practitioners. And it is not merely an arterial sedative. It is at the same time apparently a powerful depurant, stimulating the action of the skin, kidneys, and secretory functions generally. It has been successfully used as a depurant in acute gout and rheumatism, instead of colchicum. The veratrum viride is an American plant; but I think, from what I have seen, that we may fulfil the same therapeutic indications with the species which is in all our European pharmacopœias—the veratrum album. As the different species of cinchonas depend for their therapeutical effects in ague, &c., upon their all containing one and the same principle, namely, quinine, so in all likelihood the different species of veratra depend for their therapeutical effects upon a principle common to all members of the genus, namely, *veratrine*. Many of the old Greek physicians trusted often in chronic diseases, as Aretæus, Oribasius and others tell us, to a course of hellebore after they had failed in curing their patients by other plans of treatment. The researches of Dr. Adams leave little or no doubt that the white hellebore preparations of the Greeks were derived from the veratrum album. It will be curious if, in the cyclical changes to which medicine is even subject, we now turn back after 2000 years to the ancient "helleborism," or hellebore cure of Hippocrates and his successors. They had recourse to it, however, principally in chronic affections, as insanity, epilepsy, neuralgias, dropsies, &c. American practitioners have, on the contrary, been using the veratrum viride, or American hellebore, as Dr. Wood terms it, principally in acute diseases, as in pneumonia and other forms of inflammation, in puerperal and other forms of fever. Let me allude to one other drug before leaving the present indication. Chloroform when given in full doses, either by inhalation or by swallowing, depresses and brings down the rate of the pulse. In any surgical patient operated on under a complete dose of this drug, you will find the pulse sunk from 90 or 80 to 70, 60, or less. I have taken advantage of this action of chloroform in some instances of disease. The first case I used it in was the following;—A lady, whom I saw with Dr. Scott, of Musselburgh was attacked with peritonitis after abortion. The peritonitis was so very acute and severe, and all the accompanying symptoms so very formidable, with a weak and scarcely perceptible pulse, racing above 150, that I had little or no hopes whatever of the patient surviving. At her own request, to relieve her from her great abdominal pain, she was placed under the influence of chloroform; and when so, Dr. Scott and I found the pulse sank down to 100 or less, and became stronger and steadier. We found further, that as long as the action of the chloroform continued, the pulse continued thus greatly lowered in rate, and improved in power. Hence we agreed to keep her for a time continuously under chloroform; and in consequence of the evident good results, she was retained upwards of sixty consecutive hours under its influence. By the end of that time the great abdominal tenderness and tympanitis were almost entirely reduced, and the patient in an infinitely more satisfactory and hopeful state than when she first breathed the chloroform. The pulse never rose again to any very high rate; and all danger was over. But you cannot obtain the same beneficial and sedative influence upon the excitement of the heart from chloroform in all patients; or to state the fact more correctly, you will find it difficult in some, and impossible in others, to regulate its dose so as to keep up its continuous depressing action on the heart without sickness, vomiting, and other symptoms, coming on in such severity as to force you to leave off its use and have recourse to other means and other indications of treatment.

Dr Barker, in some of his interesting published observations on the veratrum viride, remarks, that while it will most surely reduce the quickened pulse of inflammation and irritation, "its use is not incompatible with that of stimulants. Experience has abundantly demonstrated the truth of this apparent paradox. One patient who recovered, took, every hour for two days, one ounce of brandy, and three to ten drops of the tincture of veratrum viride, the quantity of the latter being determined by the frequency of the pulse, which was never allowed to rise above 80 per minute, although it sometimes fell down to 40. In another case the veratrum viride did not seem to produce any effect on the pulse, which remained steadily above 130, until the condition of the patient was such that I decided to give brandy. After the first ounce was given, it fell to 108; after the second to 86. Continuing the brandy, the veratrum viride was suspended for a few hours, and the pulse again rose to 130. After this," adds Dr. Barker, "it was curious to observe the fact that, if either agent was suspended, the pulse would rapidly increase in frequency, while under the combined influence of the two, it was kept below 80 per minute.—*Med. Times and Gazette, May* 21, 1859, *p.* 516.

48.—*Needle for Metallic Sutures.*—[The needle below described is well adapted for employment with metallic sutures, and is moreover simple and inexpensive.]



It resembles in shape the ordinary sewing needle, but is flattened and grooved for about a third of its length. In the centre of the grooved portion are drilled two round holes (about a quarter of an inch apart) of sufficient bore to admit the passage of the wire intended to be used. In arming the needle, the wire is first passed through the hole at the greater distance from the point, then carried to the other, similarly inserted, and the end, which ought not to exceed the eighth of an inch in length, turned backwards, and pressed into the groove.

When threaded in this way, no portion of the wire lies above the plane of the instrument, so that no obstruction is offered to its passage through the integument, and the wound inflicted retains its incised character.

In the ordinary sewing needle, the size and form of the eye is such, that the metallic suture—which, of necessity, is doubled for a short distance—is very apt to become twisted and distorted, and thus prevent its easy application.

Two ingenious modifications have been devised by Mr. Lister, of Edinburgh, and Mr. Murray; but, I believe the one here described—which is made by Mr. Weiss, of the Strand, and Mr. Matthews, of Portugal-street, at trifling cost—will be found of general value, but especially when dealing with more than usually delicate and elastic tissues.—*Lancet, June* 4, 1859, *p.* 572.

ORGANS OF CIRCULATION.

49.—CASES OF POPLITEAL ANEURISM SUCCESSFULLY TREATED BY FLEXION OF THE KNEE-JOINT.

By Ernest Hart, Esq., Surgeon to the West London Hospital, and Alexander Shaw, Esq., Surgeon to the Middlesex Hospital.

(Communicated to the Royal Medical and Chirurgical Society.)

Mr. Hart had brought the case under the notice of the Fellows of the Royal Medical and Chirurgical Society, under the impression that they might be interested in the successful treatment of so formidable a disease as popliteal aneurism by the simple flexion of the knee-joint.

J. S———, aged forty-one years, consulted him in September, 1858, having a popliteal aneurism in the right ham. It was globular, of the size of a small apple, and situated at the lower and outer part of the popliteal space. It had a full beat, but was not very near the surface. Placing the patient on the sofa, and baring the leg in order to make a careful examination of the tumour, Mr. Hart found that its pulsation was affected by the angle at which the leg was bent upon the thigh, and that when very complete flexion was affected, its thrill almost wholly ceased. Concluding that in this position the course of the blood through the tumour was greatly retarded, he conceived the hope of effecting the cure of the aneurism by the deposition of active clots, if the leg could be retained for a sufficient length of time in the bent position. After a week's preliminary rest, treatment was commenced by bandaging the leg from the foot to the knee (not covering the tumour), thoroughly flexing the leg on the thigh, and retaining it in that position by the application of a stout roller. He was a thin, wiry man, and the flexion produced no inconvenience to him at the time. He passed a better night than during the previous week, when severe pain had been present in the aneurismal sac. What pain or annoyance was complained of during the treatment was referred to the knee-cap, but it was very trifling, and "barely deserving to be called pain." The tumour was examined on the morning of the third day (about forty hours after flexion was enforced), and considerable solidification had occurred. On the fifth day, the tumour was hard and solid, and neither pulsation nor thrill could be detected. The leg was lightly attached to the thigh at a right angle. On the seventh day, the patient was allowed to move about, the foot being slung. On the twelfth day, the leg was completely straightened, and the patient walked on it with ease, limping from stiffness at the knee-joint, consequent upon confinement. Six weeks subsequently, the tumour was hard and firm, and much smaller. After three months it was barely perceptible, and there was pulsation in that part of the artery. The patient was seen at various stages by the author's friends, Mr. White Cooper, Mr. Coulson, Mr. Holmes, of St. George's Hospital, Mr. Flower, of Middlesex Hospital, and Mr. Buxton Shillitoe.

The treatment by flexion in this case was perfectly and immediately successful. It was unattended with any difficulties, it offered no inconveniences, and was not followed by any other than satisfactory results. The case, however, was one particularly well suited for the essay of such a plan of treatment. The patient was not stout, which renders flexion difficult; nor was he aged, which makes it painful. The tumour was of average size and of average prominence; when the knee was bent, the aneurismal sac was below the line of flexure. These he believed to be all favourable circumstances. Cure was evidently effected in this method by the retardation of the current of blood, and the consequent deposition of active clots in the sac—the only manner in which satisfactory cures could be anticipated. This result was probably effected by the combined influences of pressure on the sac by the surrounding fascial and muscular tissues, and acute flexion of the artery. In so far as it was due to pressure, it appeared to be a return to the old method of treatment by direct pressure, but was free from the inconveniences of the screw and pad, which were open to the reproach of occasioning gangrene of the skin, rupture of the sac, and other accidents. Its simplicity and its success in this case appeared to strongly recommend it for further trial. If it were not always successful, there was not any other method free from the same objection; and there seemed reason to hope that the principle might admit of efficient application to a number of cases in which aneurismal tumours were developed opposite to the joints of the limbs.

[In the next case, communicated by Mr. Shaw, the same method of treatment pursued in the previous case was adopted.]

The patient, aged thirty, first perceived a pulsating tumour in the left ham a week before his admission into the Middlesex Hospital. It was of the size of a lemon, occupied the centre of the popliteal space; was easily compressed; the pulsation was strong, and there were other signs of its being a recent aneurism. On December 1st, the knee was secured in the bent position by a band brought round the foot and thigh, and fixed near the hip. The immediate effect of the flexion was that the patient ceased to feel the beating of the tumor, and that on inserting the oiled finger into the flexure behind the knee no pulsation could be discerned. On the fourth day, when the limb was unbound, the tumour was found to have lost about a third of its original size; its walls were thicker and denser, the force of the pulsation was considerably diminished, and the sac had receded more deeply into the popliteal cavity. Gradual improvement continued to take place. Between the third and fourth week from the commencement of the treatment the sac had become greatly reduced in size; its walls appeared nearly solid, and the pulsation was so faint that it was expected at each day's visit to find it extinct. The treatment was varied by occasionally undoing the strap, which confined the knee, for several hours together; but, owing to the stiffness caused by the long continuance of the flexion, the position of the joint was not much altered by the relaxation. It was not till the thirty-eighth day that the pulsation in the tumour altogether ceased. The sac was at that time about the size of a walnut. The patient gradually recovered the power of extending the joint. On the fiftieth day he could walk with only a slight halt, and on the fifty-sixth day he was discharged. During the first ten days the patient complained of the pain, as well as the irksomeness, of keeping his knee constantly bent; and for a slight swelling of the joint a lead lotion was applied. Afterwards he made light of the inconvenience, and he never at any time asked to have the belt relaxed.

At the close of the case, the author offered a few brief remarks on the principle on which the cure was effected, and, in illustration, added the observation that, by extreme flexion of the knee-joint of a sound limb, the force of the current of blood through the popliteal artery can be weakened to such a degree as to cause stoppage of pulsation in the tibial arteries.

Mr. Ferguson eulogised the papers read, and said he regarded the proceeding described by Mr. Hart as a valuable addition to the practice of surgery. He spoke of the value of pressure generally in the treatment of aneurism, and also of "manipulation"—modes of treatment which he thought would set aside, in many instances, the necessity for the knife. In the plan pursued in cases before the Society, there might be failures, but this was no reason why we should discard the operation, but should rather encourage us to persevere to determine the real value of the proceeding. This plan was not altogether novel, for it had been tried three or four years since in King's College Hospital. One of his house-surgeons had ascertained, in a case of popliteal aneurism, that when the leg was flexed upon the thigh, the pulsation in the tumour ceased. The aneurism was of the size of the fist, and was treated by pressure in the groin, and by flexure of the leg upon the thigh. This was persevered in for some time, but without benefit. The man, being impatient of treatment, left the hospital, and died of some other disease. To show the influence of position in certain cases of aneurism, he related a case of that disease in the popliteal space, in which the employment of pressure gave encouraging, but tardy, results. It was found in this case that on extending the leg to its full degree, after the employment of pressure, all pulsation in the tumour ceased.

Mr. Birkett briefly referred to three cases of aneurism treated by pressure which had come under his notice in Guy's Hospital. In one case, ordinary pressure in the groin was applied; then pressure by flexion. Neither did good; but it must be admitted that they were not fairly tried. The femoral artery was afterwards tied, and the patient recovered. In the second case ordinary pressure was applied at first with success; but suddenly the tumour became much enlarged, the femoral was tied, and the patient did well. In the third case, the patient, a man, had an aneurism in the right popliteal space. Pressure was tried, and in fourteen days he appeared well. The tumour contracted, and felt life a small hard ball. Flexion was then resorted to, but not persevered in, and ordinary pressure was again employed. The aneurism, however, gave way, and the femoral had to be tied. The man subsequently had a small aneurism in the left popliteal space; he would not submit to flexion, so the femoral vessel was secured.

Mr. Savory said that these cases were especially interesting and instructive in their relation to the physiology of the blood-vessels. It was familiarly known that a transverse wound of the artery gaped widely, and that when an artery was completely divided the ends retracted. Yet these important facts had seldom received more than a passing notice. They had never been explained. To what was this retraction due? The muscular tissue was in no way concerned in it, for it occurred at a long period after death, as well as during life. Neither would elasticity alone explain it. Another condition was required, and that was tension. The arteries were elastic tubes, always tense; so that, when divided, by no management of posture or position could the retracted ends be brought into apposition. The extent of their retraction was a measure, then, not of their elasticity, but of their tension. The constant state of tension was obviously connected with their purpose; by it their patency, under every variety of movement and position, was secured. But this rule had its exceptions, and these were to be found at the knee and elbow joints. At these parts, when an artery was divided, extreme flexion would bring their ends into apposition; but in this position, and for this very reason, the course of the vessel was interrupted; the course of the blood through it was impeded; the pulse ceased in the limb beyond. Thus he conceived was explained the principles upon which the cure of aneurism by this means was accomplished. It was not due to pressure in the sense in which that term had been employed. It was due to the fact that the circulation through the artery at a short distance on the distal side of the sac was arrested; so that, as far as the principle was concerned, it would probably succeed, whatever part of the popliteal space the aneurism occupied. Now, in connexion with this interesting fact—the arrest of the current through the artery by extreme flexion of the limb—Mr. Nunn, in some observations on the arrangement of the arteries of the limbs, recently published, had alluded to the remarkably free anastomosis which existed around these joints. They were clearly for the purpose, as he said, of compensating for the occasional interruption through the main channel. He (Mr. Savory) added, that this plan of treatment appeared free from one grave objection to the ordinary treatment by compression—namely, of interfering with the venous circulation. For although in extreme flexion the current through the main vein was interrupted also, yet here there was also an abundant superficial venous anastomosis around. The veins, like the arteries, were elastic, and, to say the least, were equally tense.—*Lancet, May 7, 1859, p.* 462.

50.—ANEURISM OF THE POPLITEAL ARTERY SUCCESSFULLY TREATED BY FLEXION AND COMPRESSION.

By Oliver Pemberton, Esq., Surgeon to the General Hospital, and Lecturer on Surgical Pathology at Sydenham College, Birmingham.

[In the following case both flexion and compression were combined in the treatment. Weiss's compressor was used.]

On the 16th of May (having made arrangements by means of relays of dressers that he should never be left for eight-and-forty hours), at twelve o'clock at noon, I fixed Weiss's compressor, and applied pressure by means of the lower pad to the middle third of the artery; at the same time, turning a bandage around the ankle-joint, I bent the leg as far as it would admit without occasioning pain, and fixed it firmly in this position by carrying the bandage around the pelvis.

The effect of this combination of flexion and compression was, that the pulsation in the aneurism was reduced to a mere wave, varying from time to time, as the compressing pad was adjusted by the attendant.

Two hours after the commencement of this treatment, he became very restless, and complained of a burning pain in the aneurism and down the shaft of the tibia; and, despite the permission to smoke, it was with much difficulty he could be kept quiet. Forty drops of the sedative solution of opium were given at five o'clock with good effect.

Toward midnight, between eleven and twelve hours after the commencement of the treatment, he became much quieter, and slept frequently. The pressure exercised by the tourniquet was comparatively slight, the aneurismal tumour having become hard and perfectly free from pulsation. Not the least movement of the leg from the state of flexion had, however, been permitted. There was considerable swelling of the knee, leg, and foot, but he did not complain of much pain. It was remarkable to observe the vehement pulsation of the superficial arteries after the arrest of the circulation through the tumour, especially about the neighborhood of the internal articular vessels. During the night, the pressure on the artery was removed, from time to time, from the lower to the upper pad; but the amount exercised was merely nominal, as it was quite evident that no blood had passed through the aneurismal tumour after the first twelve hours. A drachm of the sedative solution of opium was administered toward morning, to the great comfort of the patient, who was calm and placid, chatting with his watchers and smoking in the interval of his slumbers.

Throughout the day of the 17th, the same regulations were carried out, without the least deviation, and no annoyance was complained of from the position of the knee.

On the 18th, at twelve o'clock, forty-eight hours from the commencement of the treatment, I removed the compressor, and discontinued the supervision of the dressers. I made no relaxation whatever in the flexion of the knee. There was considerable swelling of the extremity, but no diminution in warmth. I had it carefully wrapped in cotton wool, and warmly covered up with flannel. There was good pulsation in the malleolar vessels, and not the least pulsation to be detected in the aneurism, or bruit along the course of the femoral artery. The articular vessels pulsated strongly.

On the 19th, the flexed position was slightly relaxed, and the entire limb rolled with flannel.

24th. Further liberty was allowed. The tumour has lost its lateral bulk, and begins to contract. The swelling of the leg is subsiding.

28th. Contraction going on in the aneurism. The bandage between the ankle and pelvis still more relaxed, so as to permit of the limb being straightened to the fullest extent the patient desired, without occasioning a sense of pain.

June 8th. All bandages confining the limb were removed. The aneurismal tumour was firmly strapped with adhesive plaster, and the entire limb accurately rolled.

20th. He was permitted to get about on crutches. The heel of the affected limb cannot touch the ground to bear weight, but he gets about very fairly, and is entirely free from pain.

In the course of the next ten days, much of the stiffness disappeared and he soon discarded his crutches, the limb having regained its straight condition, though somewhat larger than its fellow.

Aug. 15th. The patient has been retained in hospital during the last two months, simply for the purpose of watching the gradual dispersion of the aneurismal sac. He walks with ease, and has no stiffness whatever in the knee-joint.

Viewing the contour of the popliteal spaces from behind, the remains of the sac are plainly marked; its prominence contrasting strongly with the absence of a corresponding swelling in the parallel healthy space, so clearly defined by those slender, delicate, and distinct muscles peculiar to this race of people. It now occupies pretty much the centre of the space, extending, perhaps, somewhat more on the outer than on the inner side. It is very firm and hard, and measures in either diameter, even now, some three inches; so that a good idea can be formed of the once formidable dimensions of the disease.

16th. Professor Syme, on his way through Birmingham to Edinburgh, whilst visiting the hospital with me, examined the remains of the aneurism, and expressed himself as thoroughly satisfied with the solidity of that which yet constituted the sac, and at the same time conveyed to me his approval of the mode of treatment by flexion that had been adopted in the present instance.

For a long time past, I have been in the habit of applying flexion and pressure in combination in cases of wounds of the palmar arteries. The wound of the vessel has been compressed by a firm roller; the fingers have been laid over this in the flexed position, and maintained there; the hand has been flexed on the forearm; the forearm on the upper arm; the movement of the entire extremity has been further rendered impossible by the application of a roller from wrist to shoulder; and the consequence has been, that the happiest results have followed the treatment adopted.

It was this experience that led me to adopt the union of the two methods in the case above narrated. I had not then read the cases as communicated by Mr. Ernest Hart and Mr. Alexander Shaw to the Royal Medical and Chirurgical Society, and published in the Lancet of May 7th, 1859, in which these gentlemen had succeeded in effecting a cure of two cases of popliteal aneurism by the treatment of continued flexion alone. Had I done so, I should have felt inclined to have resorted to flexion, unaided by the assistance of the compressor. As it is, I think that the case is the first, so far as I am aware, in which the combination has been made use of from the first; and the success which has attended its adoption has been such as to lead me to the conclusion, that we may possess in it a means of treatment worthy of attentive consideration. I consider, however, that to flexion was the cure mainly due in this instance, as the use of the compressor was little more than an adjunct—at hand, to be called for in case of necessity arising. This conclusion is justified when it is considered that the pad of the compressor was never applied with severity, and that the amount of pressure exercised by it at first was decreased, instead of being gradually increased in order to control the circulation through the aneurism.

If we review the state of the tumour during the twelve hours from the commencement of the treatment, I think this conclusion will become inevitable. From the first, absolute flexion was established; compression was but partial. The circulation was reduced to a mere wave, which disappeared, and never reappeared after the first eleven hours. The compression was never, during the whole of the period, or subsequently, changed in its character; whilst the flexion was maintained unaltered in the least degree for the first three days, and then only slightly relaxed for the ensuing five.

In the case narrated by Mr. Ernest Hart, the pulsation terminated on the fifth day. In Mr. Shaw's case, it was not until the thirty-eighth day that the pulsation in the tumour altogether ceased. In neither of these cases was the aneurism half as large as in the case I have described, and yet there was no pulsation after the first eleven hours. The continued flexion of the knee did not appear to cause suffering, but there was considerable difficulty in recovering the straight position of the limb, which is not to be wondered at, when it is considered that for twenty days it was more or less retained in the flexed position. In a smaller aneurism, so long a maintenance of this position need not be called for; but in one of dimensions so considerable as this, there cannot be too great a care exercised to secure the solidity of the contents of the sac until their permanent removal becomes no longer a question of any anxiety or doubt.

I do not think that the position of the aneurismal tumour in the upper or lower course of the popliteal artery will be likely to affect this treatment by flexion. Extreme flexion will, in either case, arrest the circulation with equal safety, as the anastomosis of vessels is quite as abundant above as below the knee; indeed, we may attribute much of the success which attends the cure of popliteal aneurism to the varied character of the communications established, in the case of the obliteration of that trunk, between the articular vessels and the muscular arteries in both thigh and leg.

An unfortunate issue, in one respect, to the reputation of this treatment has occurred in the practice of Mr. Moore, of the Middlesex Hospital. A large aneurism of the popliteal artery—but not a larger one, judging from the description, than the one I have recorded—after having been submitted to incomplete flexure, as well as pressure, for about twelve days, burst through the ligamentum posticum into the knee-joint. Happily, this serious complication did not prevent the patient's recovery, for the artery was tied, the aneurism was cured, and the knee-joint recovered its usefulness. The opening in the artery was here supposed to have been situated on its anterior aspect, or that immediately contiguous to the ligament, so that extreme flexion would have a tendency to relax the artery behind the aneurismal tumour, in opposition to stretching it over where the opening might be situated posteriorly. In such a case, it is inferred that the treatment by flexion would not be indicated. That there will be cases of popliteal aneurism which neither pressure nor flexion, nor the two in combination, will cure, every one will admit; but supposing we were able to discriminate the situation with any degree of accuracy of the primary opening in the artery, must not, after all, the favourable issue of the case depend entirely on the ability possessed by the treatment applied to absolutely, sooner or later, restrain the current of blood flowing through the aneurismal tumour? Mr. Moore's patient seems never to have borne either flexion or compression with any degree of satisfaction either to himself or his attendant; on the contrary, the disease went on increasing rapidly, so as to necessitate the operation at length so successfully carried out.

This mode of treatment by flexion may not be so likely to succeed in mature subjects as in young adults; and, unquestionably, the flexibility of the muscles and joints in all must be a first consideration in its selection, as we are not likely to meet with materials so elastic to work on in the frames of even our English labourers as those possessed by my lithesome Asiatic, whose powers of genuflection so eminently contributes to his cure. It is not unworthy of remark that the continued indulgence in smoking, combined with the administration of powerful does of opium, appear to have contributed not a little to the favourable issue in this instance.—*Lancet, Sept.* 3, 1859, *p.* 232.

51.—*Hemorrhage from the Superficial Palmar Arch: Cure by Flexion of the Forearm.* (Under the Care of H. C. Johnson, Esq., Surgeon to St. George's Hospital.)—A short time ago, a paper was read at the Royal Medical and Chirurgical Society by a pupil of the London Hospital, in which the very simple plan of controlling hemorrhage from the vessels of the palm by flexing the forearm so as to make pressure upon the brachial artery in the bend of the elbow, was advocated, and a successful instance of its adoption was recorded. This plan, although not uniformly successful, is so easy, and interferes so little with other means that may be adopted, that it deserves to be widely known, especially as the accident is so common, and frequently occurs in country practice, where few resources are at hand. We therefore have much pleasure in citing the following instance of its success.

Thomas Cross, butler, was admitted on Aug. 1st, with a punctured wound on the palmar surface of the right hand, just over the region of the superficial palmar arch. At the time of admission, there was free hemorrhage from the wound; but this was restrained by pressure on the radial artery. The hemorrhage ceased after the radial artery had been compressed for a short time; but, on removing a clot which had formed in the wound, the bleeding again commenced with great vigour.

The history was this. The man was packing some wine, and one of the bottles broke, and a piece of glass entered the hand at the spot above named. On finding the hemorrhage recur, Mr. Vining determined to tie the ends of the bleeding vessel; but, just as he was about to do so, Mr. Johnson came into the ward, and advised him to try flexion of the arm, leaving orders that, if the bleeding commenced again, he was to cut down and tie the vessel. The forearm was flexed upon the arm, and a small pad of dry lint was put upon the wound, with very slight pressure. The pulsation in the radial artery was much diminished, but not stopped. In the evening the patient complained much of the discomfort of keeping the arm in one position. This was altered a little, but the arm was still kept flexed.

Aug. 2nd. There has been scarcely any bleeding since the arm has been flexed. He complains of a good deal of pain in the hand to-day, and also of numbness in the fingers; and he also complains of some pain in a lacerated wound of the arm, which he received at the same time.

Aug. 3rd. No more bleeding has occurred. The arm is still kept flexed.

Aug. 4th. The pad of lint was removed from the wound to-day, which has almost entirely healed by the first intention.

Aug. 6th. All pressure was taken off to-day, except that the forearm was still flexed, in order to raise the hand.

Aug. 9th. He complains of pain over the wound to-day, when pressure is made; and the pain also extends up the fingers. This probably depends upon some injury to the nerve. The wound, as far as any chance of hemorrhage is concerned, may be reported cured.—*Brit. Med. Journal, Aug.* 20, 1859, *p.* 665.

52.—*Dr. Warren's Styptic in Internal Hemorrhages.*—Dr. Cook reported to the King's County Medical Society, that he had used the styptic recommended by Dr. Warren, of New York, in almost every variety of hemorrhage, and has hardly known it fail in hemoptysis or uterine hemorrhage. It consists of sulphuric acid, ʒv.; spt. turpentine, alcohol, āā ʒij. The turpentine is slowly mixed with the acid, and, the alcohol having then been added the mixture is put into a stoppered phial. The dose is 40 drops rubbed up with sugar, and given in a teacupful of water, a second dose being given one hour after the first, and a third two hours after the second.—*Med. Times and Gazette, Oct.* 1, 1859, *p.* 342.

ALIMENTARY CANAL.

53.—ON THE RADICAL CURE OF HERNIA.

WITH AN ACCOUNT OF AN IMPROVED INSTRUMENT, AND NOTES OF FORTY CASES.

By Redfern Davies, Esq., Surgeon to the Birmingham Workhouse Infirmary.

[In cases of scrotal and femoral hernia, where the rings are very large and relaxed, the operation (Wützer's instrument being used) is sometimes unsuccessful, and has to be repeated. The cause of failure is not any defect in the theory of Wützer's method, but to a defect in his instrument. The author suggests the accompanying mechanical improvements.]

Upon examining a case of scrotal rupture in which the operation for the radical cure has failed (supposing, of course, that it has been properly managed, together with the proper after-treatment), the rings and canal will be found to be obliterated probably for some three-fourths of its extent, or there may only remain an aperture which will with difficulty admit a crow-quill; and thus, though the patient may be greatly benefitted, and with the aid of a truss resume his duties, a radical cure has not been effected.

That portion of the canal and rings which have been blocked up is invariably that which is nearest the abdominal walls. "The gut slips down behind the plug" are the terms in which both surgeons and patients express the mishap which has occurred, and the reasons for this, I believe, are as follows:—

The anterior or superior layer of the invaginated integument is subjected to not only the pressure of the wooden plug to keep it in apposition with the opposed surface of the rings and canal, but also to the direct pressure of the compressor. The compressor exerts its influence exclusively upon the parts included between it and the upper surface of the said wooden plug, and in no wise affects the posterior parts, viz., the posterior layer of invaginated integument and posterior surface of canal and ring, whose only chance of being kept in apposition depends upon the accuracy with which the plug fits the canal, &c., as a whole.

The floor of the canal, &c., especially where the tissues are lax, as generally occurs in cases of old and large ruptures, does not present in the same manner an opposing resistance to the wooden plug as does the compressor, and thus should the two former be not very accurately adapted the one to the other, adhesion cannot even be expected to occur.

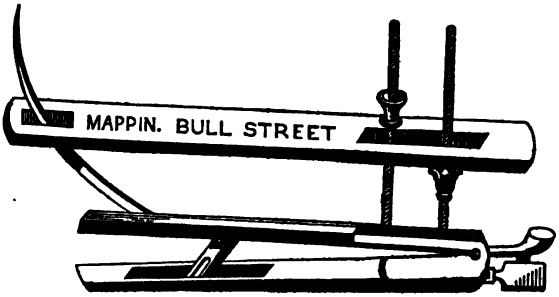
The mouth, or internal opening of the canal, is funnel-shaped, with the posterior surface the more sloped. Consequently, if there be any weak point, it will be there, and it is sure to receive all the shocks of the gut during the process of cure.

Besides it is evident that a cylinder, even closely applied to the rest of the extent of the canal, cannot fill up its funnel-shaped mouth; but must leave an interspace, which will be on the posterior surface.

And, again, it is not always practicable to introduce to a sufficient extent a solid plug, which would best fit the internal ring, by reason of the resistance of the other structures to its passage, besides entailing an endless variety of such plugs.

By the adoption, however, of the principle I now propose, viz., a plug whose lower half is capable of expanding, these difficulties are severally overcome.

A glance at the diagram will render its application at once evident: it will be seen that by turning the handle and thus causing the lower half of the plug to expand, that the pressure upon the parts included between the upper portion of the plug and the compressor, is left in exactly the same condition and relation as is the usual instrument, but that a force is exerted upon the posterior portions of the invaginated integument, canal and rings, which it gently, but firmly, retains in complete apposition one with the other.



By reason of the greatest point in its expansion being at the extremity of the instrument, and gradually tapering, two objects are accomplished; first, the funnel-shaped mouth, and the internal opening, is filled with a plug, whose sides are inclined towards its own—the invaginated integument being, as it were, modelled upon it; and, secondly, the rest of the canal is at the same time subjected to no undue pressure.

The principle adopted by Mr. Spencer Wells is likewise made available, viz., having the transverse diameter of the instrument very much greater than the antero-posterior, whereby the shape of the ring is altered, it being converted into a mere chink, and thus affording an additional security against descent of the gut; and so leaving as small as possible an amount of space between the opposed surfaces of the *doigt de gant* to fill up when the instrument is removed. A thin India-rubber finger-stall caps the end of the instrument, preventing any soft parts from getting between the blades.

[The author then gives a table of forty cases, (not including his more recent ones). Of these thirty-seven were cured, five requiring the operation to be repeated. Only two were complete failures, and of these one was owing to supervention of small pox.]—*Med. Times and Gazette, Aug.* 6, 1859, *p.* 128.

54.—MR. WOOD'S OPERATION FOR THE RADICAL CURE OF HERNIA.

[The patient was a young man, aged 20. The hernia was scrotal, on the left side, and of moderate size.]

The operation was conducted as follows:

The hair having been previously shaved from the left side of the pubes and scrotum, the patient was placed under the influence of chloroform. An incision about half-an-inch in length was then made through the skin of the scrotum, 1½ inches below the pubic spine over the cord, by means of a small tenotomy knife. The skin was then separated from the subjacent fascia, for about an inch around the incision—sufficiently far to enable the fascia to be invaginated without any of the skin—then the fascia was carried up before the forefinger of the left hand into the inguinal canal, where all the different structures can be distinctly felt. The arched border of the internal oblique muscle was then felt for, and the finger carried behind it upwards and towards the linea alba; then a strong curved needle about 4 inches in length, and fixed firmly into a handle, was passed along the inner surface of the finger with the concavity directed forwards—this was made to perforate the conjoined tendon close to the internal ring. The skin was now drawn upwards and towards the mesial line, so as to bring the external puncture nearer to the point where the second thread has to be passed, and the needle was then pushed through the integument and threaded. The needle was next withdrawn, leaving one end of the thread hanging out of the puncture. The finger was then carried behind the external pillar of the ring, the spermatic cord felt for, and pushed backwards and downwards; the needle was guided by the finger behind the external pillar, and made to perforate Poupart's ligament, and then to appear externally at the upper puncture by moving the skin towards the point; a loop of thread was left there, and the needle withdrawn. The finger was again passed behind the internal pillar, and the needle again made to perforate the internal pillar, the conjoined tendon and triangular ligament half-an-inch above the pubis. The point was brought out at the same aperture in the skin, and then withdrawn, leaving the end of thread externally. The two ends of the thread which passed through the conjoined tendon were brought over towards the external pillar, and the loop which passed through Poupart's ligament brought over to the opposite side. A boxwood pad, cylindrical, three inches long by ¾-inch in diameter, was placed over the external ring in the direction of its long axis, and the crossed threads were tied over the pad. The effect of this was to bring the two pillars and the sides of the canal into apposition, as might be distinctly felt by passing the finger into the opening first made. Lint, a stout pad, and a spica bandage were applied, and the patient was removed to bed.

The effects of this operation are, by approximating the sides of the canal in its whole length, to obliterate the portion which transmits the bowel. The ligatures and pressure which are applied are sufficient to excite inflammation enough to close the canal so that the hernia cannot return. After some days varicocele often appears in the cord, on account of the pressure to which it is subjected from the thickening of the structures. This is considered a favourable sign, as showing sufficient action to be set up; it generally subsides in a few days. Mr. Wood has operated on nine cases, all of which have been successful. The first (fifteen months ago) had a severe attack of bronchitis a few days after the operation; but the hernia did not come down in spite of the cough, and the patient now does heavy work without truss. Another patient had at different times worn thirty trusses, had had the hernia three times strangulated, he had also been operated upon for radical cure by Wützer's method; but the hernia had come down again shortly after the operation. Mr. Wood lately performed his operation on him on board the Dreadnought, and he is now perfectly cured. As a proof of the cure, he had sent his son to Mr. Wood to be operated on in the same manner. In one of the last cases, the patient (who showed himself to-day at the operating theatre) was out of bed in a fortnight, and discharged cured in three weeks. Yesterday he sent in great alarm for Mr. Wood, as he thought the hernia had come down. Mr. Wood went directly to him, and found that it was a varicocele caused by pressure of the contracting strictures on the cord, as occurred in the first case operated on. This is considered to be rather an advantage, as indicating a sufficient amount of pressure in the canal.

In some clinical remarks made after the conclusion of the operation, Mr. Wood stated he considered his method of procedure to possess the following advantages over that of Wützer:—In Wützer's, the skin and subjacent tissues are invaginated and kept in their place by a plug in the canal, and a needle passed through the sac and integuments, very little action is set up, and when the plug is withdrawn, the elasticity of the skin draws down the invaginated portion by degrees, as the adhesions are not sufficiently extensive or strong enough to hold it. The consequence is, that the rupture returns; or else, as the upper portion of the sac only is subjected to pressure by the instrument, if adhesion is obtained it is only in the upper part; and the lower and posterior part, which is the weakest, is subject to the pressure from within as soon as the plug is withdrawn, and a fresh hernia forms in the posterior fold of the invaginated sac. We believe, however, that this only occurs to inexperienced operators. In Mr. Wood's operation, the fascia only is invaginated, and the skin does not tend to pull it back again, as it is dissected from it. Moreover, the lower wound almost always heals up by first intention, and so the skin even helps to keep the fascia in its place, as it adheres to it lower than formerly. The ring also, instead of being further dilated by the invaginated skin and plug, is really restored to its normal dimensions or even lessened, and so prevents the hernia from returning. In Wützer's the instrument has to be worn some two or three months. In Mr. Wood's the ligatures and pad are generally removed on the third or fourth day. The old sailor who has been subjected to both operations, says that Wützer's was far more painful than the other; and it will be seen that it takes a longer time to complete the cure. Again, Mr. Wood's is applicable to all cases of hernia, large and small, direct and oblique, which is not the case with Wützer's.—*Med. Times and Gazette, June* 25, 1859, *p.* 652.

55.—ON THE FORMATION OF AN ARTIFICIAL ANUS.

By Dr. C. Th. Meier Physician to the Bellevue Hospital. N. Y.

[In the former mode of operating in these cases, viz., dissecting down to the intestine and then simply making an incision into it, the fœcal matter always comes in contact with the surrounding tissues and produces irritation and fatal consequences, or the wound in the intestine contracts, and requires constant dilatation. In 1835, Amussat first recommended that the intestine should be brought down to the perineum, opened, and after the discharge of the meconium, that the mucous membrane of the intestine should be attached to the borders of the perineal incision by means of interrupted sutures. The writer then recommends the following mode of operating in these cases, which we give in detail.]

After the introduction of a small silver catheter, to be kept in the bladder as a guide during the operation, and, in female infants, of a good-sized probe in the vagina, a sufficiently long longitudinal incision is to be made in the median line of the perineum, beginning at the posterior portion of the scrotum; or, in female infants, behind the posterior commissure, and terminating at the os coccygis. Then the incision is to be continued through the cellular tissue and the perineal muscles. The pelvic fascia is then incised; the finger and the handle of the scalpel will then be sufficient to clear the way along the os sacrum to the promontory. If the *cul-de-sac* is not discovered at its normal situation in the concavity of the os sacrum, we are then able to carry the finger in every direction, for the purpose of exploring the whole pelvis, and, aided by the catheter and pressure over the abdomen, to discover with certainty the fluctuation of the lower portion of the intestine, if it should happen that it is situated at some point of the anterior or any other portion of the pelvis, provided it has entered the pelvis at all. Should there still exist in the mind of the surgeon a doubt in regard to the fluctuation, the exploring trocar can be used. Being assured that he has discovered the *cul-de-sac* of the lower portion of the intestine, he begins to disengage it from cellular adhesions, mostly by means of his fingers, if it is high up; or he can use a pair of scissors or a scalpel,
is not very distant. This being accomplished, he inserts two fine

double hooks in the intestine, and draws it, or that portion of it that yields easiest, gradually down, separates it again from cellular or fibrous adhesions, if necessary, until the intestine appears at the perineal wound; taking care to make the traction more on the posterior than anterior wall of the *cul-de-sac*. An incision of about three-fourths of an inch is then made in the intestine, between the two little hooks, the meconium is permitted to escape, and the mucous membrane of the intestine is attached very accurately, by a sufficient number of interrupted sutures, to the perineal wound, in such a manner that the fœcal matter cannot come in contact with the surface of the perineal wound, and prevent union by first intention. The remaining portion of the perineal wound is also to be united by a sufficient number of sutures.

These are, in general, the leading features of the operation which I would recommend in cases of imperforate anus of new-born children.

[In an after part of the paper the author continues:]

As the surgeon can in most cases find the end of the rectum in the pelvis, he has only to explore the pelvis carefully, and, after discovering it, to endeavour to establish the anus at its normal situation, since any other method for an artificial anus in the abdominal or lumbar regions is attended with too many dangers, and must be considered justifiable as a last resource only, under two circumstances:

1. Where it is impossible to carry the intestine downward through the pelvis, on account of too strong adhesions, the separations of which would be injurious to the neighbouring organs, and fatal in its consequences.
2. Where the rectum or the termination of the intestines cannot be discovered in the pelvic cavity.

These conditions of the intestine being the exception in most of the cases of imperforate anus, the surgeon should exert all his skill and perseverance to establish the artificial anus at its natural situation.—*American Med. Monthly, June*, 1859, *p.* 444.

56.—*Hemorrhoids and Prolapsus of the Rectum: their Treatment by the Application of Nitric Acid.*—By Henry Smith, Esq., Surgeon to the Westminster Dispensary.—[Mr. H. Smith advocates the practice of employing nitric acid in prolapse of the rectum. As indicating his opinion on this point, we may quote the final paragraph of his treatise:]

It is not necessary for me to relate more cases, as those detailed show that prolapsus of the rectum may, as well as internal hemorrhoids, be cured without any other operation than the judicious employment of nitric acid. It is quite surprising to see the extraordinary comfort which one or two applications of this agent will give to patients who have been suffering years of misery. It will supersede the use of those atrocious pessaries and supports which patients every now and then bring out of their pockets to show us, and which, independently of being very injurious from the dilatation of the gut caused by them, are excessively nasty things, and chiefly calculated to amuse old women and hypochondriacal men, who have nothing else to do but to attend to the state of their bowels.—*Lancet, July* 23, 1859, *p.* 90.

57.—CASES OF PROLAPSUS ANI AND HEMORRHOIDS TREATED BY THE ECRASEUR.

By Redfern Davies, Esq., Surgeon to the Birmingham Workhouse Infirmary.

W. B., aged 54, by occupation a porter, of pale and exsanguine countenance, but otherwise in good condition, has been the subject of prolapsus ani for the last five years; no cause known for it. He was treated about two years ago by an eminent surgeon with the nitric acid plan, which only relieved him, however, for a short time; the gut descending again as bad as ever.

Present State.—Upon an examination (shortly after passing his stool), there is seen to be a prolapse of the mucous membrance of the rectum to the extent of about a hen's egg in size. It is composed of three principal lobules, and is of a dark purplish colour, returnable with a little trouble—now facilitated by his having lain in bed for a few days; on introducing the finger into the anus, the capacity is found to be increased in size. The vessels going to the prolapsus are much increased in magnitude, some two or three of them presenting to the finger a barrel nearly equal to that of the radial artery. (This fact was particularly pointed out at the time of operation.) He states that the prolapse is usually down; the least exertion, as of walking a few yards, sufficing to cause its descent; that he is in constant pain and misery from it; and that defecation is always attended with the loss of a variable quantity of blood, and at other times an offensive ichorous discharge.

May 18. The patient well under chloroform (administered by Mr. Jauncey). The mucous membrane of the rectum constituting the prolapse was dragged down by double pronged hooks, and the whole of it removed with the aid of the écraseur devised by M. Chassaignac. About a teaspoonful of dark blood was lost, chiefly the contents of the prolapse, which were partially squeezed out. A compress and T bandage applied; to take two grains of opium. *Vespere.*—Feels comfortable, and passes flatus. Repeat opium.

20th. He has gone on well. Bandage removed. An injection of warm water given, which produced a copious evacuation, causing no pain, and accompanied by no blood, save a few dark clots about the size of peas. Continue the opium—gr. ij. daily.

22nd. Bowels again opened by injection. No pain—no blood; is up and about the ward.

June 25th. He states that there has been no descent of bowel since the operation, and that his stools of natural calibre, pass without any pain, blood, or difficulty. He is daily increasing in strength and flesh.

A. M., aged 53, has been the subject of piles for the last three years, during eighteen months of which time she has been confined to her bed almost continually, walking or even sitting being attended with much pain and discomfort. Defecation is always accompanied with great suffering and a variable loss of blood. Her general appearance is that of a person in a state of great debility from constant hemorrhages. During the last three months, nitric acid has been freely applied to those piles within the anus, by means of the usual anal speculum. On those occasions its application was, both at the time and subsequently, attended with much pain, without being followed by any amelioration of symptoms whatever.

May 25th. The patient well under chloroform (administered by Mr. Jauncey). Five large hemorrhoidal clusters were drawn down, and the chain of the écraseur having been adjusted at their base, they were removed, with the loss of not more than half-a-teaspoonful of dark blood.

A compress and T bandage. To take gr. ij. opii.

In about half-an-hour after operation, she suddenly got out of bed, intending to make water, and the straining so to do caused her to lose about half-an-ounce of blood and she became faint.

The same treatment as above was adopted.

Save this trifling accident she has progressed very favourably, and is now able to walk about the garden without discomfort, and has neither pain nor loss of blood in passing her stool.

Remarks.—The chief point worthy of notice in these two cases is the demonstration of the great and practical value of the écraseur in these affections. In both instances a speedy and safe cure has been effected, and that after the nitric acid plan has failed.—*Med. Times and Gazette, July* 9, 1859, *p*, 31.

58.—*Prolapsus Ani.* By Dr. THOMAS WOODS, Parsonstown Workhouse Infirmary.—Biddy Grattan, aged six years, suffering from prolapsus ani for nearly two years. The prolapsed part cannot be returned; it is about three inches in length, and the size of a lemon. The mucous membrane lining the tumour is congested. The child is emaciated and cadaverous. A large abscess is forming on the thigh. There is occasional diarrhœa, and no appetite.

The prolapsed rectum was rubbed over with nitrate of silver in substance. The protruded part became smaller, and the nurse was able to return it, but after a few hours it came down again. Strong nitric acid was then applied, by drawing a glass rod, dipped in the acid over the protruded bowel, a quarter of an inch wide on each side. The bowel was then returned, but it prolapsed the next day. The application of the nitric acid was repeated, a little more liberally; the bowel was returned, and remained so for two days. The child got meat and ate it. When the bowel protruded, it could be seen that where the nitric acid was applied the mucous membrane was removed. The nitric acid was again applied, the bowel returned, and it did not again come down. The acid was put on the prolapsed bowel by drawing it in the way first mentioned four times in the week; and the child perfectly recovered. The abscess which was forming on the thigh disappeared by absorption; and in about two weeks after, the bowel remained permanently up. The child —who for the last two years was a most miserable-looking object, emaciated, suffering, and constantly confined to bed, with a poultice to the prolapsed rectum—gained a healthy and cheerful look, and became the fattest pauper in the house, with good use of her limbs. The cure is permanent.—*Dublin Quarterly Journal, August* 1859, *p.* 18.

59.—*Fissure of the Anus.*—One of the most painful affections situated in the neighbourhood of the anus, is a fissure alongside of the sphincter. When examined, scarcely any lesion is to be detected; but on rendering the structures tense, a very small slit with reddened margins may be observed, and from which there may be a little secretion. This apparently trifling malady occasionally causes the most intense agony. Latterly, several examples have come under Mr. Hancock's care at the Charing-cross Hospital, which have been effectually cured by the division of a few of the muscular fibres of the sphincter at the situation of the fissure. It is unnecessary to divide the entire sphincter in the treatment of this affection, and it is now seldom resorted to. On the 2nd instant, this operation was repeated by Mr. Hancock on a woman twenty-seven years of age, whose sufferings have been very great for nearly twelve months, from the presence of a fissure of the kind mentioned. On passing her motions, the sensation was compared to that of a knife running through her. When we saw her on the 7th, five days after the operation, she expressed herself as completely relieved; all pain had gone, her health had generally improved, and she was beginning to assume a cheerful aspect. Patients with a fissure of the anus have a careworn and anxious expression of countenance, more so than is observed in fistula of the bowel; but it quickly disappears when surgical relief has been obtained.—*Lancet, July* 16, 1859, *p.* 61.

60.—*Double Fistula in Ano treated by a Single Division of the Sphincter.* —Although at first sight it may seem to be a trifling matter, whether one or more divisions of the sphincter ani muscle be made in cases of complicated fistula about the anus, in reality considerable importance should be attached to it if the future comfort of the patient is considered. There can be no doubt whatever, as we heard Mr. Fergusson remark, at King's College Hospital, on the 2nd instant, that if there are two or more divisions of the sphincter muscle, subsequent union does not permit of such an amount of control over its functions as when only one is made. Being aware of the truth of this from experience, he treated the case of a young woman, who had what might be called a double fistula, in the following manner:—Three years ago she had an abscess in the perinæum, which burst externally at the margin of the anus; probably a second formed, which also burst externally, but the two cavities merged into one. This aperture, on examination, was found not to communicate with the rectum, and was, therefore, what is called in surgical language, *a blind external fistula*, with a double opening, Instead of running a bistoury through the sphincter in two places, as we have seen done by some surgeons, Mr. Fergusson divided the skin between the fistulæ, and laid open the cavity to which they were the outlets. He then cut through the sphincter nearest the upper fistulous opening, in the usual manner, and the wound was carefully dressed from the bottom. Thus, by a very simple proceeding, the case was converted into one of ordinary fistula in ano.

The practical surgeon will at once recognise the benefits to be derived from an avoidance of multiple divisions through the sphincter ani.—*Lancet, July* 16, 1859. *p*, 61.

61.—ON THE TREATMENT OF SINUS BY THE INJECTION OF IODINE, PARTICUARLY FISTULA IN ANO.

By Dr. Thomas Skinner, Liverpool.

[In 1830 Lugol first proposed the use of iodine injections in the treatment of various affections, as scrofulous abscess, sinuses connected with disease of the hip and other joints, and caries of the bones. Subsequently Dr. Clay, of Manchester, recommended it in fistula in ano.]

During the month of Nov. 1856, I was initiated into the practice by my esteemed master, Professor Simpson. The first case I injected was one of fistula *in ano*, and it is referred to by Dr. Simpson in the Medical Times and Gazette of 16th of April last. The patient came from the north of Ireland, and had suffered for many years from the disease. By Dr. Simpson's direction, I attempted to inject the fistula; but from the extreme smallness of the orifice, I failed to do it justice. On informing him of this, he suggested the nozzle to the syringe represented in the accompanying woodcut, which I will describe immediately. The patient being again prepared by an aperient and an enema, I injected the fistula, and this time I caused the injection to appear at the internal or rectal orifice. The strength I then used was two drachms to an ounce of spirit (No 3, as opposite), and the quantity I injected was under thirty minims. There was scarcely any pain complained of, and in three days the fistula was entirely and permanently cured. Within a week of the operation, it withstood the powerful test of the hydraulic pressure of one pint of tepid water injected into the rectum, not one drop of which passed through, or in the slightest degree distended the cicatrised cutaneous orifice of the fistula. This case was seen again three months afterwards, and there was no sign of the disease beyond the sunk cutaneous cicatrix; and some months after that, Dr. Simpson received information that the patient was in the enjoyment of the best health and spirits, the old enemy being perfectly quiescent. I have taken the liberty of detailing this case of Professor Simpson's, as it was my first attempt at the practice; and I do so with less hesitation, as he has already associated my name with it. I have permanently cured other three cases in the female, one of which is fully detailed at page 499 of the British Medical Journal for 1859, and which is especially interesting, as showing, that fistula *in ano* may be an efficient cause of sterility. It also shows, that a formidable fistula with three cutaneous sinuses and apertures communicating by fistula with an ulcer in the rectum, may be cured with little or no inconvenience and without any danger, in the short space of three days. The means adopted were a simple longitudinal incision through the mucous membrane including the ulcer, and a single injection with tincture of iodine through the fistula. The details of the other cases would only occupy unnecessary space; so I will content myself with offering a few practical observations.

Practical Remarks.—Dr. Clay took three weeks to effect a cure in his case; I suspect this was owing to the weakness of the tincture he used, and the plan he adopted. He injected the fistula every day for the first three days, and then every other day for the next twelve: in all, nine injections or dressings. Now, I cannot but think that this does harm, as it tears up the adhesion of the previous day, and leaves little chance of consolidation. Dr. Clay says: "The tincture could not be detected in the rectum after the second injection." (Medical Gazette, Sept., 1843.) I am inclined to believe that if the injections had then been discontinued, the treatment would not have extended to three weeks. We must remember that the force of an hydraulic instrument, like a syringe, is very great: the stream is a most insinuating and powerful wedge, particularly when the bore of the nozzle is very fine.

M. Boinet and Dr. Clay inject one or two teaspoonfuls of their tincture. I have never used more than thirty minims of any of my own, and I have often succeeded with less. M. Boinot uses a "knobbed canula," which is objectionable, unless the orifice is dilated. He recommends a finger to be placed on the internal opening in the rectum, and the injection to be retained for five or six minutes. But, in my opinion, the finger on the internal orifice will not prevent the injection from passing into the rectum; and if the injection is strong enough, retention of it is unnecessary.

Preparations and their Strengths.—In regard to the strength of the solution for injection, it varies with each observer. M. Lugol's solutions are as follows:—

	No. 1.	No. 2.	No. 3.
Iodine....................	gr. ii	gr. iii	gr. iv
Iodide of potassium........	gr. v	gr. vi	gr. viii
Distilled water............	lb. i	lb. i	lb. i

These were intended for injecting the fistulous tracks leading to carious joints; and, although these strengths are said to be attended with danger, "causing occasionally intense inflammation and sloughing of the soft parts" (Manual of Therapeutics, Waring, sect. 1465); yet, I am certain they will be found almost inert in fistula *in ano*. I rather incline to think, that the damage done to the soft parts by such injections is owing to the injury first done to the diseased joint. M. Lugol used No. 2 in fistulæ connected with *morbus coxarius*.

Dr. Clay used the ordinary official tincture of the Pharmacopœia, full strength, which is very good for some cases, but it rarely succeeds by one injection, so far as fistula *in ano* is concerned.

M. Boinet's injection solutions are of two strengths:—

	No. 1.	No. 2.
Iodine................................	ʒ ss	ʒ i
Iodide of potassium...................	gr. xv	gr. xv
Distilled water.......................	℥ ii	ʒ x

I object to the iodide of potassium and water. I believe that the less there is of water, and the purer the iodine, the greater certainty there is of success. I use three strengths, as follows:—

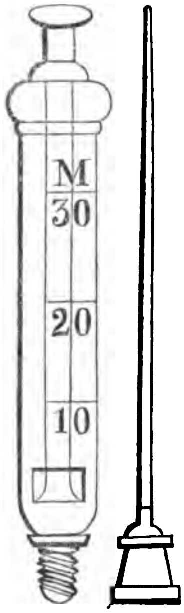
	No. 1.	No. 2.	No. 3.
Iodine.........................	ʒ ss	ʒ i	ʒ ii
Sulphuric ether................	q. s.	q. s.	q. s.
Spirit of wine.................	℥ i	℥ i	℥ i

The sulphuric ether is added simply as a quick solvent instead of iodide of potassium. I find that a tincture composed purely of sulphuric ether and iodine is best, but it will not keep at the same strength for one day. I have, in consequence, lately been in the habit of extemporising such tinctures at the moment, for various purposes to which I apply them. They are as follows:—

	No. 1.	No. 2.	No. 3.
Iodine....................	gr. iiiss	gr. viiss	gr. xv
Sulphuric ether...........	ʒ i	ʒ i	ʒ i

In preparing these tinctures, the following facts must be borne in mind. The iodine should be dry and resublimed. The ether should not be of less specific gravity than ·750; of density ·725 it is useless, as it cannot be drawn into, far less retained in the syringe. It should be prepared in a test tube with a well fitting cork. By using such tinctures, the ether evaporates at once, and leaves a perfect coating of pure iodine on the walls of the fistula or sinus.

Description of Syringe.—My own method is little different from the plan first suggested to me by Professor Simpson. I will first describe the syringe; the accompanying woodcut will assist. The cylinder and piston are such as belong to Dr. Alexander Wood's syringe for hypodermic injection; the nozzle is the important part. It is composed of a very fine gold tube, so as to resist the corrosive action of the iodine; the tube is about one-sixteenth of an inch in external diameter, so as to pass into the finest fistula the same as a delicate probe. The tubular portion ought to be, at least, two and a half inches long, fixed into a silver socket with *hard* solder, as iodine destroys soft solder. The silver socket has a female screw in it adjusted to the male screw of Dr. Wood's syringe. Any one possessing Dr. Wood's syringe may have the extra nozzle supplied by Mr. Young, cutler, Princess Street, Edinburgh. After using it for this purpose, it should always be cleaned by pumping dilute liquor potassæ through it; and before using the piston for narcotic injection, fresh cotton should always be applied.



Operation.—Prepare the patient with an aperient and enema. If it is a *complete* internal fistula, which may be easily diagnosed by dilating the rectum with Weiss's female dilator, or with the bivalve anal speculum of Mr. Ferguson, of Giltspur Street, and injecting from the external opening a little sweet milk. (There is no probe for this purpose like a hydraulic one.) If the fistula is *complete*, place the fenestrum of the speculum over the rectal orifice. Clear out the sinuses by injecting them with plain tepid water. Insert a little loose cotton wool into the speculum to absorb any excess of injection, as also to protect the mucous surface of the rectum and anus. The stain on the wool, if any, will witness to the passage of the injection. Fill the syringe with the iodine solution of the selected strength, expel all air from it, pass the point of the nozzle a little way into the fistula, and force the injection through it. If there are any cutaneous sinuses, with a little dexterity, they may also be injected at the same time; or, the instrument may be refilled, and every sinus injected, although this is not always absolutely necessary. Previous to the operation, or as soon as convenient, I introduce a suppository into the rectum containing half a grain of morphia.

If the fistula is *incomplete*, or blind external, no speculum is required, but it is necessary to attend to the following particulars:—Dilate the external orifice if necessary, and sound the sinus delicately with a blunt pointed probe, avoiding to make false passages. Having found the "bearings" of the sinus, clean it out with tepid water, pass in the gold tube of the nozzle as far as possible and inject. If this is not attended to, and a portion of the pyogenic surface or membrane is left untouched at the cæcal extremity, failure may result. It is not necessary that the injection should be retained; I generally press the parts immediately, so as to expel as much as possible; it seems sufficient that the surfaces are touched.

Where the internal aperture of a complete fistula opens into an ulcer, the ulcer should be incised immediately before injecting. It will certainly heighten the pain, but I believe such incision to be essential. Or, it may be more conveniently accomplished by two operations; first, incise the ulcer, which will be healed in three days or so, and as soon thereafter as is thought desirable, inject the fistula. I followed this plan lately, in a case where I was kindly assisted by my friend Dr. Taylor, of Walton Lodge, who first suggested the idea of dividing the operation.

Regarding the repetition of the injection, I would remark, that, if the fir is properly performed, and of the proper strength, a second operation oug not to be required. In the event of failure, I do not repeat the injection before the expiration of the third day; and I am convinced that, if such is necessary, on each occasion the solution ought generally to be increased in strength. I have only once known No. 3 fail in producing adhesive inflammation in cases of fistula *in ano*, by a single injection, the cause of failure seemed to be over action; and I have not met with a single bad consequence, pain excepted. To judge of the proper strength to inject, I am guided by the age of the fistula and by the pain produced while probing.

Subsequent effects: Pain and the Stains of Iodine.—Although in many patients pain is little complained of, in others it may be so great as to require the immediate administration of chloroform, which may be given without fear, so long as the patient suffers pain, notwithstanding the previous half grain of morphia. Suppositories and poultices of ice will also be found useful. Water, or a weak solution of liquor potassæ, is a good antidote, the latter converting the iodine into the iodide of potassium. A still better may be found in an emulsion of starch, which has the power of resolving the free iodine into the perfectly inert iodide of starch.

In the event of any stains of iodine getting on the linen, whether blue from the presence of starch, or brown from its absence, a solution of liquor potassæ in proportion to eight or ten of water will speedily and safely remove them. For this useful information, I am also indebted to my friend Dr. Taylor. It is much safer than the cyanide of potassium, as any stains on the skin of the patient may also be safely removed by it.

Contraindications.—If the patient's health is in a low condition or if the sinus or fistula is of recent date, or if there is a pouch-like cavity on introducing the probe and sounding, I do not operate. In such cases, the patient should be placed on good diet, with change of air and sea-bathing if possible, or the cold sitz-bath with alum, the cold douche, or cold sponging. At the same time, the fistula should be injected occasionally with a solution of tannin or of tincture of steel. In short, all means, constitutional and local, which are calculated to improve the health and produce contraction and consolidation of the cavity, ought first to be adopted; the injection of iodine may then be used with effect. If any local or constitutional irritation, or more than usual inflammatory action is present, it first must be subdued by proper means.

The after-treatment need scarcely be mentioned. A little opium, to allay pain and keep the bowels quiet, with ordinary diet, and keeping the patient at rest for a day or so, are about the only directions necessary.

I have been thus minute in details, in order that there may be as little excuse as possible for reports of failure by those who may try their 'prentice hand at the operation.

Possibility of its return.—In the case I recorded in the Journal, the fistula was closed upwards of nine months. The cause of the return was a fresh phlegmon, established in the site of the old fistula during the course of an attack of scarlatina. It ought not to be wondered at, if the fistula should also return in cases where the mucous membrane is perforated extensively by ulceration. In these cases, there are often hemorrhoids, accompanied with dilation of the rectum, and possibly, ball-valve obstruction—these conditions being very common in the female. In such patients, and those who suffer from habitual constipation, the mucous membrane becomes jammed and lacerated by the undue pressure it is exposed to between the hardened fæces and the point of the coccyx, more especially during the act of defecation. Under such circumstances, perforation of the mucous membrane again takes place, fæcal matter gets into the cellular tissue, fresh abscess and fistula result, and we feel disappointed. Why? because we do not consider that similar causes will always produce similar effects, whether we incise or inject, or whatever method we may adopt for the purpose of cure. Measures should be adopted to obviate the above conditions, if they exist, otherwise disappointment will certainly follow.

Advantages.—I am not so enthusiastic as to suppose that injection with iodine is at all likely to supersede the knife; no one can possibly have greater confidence in the knife than myself; but it is a difficult matter sometimes to inspire our patients with the same amount of confidence and love for it. I advocate the claims of iodine as being next in certainty to the knife, and as being infinitely less dreaded by the patient. When we contrast the perfect innocuousness of injection (barring pain), its simplicity, and its wonderful efficiency, with the possible and not at all improbable dangers of incised wounds of the anus and rectum, and as " we ought to do unto others as we would be done by," I cannot help suggesting that injection with iodine ought, in the majority of cases, to take precedence of the knife.

Concluding Remarks.—I will only add, in conclusion, that it has been very successful in my hands; in fact, it has rarely failed. I have injected sinuses of many months standing, following bubo in the groin, with No. 1 or No. 2, and I have always been successful at the first injection. In one such case, a gentleman had been most carefully treated by a skilful practitioner with cod-liver oil, syrup of the iodide of iron, and other tonics, sea-bathing with change of air, cold sponging, and such measures, but all to little purpose; the sinus kept on discharging. One single injection of No. 1, without the slightest pain or inconvenience, did all that was requisite. I gave him no prescription, which seemed to discourage him; but next day he reported himself cured. He has since been to Montreal and back on mercantile business, and the sinus remains perfectly healed. Sinuses about the nipple, mamma, axilla, and vulva, I have treated with similar success; also sinus in the gums following gumboil.

About a month ago I had a case, where I had good reason to suspect a carious condition of the root of a tooth, or rather of the alveolus; I requested the lady to see a dentist, but she declined. I injected the cloaca once with No. 3; the *fœtid sanious* discharge ceased, and the mucous surface closed over the opening. I would here remark, that whether this was a case of genuine caries or not, considering the success obtained by M. Lugol in the use of iodine in this disease, it deserves a further trial. M. Lugol used baths and frictions of iodine, at the same time that he injected the fistulæ with his solution No. 2. I would not recommend injection in the caries of joints, or where the disease is deep-seated, nor in the immediate neighbourhood of serous or synovial membranes. If it is to be at all successful, I think it should be limited to superficial caries, where the disease is not extensive or very far advanced, and in those cases only which are likely to be benefitted by a stimulating and caustic treatment. Caries of the bones of the tarsus would probably be the most favourable for the treatment. I would first clear out the diseased part with tepid water, and then inject from five to ten minims or more of the *ethereal* solution No. 3, and I would not repeat it until the effect of the first injunction had passed off. Constitutional treatment, as a matter of course, must precede, accompany, and follow the local means. I have, however, had no experience of injection of iodine in caries beyond what I have stated. Lastly, I was asked by a lady to see her servant, whose appearance was disfigured by scrofulous enlargement of the glands of the neck, with two of them discharging slightly. I injected them with No. 1; they have ceased to discharge, but how long they will continue so, I cannot say.—*Brit. Med. Journal, Sept.* 3, 1859, *p.* 721.

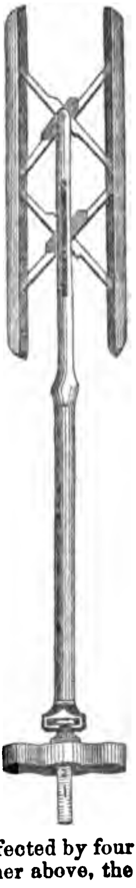
62.—REMARKS ON STRICTURE OF THE RECTUM.

By Armstrong Todd, Esq., Surgeon to the Marylebone Dispensary.

[The great difficulty experienced in treating cases of stricture of the rectum is from the anus and sphincter muscle being extended to the same degree as the bowel and stricture are themselves. These parts are much more irritable than the rectum itself, besides being naturally, even when fully distended, of much smaller calibre than the bowel above. Thus much useless annoyance and pain is caused.]

The causes of stricture may be considered to be—Firstly, a submucous deposit resulting from inflammation, which becomes of a fibrous character, having a great tendency to contract. It forms a ring round the mucous membrane, so that any contraction of it must lessen the calibre of the rectum. Secondly, stricture may arise from cicatrisation of ulcers; and, Thirdly, from cancerous or other deposits. Over this latter form surgery has little control. Dilation in such cases cannot be expected to prove beneficial. In the first two forms, however, I believe perfect cure may be accomplished if proper extension be employed, and if it be continued for a sufficient length of time.

In the first form of stricture the effused lymph, although it appears to have become hard and resisting, yields pretty readily to continued and persevering pressure, and under this treatment it can be caused to absorb, so that a small-sized bougie is in a short time succeeded by a larger one, this again by a still larger, and so on until one of considerable diameter is admitted; but dilatation here becomes limited by the sensitive character of the anus, and the total absorption of the stricture is therefore not effected. If such be the case, although the patient may derive considerable benefit for a time, the contractile propensity of the stricture is, on the discontinuance of the bougies, permitted to proceed, and the bowel again becomes narrowed. The second form, viz., that produced by cicatrices after ulceration, also yields to the pressure of dilatation; in fact, all contractions of cicatrices, even those resulting from burns, yield to extension made by mechanical contrivances. This has been fully shown by my friend, Mr. Tamplin, who has successfully used this treatment at the Orthopædic Hospital in cases of contracted cicatrices.



In this form of stricture the obstruction does not alone proceed from the cicatrisation; there co-exists always some plastic effusion resulting from the inflammation, which, taking the fibrous character, I believe, performs a very great, if not the chief part, in obstructing the bowel. Thus it is that after ulceration one finds a stricture of considerable tightness, which from simple attention to the general health, occasionally becomes resolved without any mechanical treatment whatever. In such cases the effusion lymph is reabsorbed before it has taken on that fibrous character which requires dilatation. It is, therefore, unnecessary to interfere with this form of stricture in its early stage, and such practice is frequently injudicious, as any attempt at dilatation may rekindle inflammation. Dilatation, then, seems to be the only means we possess of causing the obliteration of stricture of the rectum; but in order to make sufficient pressure by extension so as to produce complete absorption, it becomes necessary to construct an instrument, which will admit of the rectum being stretched to its full extent, while the anus and sphincter are allowed to remain in their natural contracted state. In all cases dilatation requires to be conducted in a judicious and gradual manner, and, therefore, in the construction of any instrument for dilating purposes this requirement necessitates strict attention.

The adjoining woodcut is of an instrument which has been made from my directions, by Mr. Ferguson, of Giltspur-street.

I have drawn it in a partially extended position, so as to show its action more perfectly. It consists of two blades of finely-polished steel, forming, when closed, a small-sized oval bougie. These blades are about three inches and a half long, rounded above and below, and made to separate from and approach each other in a parallel direction, by mechanism contained within. Beneath these is a round stem, quarter of an inch in thickness, upon which the anus and sphincter are allowed to contract. The parallel movement of the blades is effected by four slight bars of steel, placed in pairs—one pair crossing each other above, the other below, united at their intersection by a pivot. The extremities of each pair, at the centre of the blades, are connected together and to the centre of the blades by means of hinges, their distal extremities being permitted to traverse a groove within the blades. The stem before spoken of is hollow, and is continued above, within the blades, into a fork, the extremity of which is attached to the pivot connecting the intersections of the superior crossbars. Through this hollow stem passes a rod, which also ends above in a fork, attached in a similar manner to the pivot through the intersection of the inferior crossbars. The other extremity of this rod is a screw, on which is a graduated scale; to this a thumb-nut is fitted, having a rim on its upper part, which revolves in a groove in the extremity of the outer or hollow stem.

The effect of this mechanism is, that when the nut is turned from right to left, the inner rod is pushed up, and the intersections of the crossbars are made to approximate, the horizontal diagonal of the central qradrangle becomes, therefore, elongated, and thus the blades are separated. A contrary movement of the nut draws down the rod, and brings the blades together. The screw is made so fine, that dilatation can be effected by an exceedingly gradual movement.

This instrument may be found useful in other surgical operations. In dilatation of the vagina, in Lloyd's operation for lithotomy, it would be found advantageous: also in the cure of fissure of the anus by extension. In this affection it would permit of unctuous applications, or those of solutions of nitrate of silver; or even it might facilitate the division of the fibres of the sphincter. And if the internal mechanism could be made fine enough, it might become applicable to stricture of the urethra, when its parallel expansion would be found very advantageous.

I used this instrument, in the case of a lady who had suffered for a very long period from inflammation of the rectum and fistulæ. After the latter had been operated upon, and all became perfectly healed, a stricture, which co-existed with the fistulæ, engaged my attention. Two months having been spent under treatment by bougies, the anus would only admit of one five-eighths of an inch in diameter. On the introduction of the above instrument, almost immediately, by cautious and gradual extension, the stricture was dilated to an inch and an eighth, without the least pain or unpleasantness to the patient. She also stated that she could retain the instrument as long as I wished, as it caused no inconvenience whatever.—*Med. Times and Gazette*, *Aug.* 6, 1859, *p.* 130.

63.—*Enormous Ranula.*—At the Cancer Hospital lately, under Mr. Weedon Cooke's care, was an eccentric old woman, who has had a ranula for some years, which had grown to the size of a large orange. When in the mouth it protruded one cheek in the most unsightly manner, and when allowed to hang out of the mouth it was like a transparent jelly-bag. She was in good health, and able to eat and talk with but slight inconvenience. Many surgeons have seen it, and wished to operate; but she steadily resisted all such interference. Not being allowed to snip out a portion of the membrane, Mr. Cooke proposed the application of potassa fusa, to make an aperture which would not close up readily; but this also she decidedly objected to.

It is very seldom that a ranula is seen larger than a walnut or pigeons egg, because when it attains that size under the tongue, it pushes this organ upward and backward, and sometimes most seriously interferes with both speech and deglutition. If the cyst continues to increase in Mr. Cook's patient, it may spontaneously rupture, and partial relief be thus brought about. Such a ranula as this cannot be said to be a dilatation of Wharton's duct.—*Lancet*, *Aug.* 20, 1859, *p.* 186.

64.—*Ascarides.*—Dr. Compéral has got a cure for ascarides, which has never failed in his hands. It is a simple injection of water, containing five, ten, fifteen, or twenty drops of sulphuric æther, according to the age of the individual, and repeated more or less frequently, according to the number of the animals present. This agent, he says, has a double advantage. By its subtility it readily enters into and destroys the larva; and by its antispasmodic powers it allays the spasmodic and nervous symptoms produced by the animals.—*Med. Times and Gazette, Aug.* 27, 1859, *p.* 223.

ORGANS OF URINE AND GENERATION.

65.—ON CASES OF MEDIAN LITHOTOMY; WITH REMARKS UPON THE OPERATION.

By Claude Wheelhouse, Esq., Surgeon to the Leeds Public Dispensary, and Lecturer on Anatomy and Physiology in the Leeds School of Medicine.

[Experience has shown that there is far greater power of dilatation inherent in the prostatic urethra than was formerly supposed, and the microscope has revealed that, so far from being truly glandular in its structure, very little gland tissue is found in it as compared with involuntary muscular fibre; and Professor Ellis even asserts that it has a direct sphincteric action on the contained urethra. The rapidity of the contraction subsequent to dilatation is very remarkable, sometimes, in a few seconds after the removal of even very large calculi, being so contracted as not to re-admit the passage of the finger into the bladder, except under the influence of renewed dilating pressure. One of the chief sources of danger against which we have had to guard has been the total division of the prostate. If, therefore, we find that the prostate is sufficiently dilatable, without any division at all, to allow even large calculi to pass through it, and that the urethra may be so opened as to allow us thus to extract them, one main danger of lithotomy is overcome.]

But neither is this too free division of the prostate the only source out of which, in lateral lithotomy, the danger of urinary infiltration may arise. The sphincteric fibres of the prostate being divided, all control is lost, for the time being, over the contents of the bladder, and the urine, continuing to dribble through the wound for many days, is unceasingly in contact with its cut edges, rendering the patient still further liable to the same danger, should healthy lymph fail to be thrown out over their surfaces.

By reference to the accompanying cases, it will be seen that by median lithotomy this second source of peril is also removed; for it is worthy of observation that immediately from the time of the operation, the neck of the bladder being uninjured, the patient possesses perfect control over that viscus—can empty it at will, and in stream—and that, having done so, he can be washed and made clean and dry, and thus be kept perfectly comfortable between each act of micturition; and furthermore, since the posterior layer of the deep perineal fascia remains undivided, it must so guard the areolar tissue of the pelvic cavity from infiltration *during* the evacuation of the bladder as to render that accident almost impossible. Again, the liability to subsequent erysipelas is greatly lessened by the fact that the patient occupies a clean, *dry* bed, instead of lying for many days in sheets saturated with decomposing, and therefore ammoniacal, urine. If the operation be carefully performed, it is hardly possible to wound either the rectum or the artery of the bulb—impossible to wound the pubic artery; and though there may be some considerable amount of hemorrhage from the prostatic venous plexus, this will not often be such as to give rise to serious apprehension or alarm. So far, its results in the hands of our Leeds surgeons have been very encouraging: only one death has occurred in seventeen operations.

Case 1.—June 30th, 1858. C. O., aged seven years, a puny, ill-grown boy, presenting all the appearances of one who had long been the subject of great suffering, thin, cachectic, and scarcely able to move about, was shown to me as the subject of stone; and on passing a sound into the bladder, a calculus was readily detected. His sufferings during and after micturition were described as so great as to render the child an object of commiseration by his neighbors, and such as to lead his mother willingly to accept the risk of any operation by which they might be relieved. On careful examination, the urine was found free from any condition contraïndicating the operation of lithotomy, or leading to the supposition of any disease of the kidneys; and since the bladder was also believed to be free from morbid change, the operation was decided upon. A dose of castor-oil at bed-time, and an enema of warm water on the following morning, were the only preparation required; and on the 30th of June, I proceeded to operate by Mr. Allarton's "median method," the child being first placed under the influence of chloroform.

A curved staff, grooved on its posterior aspect, was passed into the bladder, and held by an assistant firmly hooked up against the pubes. The forefinger of the left hand was then passed into the rectum, and allowed to rest against the apex of the prostate, and with this finger the staff could be felt entering the body. A long, sharp-pointed knife was then made to enter the perinæum immediately in front of the anus, and passed deeply towards the groove in the staff, into which it was directed, at the apex of the prostate, by the finger in the rectum. The membraneous urethra and tissues of the perinæum were then laid open by one sweep of the knife, from behind forward, to the extent of about an inch and a quarter. A probe was next passed along the groove into the bladder, and the staff withdrawn. On then passing the oiled finger into the wound, it was found to pass with the most perfect ease through the prostatic urethra, and the stone was felt at once. It was readily seized by lithotomy forceps passed in upon the finger, and extracted by one or two semi-rotatory movements without the smallest difficulty.

Very little blood was lost during the performance of the operation, and the child was placed in bed before the effect of the chloroform had passed away. The calculus was a phosphatic one, pear-shaped, seven-eighths of an inch long, six-eighths broad, and five-eighths deep.

[The child recovered without a single unpleasant symptom, the urine for two or three days being voided principally by the wound but by the end of the week flowing wholly through the urethra.]—*Lancet, May* 28, 1859, *p.* 531.

66.—*Different Modes of Performing Lithotomy in the English Hospitals.*—A large majority of English surgeons employ the ordinary lateral method of lithotomy on a curved staff. There has been, however, a considerable disposition to endeavour to improve it of late years. The median plan, so strongly recommended by Mr. Allarton, has been tried by not a few London surgeons, and amongst provincial ones has found a warm advocate in Mr. Teale, of Leeds. At the London Hospital it was first adopted by Mr. Ward about two years ago, and since then has been employed by his colleagues, Mr. Critchett and Mr. Gowland, each in a single instance. All the three patients were children, all recovered well, and in all it was considered that much less than the usual amount of bleeding took place. At Guy's Hospital, Mr. Cock has performed median lithotomy several times, and Mr. Erichsen has done the same at University College Hospital, both surgeons being, we believe, well satisfied with its results. On all hands it is considered to be the best adapted for children and for small stones. At St. Bartholomew's, Mr. Lloyd still continues to operate in all cases by his recto-urethral (median) method, which we described in detail when he first adopted it in 1853. He informs us that he has not yet lost a case after it, and considers it decidedly preferable to the lateral operation. His colleagues, however, without exception, we believe, always employ the latter. At the Metropolitan Free, Mr. Hutchinson always employs his rectangular catheter-staff, and considers that he obtains great advantage from it. The same instrument has been employed at King's College by Mr. Lee, but it is not, as far as we observe, in use at any of the other Hospitals. In a recent instance in which the calculus was of large size, Mr. Hutchinson injected the bladder with oil instead of water, in the hope of facilitating the dilatation of the parts.

With regard to the median operation as advised by Mr. Allarton, it is universally admitted to be adapted only for small calculi. Now, Mr. Lloyd's experience during the last few years has quite proved, that when the anterior commissure of the sphincter ani is cut clean through from the perineal wound, there is no danger of the parts not healing. Might it not be well, therefore, to adopt this measure whenever, after the usual median incisions, the stone has been reached and is found too large for removal? Mr. Lloyd's operation gives abundance of room. *Med. Times and Gazette, July* 9, 1859, *p* 34.

67.—ON LITHOTRITY.

By Frederick C. Skey, Esq., F.R.S., Surgeon to St. Bartholomew's Hospital.

[The author commences by stating that the larger his experience of calculous affections, the stronger is his conviction of the well-marked superiority of the operation of crushing over that of lithotomy. He approves of Allarton's mesial operation, which has recently been tried by Mr. Lloyd.]

The staff being introduced into the bladder, the sphincter ani muscle is divided in front, a kind of speculum being passed into the rectum, for the purpose of rendering it tense. The urethra is then opened through the upper wall of the rectum, the remaining part of the canal is dilated up to the bladder by the forceps, and the stone extracted. I employ the term *dilated* because it is the term in common use, but I do not believe that the urethra, especially of a child, is susceptible of such rapid, or rather sudden, dilatation, without some rupture or laceration of structure. It cannot be effected by mere stretching, by which the walls of the canal are enlarged to a circumference of at least three or four times their natural magnitude. I do not mention this feature in the operation as detracting in any great degree from its merit. Dilatation of the prostatic portion of the canal has always been deemed a commendable feature in the operation, and if it be coupled with laceration, it is laceration in detail, the parts so lacerated being restored to their natural contact on the removal of the cause. I am informed by Mr. Lloyd that he has hitherto experienced no difficulty in the restoration of the functions of the sphincter muscle. The operation, to all appearance, is performed without difficulty, and the loss of blood is remarkably small.

No amount of excellence to which the operation by means of the lithotrite can be carried will ever supersede that of lithotomy—most especially, and for obvious reasons, in the case of children, in whom, fortuaately, the mortality is far less than when the operation is undertaken at a more advanced period of life; also in some examples of disease in the adult. But I believe such selections should be comparatively few, and should obtain only as exceptions to a prevailing rule.

I propose now to call your attention to two causes on which I have recently operated in private. In the first of these the operation was successful, and although its progress was marked by symptoms of an untoward kind, the stone was entirely removed within twenty-seven days from the date of the first operation. The second case was unsuccessful, and on that account I give the particulars.

Case 1.—A gentleman of 41 years of age, stout in build, but healthy, became the subject of stone in the bladder, the signs of which might be traced back to a term of eight months. Having ascertained that he had a healthy urethra, admitting a No. 10 catheter without difficulty—that his bladder was so far tolerant of urine as to permit its retention for three or four hours, and that the urine itself was free from morbid deposit, I broke the stone across and withdrew the instrument. The operation occasioned so little pain that the gentleman dressed himself, and subsequently took his chair at the dinner table, at which he ate moderately, but with fair appetite. Within twenty-four hours he had passed some small fragments of lithic acid calculus, but without pain or inconvenience. On the fifth day I repeated the operation; but on this ocaasion, having now acquired some experience of the liabilities of the bladder, I broke the fragments by eight successive applications of the lithotrite. The pain of this second operation, although it somewhat exceeded that of the first, was by no means severe; but I directed my patient to lie in bed, and to drink largely of barley water and other diluents. On the following day he had passed a considerable quantity of detritus, the aggregate of which would have filled a large thimble. A less quantity passed on the second day, when he began to complain of pain in the bladder, and his urine deposited adhesive mucus. The pain became considerable. The adhesive mucus increased. He took mercury-and-chalk, with full doses of Dover's powder, night and morning, with infusion of diosma two or three times during the day. The pain subsided, and the mucus diminished in quantity; and on the eighth day I repeated the operation, and crushed the stone nine consecutive times. Catarrh of the bladder followed as before, but he passed in the course of thirty-six hours a yet larger quantity of fragments than on the former occasion. The symptoms of what is termed chronic inflammation ran high, the mucus appeared in large quantity and was tinged with blood, appetite and sleep failed, his pulse rose to 100, and he was decidedly ill. The former treatment failed to control the symptoms, and I operated again on the sixth day. Immediately all the symptoms of internal mischief vanished; the pain subsided, the mucus diminished, his bladder became more tolerant of its contents, and he again passed detritus in a large quantity. It was now quite obvious that we had passed the ordeal of difficulty, and that the fragments of stone remaining in his bladder were very inconsiderable in quantity.

I operated again on the fourth [?] day, and completed the crushing, reducing every remaining fragment to a size capable of transmission along the canal of the urethra. Within forty-eight hours the bladder had entirely evacuated the whole of the calculous matter, and on carefully sounding with a variety of instruments, I was unable to detect the presence of the smallest fragment. Some months have now elapsed, and the vigilant observation of my patient fails to detect one symptom of his former disease. The stone, judging from the quantity of detritus obtained, was of moderate, not of small size. Had it been removed by means of the knife, it is highly probable that the time required for the patient's recovery would have exceeded that occupied by many days. During the progress of the treatment he cannot be said to have suffered severe pain; he was never brought within the circle of danger, and he left London with health unimpaired by surgical discipline or deprivation.

Case 2,—was that of a gentleman, sixty-two years of age; of a less healthy aspect than the subject of the last case. He had had symptoms of stone about nine months, and his health had suffered in consequence. His expression was that of a man worn by internal irritation. I ascertained the stone to be of moderate size, and its contact with the metal caused a ringing sound which was audible at a distance. On testing his urine, I found it albuminous, and postponed the operation. He was ordered diluted nitric acid in infusion of diosma, and his urine improved. In a week I introduced the lithotrite, and simply broke the stone once across. On withdrawing the instrument, and having completed the operation, Mr.—— exclaimed, "What! is that all? I have had no pain whatever." He dressed himself, and returned to the society of his family. On the following day he passed one or two small fragments of stone, composed of phosphate of lime; but he continued to sustain no inconvenience from the operation.

On the fourth day I performed the second operation. On introducing the lithotrite, the bladder appeared contracted. At all events I had some difficulty in expanding the blades of the instrument, although I had thrown into the organ the quantity of water I usually inject—viz., about four ounces. However, I caught the stone, and while screwing home the blades, I perceived blood flowing somewhat freely from the orifice of the urethra. This determined me to desist, and I withdrew the instrument. For some time bloody urine continued to flow from the canal; but on the second day I was summoned by his medical attendant, in consequence of an attack of retention of urine, and I drew off about a pint and a half of urine, deeply coloured with blood. Retention again followed, and I removed nearly the same quantity, and of the same character of urine, on the fourth day. The presence of the distended bladder did not appear to cause him much inconvenience, nor its evacuation much relief. Urine, more or less bloody, continued to escape from the urethra without the effort of micturition. He had no local pain, and bore pressure, both over the bladder and on the perineum, without complaint. He was ordered gallic acid, in full doses. Sir Benjamin Brodie saw him at this stage, and did not augur unfavourably of my patient's case. He recommended the employment of Ruspini's styptic, and suggested the repetition of the operation as early as permissible. His increasing weakness was aggravated by the excessive action of a moderate dose of castor oil, and although the hemorrhage was reduced in quantity, his vital powers were now only sustained by the frequent employment of stimulants. Bladders of ice were applied to the epigastrium and to the perineum without effect. He became comatose, and died on the fifteenth day from the second operation.

The post-mortem examination exhibited a contracted bladder, thickened with its inner surface coated with coagulum; a calculus of about the size and form of a moderate-sized walnut, broken into three parts—its composition, oxalate of lime, coated with phosphatic salts, and two lesser calculi entire; ureters dilated; kidneys diseased.

In my work on "Operative Surgery," I have referred to two or three similar cases to that of Mr. ——, but they are rare. The fatal issue in this instance is not to be cited as injurious to the good name of lithotrity. Had the old operation by the knife been substituted, the issue had, in all probability, been the same. The presence of albumen in the urine, when coupled with stone in the bladder, is not conclusive evidence of diseased kidney, and with symptoms of an urgent character we are compelled to make the effort to obtain relief. The operation of lithotrity, when carefully performed, creates little more pain than that of sounding for stone, and generally, in a healthy bladder, leaves as little irritation behind. The proportion of persons who suffer from complications of renal disease, coupled with calculi, is small. I do not think such cases can be safely treated by the lithotrite. Neither is the cutting operation a security against a fatal termination.

Believing as I do that the operation by means of the lithotrite is applicable to a large majority of calculus affections of the bladder—that, if well executed, it is safer as regards the life of the patient, quite as certain as regards entire recovery, and less exhausting to the system, I recommend its practice for your consideration. And as I may not have the opportunity of addressing you again for some time, I propose concluding these remarks with some general rules which may prove useful in your future career.

I advise your rejection of cases for lithotrity presenting the following characters:—

1. Manifest disease of the kidney.
2. The urethra so contracted as not to admit with facility a lithotrite of ample size.
3. The bladder so intolerant as to be incapable of retaining its urinous contents for three or four hours; and on the other hand, a bladder of low nervous susceptibility.
4. Much enlargement of the prostate gland.

The quantity of water injected should not exceed four or five ounces. In many subjects, the employment of chloroform excites the bladder to contract, and the injection has to be repeated. The lithotrite, from its full size and angular form, should be introduced with more caution than is usually required on the introduction of a catheter. No attempt should be made to open the instrument in the bladder until it has been pushed thoroughly home into the organ. In the act of separating the blades, do not withdraw the upper, without at the same moment pressing the lower blade downwards towards the bottom of the bladder. If this rule be not strictly observed, the upper blade will be painfully pressed against the neck of the bladder, from which hemorrhage may follow. The stone is to be brought into the lithotrite by pressing the lower blade suddenly, and by a slight jerk or twist of the hand, against the base or bottom of the bladder. There is neither necessity nor advantage in directing the instrument to the right or to the left. It should retain the mesial line throughout the entire operation. When the stone is caught, the blades should be screwed "home," lest small accumulations become large, and render the withdrawal of the instrument through the urethra difficult. At the first operation, do as little as possible. It may be deemed an experimental occasion, and it will be sufficient to break the stone once across. On all future occasions, the number of applications of the screw may be determined by the tolerance of the patient. The stone may be broken six, eight, or more times. The intervals between each operation will vary according to the condition of the bladder, and the quantity of detritus expelled. If the quantity be considerable, and the bladder quiet, the operation may be performed in four or five days. The average interval is longer than this. Few cases pass through their course of treatment without giving evidence of irritability of the mucous membrane of the bladder, manifested by a discharge of tenacious mucus adhering to the bottom of the vessel. Unless in its aggravated form, it is not a serious symptom. It may be treated with diosma; or uva ursi, nitric acid, Dover's powder, &c.; but the best remedy is the lithotrite. I have repeatedly seen this symptom subside on the repetition of the operation. The constitutional treatment is chiefly dietetic. Diluents should be ordered largely; and the moderate use of wine is unobjectionable. I have never seen any advantage obtained by an abstinent diet, nor any evil arise from an ordinary and habitual one. It is surprising how large a fragment may travel along a healthy urethra. They are arrested, however, most frequently at the glans, and if a fragment cannot be extracted by a pair of fine forceps in this situation, the urethra should be divided. When fixed low down in the urethra, they should be pushed back into the bladder. This may be effected by a large catheter (No. 12). cut off straight at the point, the extremity of the instrument being supplied by a moveable knob, which is withdrawn when the catheter touches the stone. The open extremity of the instrument encircles the stone, which is forced back without injury to the mucous membrane. A small abscess in the perinæum may occasionally follow the violent employment of the lithotrite, or forceps, &c. It presents itself under the form of a small rounded tumour. It seldom requires active treatment, and, as a general rule, may be "let alone."—*Lancet, July* 30, 1859. *p.* 105.

68.—*Lithotrity and Removal of the Fragments at the time.* (Case under the care of Mr. Fergusson.)—[In a case lately under treatment at King's College Hospital, Mr. Fergusson employed what he termed a "favourite method" of his in expediting the cure. It consists in the removal by means of the lithotrite of as many fragments of the stone as can be got away, at the third crushing.]

The man having been put under the full influence of chloroform, the stone was broken by a large and strong instrument. A much lighter lithotrite was then introduced, and a fragment having been seized, the size of which was evidently moderate, it was dragged out. This manœuvre was repeated ten or fifteen times, and enough was removed to have made up a stone of moderate bulk. It did not, however, appear that all had been got away, as the fragments could not be made to fit so as to make up one stone, and the original opinion as to there being more than one seemed strengthened. In several instances considerable difficulty was encountered in getting the fragment seized out at the meatus, although it had slipped quite easily through the deeper tracts of the urethra. Occasionally it would escape from the grasp of the instrument, and remain impacted about half an inch from the orifice. When this happened, Mr. Fergusson employed a small scoop or a pair of common dressing forceps to effect the final removal. Of course some bleeding attended the forcible dragging of the fragments out; it appeared, however, to come almost wholly from near the meatus. On the plate, after the operation was completed, we counted six fragments, all of them of too large a size to have been voided spontaneously, and there were at least twenty others of smaller dimensions. There can, therefore, be no doubt that this plan of forcible extraction, per urethram, if safe, very greatly expedites the cure. Mr. Fergusson's dexterity in the use of the lithotrite may, it must be borne in mind, make that safe in his hands which would be otherwise in those of less experienced surgeons. Thus, although the instrument had, on the occasion alluded to, been introduced and withdrawn at least fifteen times during the sitting, yet the whole had not lasted more than about twenty minutes. The instrument seemed as if spontaneously, at each introduction to seize a fragment of right size for extraction the moment it entered the bladder.

Mr. Fergusson, in some clinical remarks at the conclusion of the operation, admitted that the plan he had adopted was one as yet on its trial. His own belief was, however, strong that when judiciously carried out it very materially hastened the patient's recovery, without adding to his risks. With regard to the degree of force which might be used, he said that his rule was, never to employ violence if the stone stuck in the lower part of the urethra. By the lithotrite the size of the fragment might be estimated with a certain degree of accuracy, and if too large it should be crushed instead of being extracted. If, however, no difficulty occurred until reaching the anterior inch, he believed that forcible extraction through that part did no injury whatever. Some might incline to adopt the plan of the older lithotritists, and slit up the meatus before beginning; but he thought it unnecessary. He directed attention to the smallness of the intrument which he had used; and stated that though much too weak for crushing large stones, it did very well for the extraction of fragments.—*Med. Times and Gazette, June* 25, 1859, *p.* 651.

69.—*On a New Mode of Relieving Retention of Urine.* By LANGSTON PARKER, Esq., Surgeon to the Queen's Hospital, Birmingham.—[The writer has lately succeeded in two separate instances in relieving retention of urine in the following manner:]

A gentleman lately entered my consultation-room in great pain from retention of urine. He had not passed water for many hours; the bladder was much distended. He stated that ineffectual efforts had been made to pass a catheter, during which operations he had lost a considerable quantity of blood. I attempted to relieve him by the catheter, but failed to do so; I tried instruments of various sizes and various curves, but could not succeed in passing one into the bladder. I then took a No. 2 wax bougie, and inserted a small portion of potassa fusa into the end of it, after the manner proposed by Mr. Whateley, and practised by Mr. Wade in the treatment of permanent stricture of the urethra. I well moulded the wax over all but the extreme point of the caustic, and passed it rapidly down to the point of obstruction; by pressing against this for a short time, it yielded, and I had the satisfaction of finding the bougie easily enter the bladder. I directed the patien to strain as I withdrew the instrument: a stream of urine followed, and the bladder was emptied. The retention did not again occur, and very little irritation accompanied or followed the proceeding. On the next day the patient made water freely, but in a small stream.

The second case was very similar. The patient had travelled some distance by rail. The bladder was much distended, the symptoms urgent, and a catheter could not be made to enter the bladder. A small wax bougie was armed as in the last case, passed down to the stricture, and firmly pressed against it. It yielded very shortly; the instrument entered the bladder, and a stream of urine followed its withdrawal. This patient had a second attack of retention two days afterwards, which was completely relieved in the same manner.

A modification of this plan might be attempted by inserting a small piece of potassa fusa into the extreme point of a small gum-elastic catheter, and using it without the stilette. I am sanguine enough to hope that many cases of retention of urine might be easily and quickly relieved by the simple means I have suggested, and more formidable and dangerous operations thus frequently avoided.—*British Med. Journal, May* 21, 1859, *p.* 400.

70.—THE VALUE OF INTERNAL INCISION IN THE TREATMENT OF OBSTINATE STRICTURES OF THE URETHRA.

By HENRY THOMPSON, Esq., Assistant-Surgeon to University College Hospital, &c. [Read before the Medical Society of London.]

[Unquestionably, in all cases of stricture, the remedy first to be tried is dilatation, being the safest and simplest. But a large number of cases remain, not amenable to this mode of treatment alone; in many of these cases internal division may be practised with great advantage, being a means much less severe than external urethrotomy. More than thirty years ago Mr. Strafford introduced this practice, his instruments being probably now well known. Subsequently it has been followed by Liston, Guthrie, and Coulson. The exceptional cases in which dilatation proves to be only a slight palliative, appear to be found in two distinct classes of patients, which may be thus generally indicated.]

1. The first class comprehends cases in which the stricture is so unyielding, that no dilatation, simple or continuous, materially enlarges the passage or ameliorates the symptoms. These are almost invariably strictures which have existed some twenty years or more, and in patients who have been the subjects of repeated and long-continued courses of treatment.

2. The second class includes cases which, as compared with the preceding, occur during early life, but in which, nevertheless, the stricture exhibits considerable obstinacy to dilatation, although pursued under favourable circumstances. The symptoms are, however, ameliorated by it, although only for a short period of time, while usually on each recurrence they appear to be a little more severe and obstinate than before.

In the first class, internal division is recommended solely to meet existing evils, of a very grave character.

In the second class, it is employed not only to remedy present difficulties, but also to anticipate future ones; since it is almost absolutely certain that serious if not fatal injury will result long before the natural term of life is spent, when in the early part of it a severe stricture is established, and merely palliative treatment is applied. And it is a question which demands our serious consideration, whether it is not wiser to give permanent relief to the patient, if possible, before extensive urethral disease has been established, or organic complaints in the bladder or kidneys have been set up, than to postpone the attempt until such changes have already taken place, and the power of palliative measures has been well nigh exhausted.

After a careful study and a considerable practice of urethrotomy, both internal and external, I believe that the former affords a most useful and efficient mode of treatment for a large proportion of the exceptional cases indicated in the foregoing classes; that is, for those cases in which the indurated tissue constituting the stricture can be divided with ease and certainty by an incision of very moderate depth. It is inapplicaable to those which cannot be so divided; for where the indurated tissue is of such extent as to render necessary for its complete division a *deep* intra-urethral incision, I believe the hazard of internal is greater than that of external urethrotomy. Consequently I would employ no instrument, for cutting within the urethra, *capable under any circumstances*, of making a deep incision.

There is a fact in connection with this subject well worthy of notice—namely, that the unyielding or non-dilatable character which certain strictures possess appears to be developed for the most part in proportion to their proximity to the external orifice of the urethra. Strictures at or near the orifice itself are notoriously non-dilatable; so are those, although in a somewhat less degree, which occur at the distance of from two to four inches—

that is, just in front of the scrotum. Much less so are those which affect the most common situation—viz., the bulbous portion; that is, a part of the urethra, about an inch in length, which lies anterior to the deep perineal fascia. This is a fact which has a very important bearing on the question of treatment, since there is no doubt that intra-urethral incisions of the kind here recommended are almost absolutely devoid of risk when made anterior to the scrotum, so that the indications for their use and the freedom from danger happily coëxist in relative proportions. Thus, it is not very common to meet with stricture of the bulbous portion which requires internal division. Although perhaps scarcely necessary, it may be desirable to premise that no internal incision should be applied to a stricture through which an instrument of some size cannot be passed into the bladder. I do not hesitate to endorse the proposition, that where the urine issues by the external meatus, an instrument may, with time and patience, care, and gentle manipulation, be passed through the stricture into the bladder; the crisis of retention sometimes excepted, when time is not always present, nor are circumstances favourable. I do not affirm the impossibility of exception; but I am entitled to say, after a considerable experience, and with the full consciousness of the responsibility incurred by so doing, that such exceptions should be very, very rare indeed.

For the successful practice of internal incision, as a rule, three conditions must be complied with. There may be some few exceptions to these rules, but they are to be regarded as almost of universal application.

First. The cutting instrument must be passed through the stricture, and the incision be made from behind forwards—that is, toward the orifice of the urethra; not from before backwards.

Secondly. The limits of the stricture being first accurately defined, the whole of the contracted part should be divided.

Thirdly. The borders of the incision should be maintained apart by catheterism subsequently performed, and healing by first intention thus be prevented.

These three points shall be separately and briefly considered.

The first condition.—The safest and most efficient internal incisions are those which are made from behind forwards—that is, when the operator divides the stricture by drawing outwards or toward himself an instrument which has been already passed through the whole of the contracted portion. The incision is then cleanly made, not jagged; but the reverse is apt to take place when the bladè is pushed inwards—that is, from the operator. In this case, the flexible yielding walls of the urethra are liable to be pushed into folds in advance of the blade before they are cut, and the result is an uneven incision, the length and other characters of which are not under the perfect control of the operator. But in making the incision from behind forwards, the urethra is rendered tense, folds do not exist, and the incision may be made to correspond precisely in length and direction with the intention of the operator. That this difference between the two methods exists may be easily believed from a consideration of the structure and relations of the urethra; but it may be demonstrated by comparative experiments on the dead body. Further, I may state, that a very large experience by continental surgeons, apart from theoretical considerations, has now left little doubt in their minds as to the comparative efficiency and safety of the two methods. While the occurrence of perineal or scrotal abscess, say they, as a result of the operation, is not extremely uncommon after incisions from before backwards, it is almost unknown in those of the kind above described made in the reverse direction. These facts, it may be added, I have verified by personal observation.

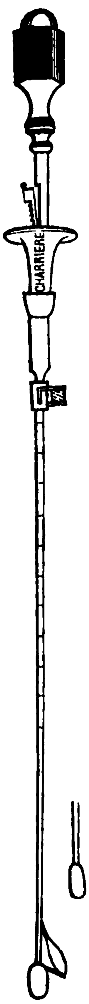
The second condition.—The limits of the stricture having been first accurately determined, the whole of the contracted part is to be divided. Incomplete division of the stricture almost always entails an unsatisfactory result. It is not depth of incision which is here referred to; for it has been already stated that unless the thickened tissue of the stricture can be divided by an incision of very moderate depth, (a term which shall be accurately defined hereafter when I come to consider the question of instruments,) internal urethrotomy should not be employed. The partial failure of an intraurethral incision is generally in its length. Unless the operator is well aware of the liability to error in this respect, he is apt to miss a portion of the stricture, especially of its distant limit. The cause of this may be thus explained. A stricture, as is well known, is not generally constituted by a sudden narrowing of the canal, limited to a distinct portion as of a line or two in extent, but implicates more or less of the urethral walls before and behind the point of extreme narrowing; in other words, a certain portion of the canal is the seat of some lymph deposit, and consequently of some tendency to contract, to the extent of generally about a quarter or third of an inch before and behind the point in question. When the cutting portion of the instrument, therefore, has been passed beyond the point of extreme narrowing, it is not sufficient merely at once to draw the instrument outwards, but the line of incision must commence about half an inch beyond that point, so as to ensure the division of all the portion affected by the disease. The incision will usually require, therefore, to be from an inch to an inch and a half in length. And freedom, in this particular of length, is in no way injurious. It cannot be too often repeated, as the result of the practice of modern urethrotomy—a fact enunciated by all those who have a large experience—that hazard attaches only to deep and not to long incisions. It is extremely important to remember that incomplete division is almost certain to be followed sooner or later by a re-formation of the stricture in that portion of the urethra which, although involved in the disease, was permitted to escape the blade of the urethrotome. Unless attention to this be ensured, a useful method of treatment may acquire undeserved disrepute. I have myself seen cases which, solely from want of attention to this particular, have not been benefited; and in this manner the operation has been unfairly discredited. It is not necessary to enlarge here on the means of acquiring an accurate idea of the situation and extent of a stricture, which I have detailed elsewhere, but only to remark that, with the aid of an instrument with a bulbous extremity, such information, which is absolutely necessary to ensure the successful performance of internal urethrotomy, is easily obtained.

The third condition.—The borders of the incision are to be maintained apart by catheterism frequently employed, so that healing by first intention be prevented. Theoretically considered, the propriety of this proceeding would appear to be unquestionable. Reasoning by analogy, also, from the unsatisfactory results which have been known to occur from neglect to pass full-sized instruments after the operation by external division, we should assert the absolute necessity of the practice here also. But it is certain that some foreign surgeons, who have had considerable experience, believe that the subsequent catheterism is unnecessary. I have no hesitation, however, in regarding it as unwise not to practice it, believing that experience has proved it absolutely necessary in some instances, and, being so, it must be desirable in all. For if, in some cases, a complete cure has resulted from the mere internal incision, unassisted by subsequent dilatation, we have no reason to infer therefrom, that in all cases the same consequence is to be anticipated; and as we possess no means of distinguishing the cases which may be cured without after-treatment, or, indeed, of knowing why such a result is met with at all, it appears to be hazardous not to practice the subsequent catheterism in all. The plan which I adopt and advise is, to pass a full-sized instrument daily during the first fortnight after the operation, and every other day during the subsequent week or fortnight. This ought to be a metal sound, having a uniform diameter throughout, of not less than 11, 12, or thirteen; and it is desirable in withdrawing it, to press its extremity downwards into or upon the line of incision, in order to maintain the borders apart, and subsequently to extend the recently-formed tissue there. The patient should then be taught to pass one for himself, which he may at first do twice a week, then once a week, and finally only now and then, as once a month or so, usually as a precautionary measure. The little wound which separated the divided borders of the fibrous material hitherto surrounding the urethra, and constituting the stricture, is thus prevented from closing by mere adhesion of those borders, but fills up by granulation, and the calibre of the urethra is restored. The new tissue thus formed is exceedingly extensible, and when subjected to daily and bi-weekly dilatation during three or four weeks after the operation in the manner above described, ensures a condition of the urethra which, although not equivalent, perhaps, to that which exists in a urethra which has never been diseased, is still one of immense improvement for a patient who had been previously the subject of severe and strongly-contractile stricture. The removal of these specific and dangerous characters is in fact the cure of the stricture, so far as that term can be applied as a result of any known mode of treatment. It constitutes a stricture completely dilatable which was not so before. The patient is relieved not merely from present ills, but from the apprehension that evils of a grave character will occur in after-life.

There is one important point in connection with the selection of the cases to which internal urethrotomy may be deemed applicable, which appears to me, judging from opinions frequently expressed respecting operative measures for the cure of stricture, to require a little elucidation. There evidently exists a common belief, that a stricture must be very narrow indeed—say one that admits at most a No. 1 or 2 catheter—in order to require treatment by any incision. Now, with great deference to those who may hold this opinion, I undertake to affirm that the narrowness of a stricture, *per se*, is not the single or even the most important element respecting it. True, it is usually the most obvious one; but I must venture to say, that want of reflection or experience, or of observation, which is the same thing, has alone led to the supposition that the degree of diminution in the calibre of the urethra, and the intensity of the symptoms, or the gravity of the disease, have a corresponding relation. In other words, although it sometimes happens that a very narrow stricture produces comparatively slight symptoms, and these easily amenable to treatment by dilatation, it is no less certain that a stricture which is not narrow, but which is at the same time non-dilatable, may produce distressing, and indeed, dangerous symptoms. The extreme narrowness of a stricture, therefore, by itself, is not necessarily an indication for treatment by incision; neither, also, does the converse condition necessarily forbid it.

The most important physical characters of an organic stricture, whether its calibre equals No. 1 or No. 6 of the catheter scale, are Non-dilatability and Contractility. By the term Non-dilatability I intend to denote a condition in which the tissue constituting the stricture is so unyielding, that instruments well and perseveringly applied, even when favourable hygenic conditions have been ensured, will not materially enlarge its calibre. By Contractility I understand a quality which some strictures possess, through the agency of which, whatever temporary effect may be produced upon them by dilatation, the original degree of narrowing reappears almost immediately.

Thus, a patient may present himself with a stricture through which an instrument of the smallest size only can be passed, yet in a short time the normal calibre may be re-established by simple dilatation, and may be maintained with little attention subsequently. On the other hand, we sometimes encounter an example of the disease in which a No. 4 or 5, or even larger, may be passed into the bladder; nevertheless, the symptoms are very severe, and dilatation, perseveringly employed, produces little effect on the narrowing, and affords very slight or no relief. It is amongst these latter cases especially that I have often witnessed the excellent effects of a division of the indurated and unyielding ring which surrounds the urethra at the point of stricture, and which no other treatment so effectually, simply, and safely combats. Hence it is, that a remark often heard, appears to me appropriate—viz., that if an instrument of the size of No. 5 or 6 can pass, there can be no occasion to make any division of the stricture. Such a remark, I repeat, is due to the erroneous assumption, that the calibre of a stricture is its mainly important character, the determination of which alone is sufficient to indicate its gravity; and also the kind of treatment which may be required. Few greater mistakes can be committed in connection with this subject than to endorse this statement as a pathological truth. There are many other characters which possess equal, if not greater weight, in determining the nature and probable influence of a stricture upon the constitution, such as the degree of local sensibility and disposition to inflammation; its liability to occasion severe disturbance of the nervous system, as evidenced by rigors and fever on slight provocation, besides those physical characters of contractility and non-dilatability already alluded to. It is the presence or absence of such conditions, and not the mere fact of narrow calibre, which requires to be considered in coming to a conclusion respecting the appropriateness of any particular treatment.



Civiale's throtome

We have already seen that the physical characters just named are found to affect more commonly those strictures which are situated about four inches from the external meatus than those which exist behind that limit, or within the bulbous portion of the urethra. Of this fact I believe a satisfactory anatomical explanation may be afforded. A stricture situated within the bulb, unless consisting of a very dense and considerable fibrous deposit, is dilatable, in part, probably, because it is surrounded there by a large mass of very extensible tissue—the erectile—which can oppose little or no mechanical resistance to the passage of a large instrument through the contraction. It is widely different, however, in the antescrotal part of the urethra; there the extensible structure exists in much smaller proportion, and the possibility of dilatation is limited by the two fibrous layers of the corpus spongiosum which, having comparatively little erectile tissue between them in the situation, oppose a mechanical obstacle to dilatation, while their close proximity appears also to favour the permanence of lymph deposit, which, during inflammatory action, may rapidly infiltrate the thin stratum of vascular structure intervening.

[In the second and remaining part of this paper the writer passes on to consider the instruments best adapted for the performance of internal division of stricture situated in the anti-scrotal and bulbous portions of the urethra.]

In the first place, it is necessary to possess a clear idea of the situation and extent of the stricture, or strictures (if there be more than one), which it is proposed to divide. The urethra should be examined by passing a full-sized instrument, No. 9 or 10, as far as to the seat of stricture, wherever it may be, and the distance from the external meatus noted. Next, an instrument with a slender stem and a bulbous extremity should be passed through the stricture, that size being employed which requires a slight degree of pressure to enable the bulb to slip through the contracted portion. By means of this instrument it is easy to estimate the extent of the stricture, and also to ascertain whether another exists behind the first. To effect this purpose most of the urethrotomes themselves which cut from behind forwards are constructed. The enlarged extremity which conceals the blade serves as an exploring bulb, by means of which the length of the obstruction may be estimated, and also, when the difference exists, that side of the urethra on which it is most salient.

The instrument which I have most commonly employed, and still prefer, is that which was designed some fifteen years ago by Civiale, of Paris, and has been used by him with little modification to this day. For a few exceptional cases, in which the instrument is too large, I have used one designed by myself, and which is capable of being made in a smaller size than any previous urethrotome which cuts from behind forwards. The shaft of Civiale's instrument is equal in size to about No. 3, the bulb to No. 5 (see Fig. 1). Consequently, the stricture must have a calibre nearly equal to the last number; and if it does not admit it, my small instrument, of which the bulb equals in size only No. 2½ or 3, may be used, or, as is the custom of Civiale, dilatation, either occasional or by tying in a catheter for a given period of time, may be previously employed, in order to bring up the calibre of the stricture to the required size. The bulb of the instrument having been introduced about half an inch beyond the narrowest point of the obstruction, the cutting side being directed downwards and directly in the middle line, the blade is made to project to the required extent by means of an apparatus in the handle, which accurately controls the blade, and the instrument is firmly pressed on the floor of the urethra and slowly and steadily drawn outwards about an inch or an inch and a half, so as fairly to divide the obstructing portion—a result easily appreciated by the hand, in the overcoming of the resistance which the thickened and hardened tissue of the stricture presents to the sharpest blade; this latter is then immediately sheathed by a movement of the thumb, and the instrument is withdrawn. If two strictures exist in the same urethra, the second may be divided in a similar manner, if necessary. A full-sized metal or gum-elastic catheter is then passed into the bladder and tied in. It seems hardly necessary to remark, although I have seen instances in which the precaution has been overlooked, that ordinary care must be taken to guide the catheter along the roof or upper wall of the urethra, and curve it well up under the pubic symphysis, to avoid engaging its point in the incision in the floor. The catheter should remain in the urethra at least twenty-four hours, the patient remaining in bed during this period. On the succeeding, and on each subsequent day during two or three weeks, a full-sized metallic sound should be passed into the bladder, and be at once removed. The operation is attended with very little pain, never requires the use of chloroform, and the confinement within doors amounts only to a period of from three to four days.

Figs. 2 & 3.

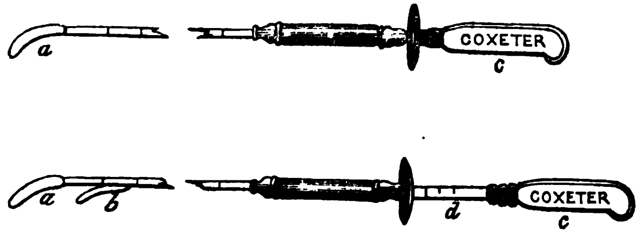


Fig. 2. The urethrotome of the smallest size.

Fig. 3. The same, with blade drawn out. *a*, bulbous extremity; *b*, blade.

The form of this small urethrotome is very similar to that of an instrument employed by M. Mercier, of Paris, for incising the prostate (figured in my work on the Prostate, p. 254), only that in the latter a bulbous end is not employed, and the size is necessarily large, while the former is of the smallest that can be made.

My own urethrotome, which has not hitherto been described, is so constructed that the smallest size which can be needed may be employed; much smaller than it is possible to adopt with the mechanism of Civiale's instrument, no special apparatus being necessary, as in his, to make the blade project. And it is solely for the sake of having a size thus small for exceptional cases that I have designed it. It is employed in a manner similar to that above described, excepting only that in making the incision, instead of drawing outward the entire instrument, as in using Civiale's urethrotome, it is only necessary to draw outwards the handle of the blade to an equal extent, the canula remaining *in situ*, and being at the same time firmly pressed downwards on the floor of the urethra. The projecting of the blade is provided for in the simplest manner, by merely drawing it outwards from the bulbous extremity, which is slightly curved for the purpose of concealing it. (see Figs. 2 and 3.) There is no provision, however, for regulating the degree of projection in the blade, as in Civiale's; but this power is rarely required, because it is undesirable to use anything like the maximum degree of projection of that instrument. It is the medium degree which is always required, so that the advantage of adjustment apparatus is rather apparent than real. I have been careful in my urethrotome to adopt a degree of projection which is not capable of doing harm, but the whole of which is necessary if an incision is required at all. It is intended, moreover, only for those very narrow strictures through which it is not possible to introduce one of Civiale's instruments; being made therefore only in a small size, degrees of adjustment are still less necessary. In order to estimate exactly the degree of projection to be employed, it may be stated that when the blade is exposed, the distance between its point and the back of the instrument, when applied to the ordinary catheter scale, equals only the diameter of No. 14. The full medium degree of projection which I use in Civiale's instrument, and which I think ought not, or rarely, to be exceeded, is equal to No. 18 of that scale. Although the construction of the instrument admits of its being used to cut more deeply, I have never seen reason to exceed the projection above indicated.

For the division of strictures at or near the external meatus, the following method of accomplishing it is, I think, the best. Such constructions are generally remarkably obstinate and unimproveable by dilatation, and are quite curable by incision, which however should be free. The most perfect instrument for the purpose is a small *bistouri caché*, which may be employed with or without previous dilatation, according to the calibre of the contraction. Having previously arranged, by means of a screw in the handle, the extent to which the blade is intended to project, it is introduced fully through the contraction, the edge of the blade being directed downwards, pressure is made on the handle, and the instrument is quickly drawn outwards. A plug of oiled lint, of the size of a 12 or 14 bougie, is then introduced, and left there until the patient requires to pass urine, when it is removed and exchanged for another. After a day or two the patient may introduce for himself a short mettle bougie of full size, about three inches long, and provided with a handle sufficiently broad to obviate the possibility of its slipping altogether into the urethra.

I shall now very briefly remark upon some accidents which have been known to happen after internal urethrotomy, as practised in some of its varied forms. I refer to hemorrhage, urinary extravasation, perineal abscess, and constitutional affections, such as fever and pyæmia. Certain of these, it is well known, may be occasionally encountered after the use of instruments of whatever kind in the urethra, and for whatever purpose they may be employed.

First, as regards my own experience. I have the notes of forty-two cases, in which internal incisions of the kind here recommended have been performed in different parts of the urethra. From incisions practised upon stricture situated within one inch of the urethral orifice, I have never witnessed any effects whatever of an unpleasant kind, unless one may regard as such some œdema of the prepuce, lasting for two or three days, an occurrence I have observed on one occasion only, and which was unquestionably due to voluntary imprudence on the part of the patient.

From incisions practised for stricture situated about three or four inches from the external meatus—i. e., just in front of the scrotum, I have never seen any ill effects whatever. Neither hemorrhage, abscess, nor any constitutional affection, has resulted in any case which has come under my observation. A little discolouration of the skin from ecchymosis sometimes occurs when the stricture is just anterior to the scrotum.

From incisions practised in the bulbous portion of the urethra—that is, in the portion comprising about an inch or an inch and a half anterior to the deep perineal fascia, I have twice seen somewhat free, but never dangerous, hemorrhage. Generally speaking, it has been slight—that is to say, as far as can be estimated, from one to three drachms at the time of the operation, and a little oozing for a few hours afterwards, often scarcely perceptible. In exceptional cases, it continues to colour the urine for a day or two; rarely, the loss may amount to a few ounces. I have never seen it serious in amount. On one occasion, I thought proper to control it by placing in the urethra the largest-sized catheter it would take, and a large pad on the perinæum, made to press firmly there by means of a tight T-bandage. This was completely successful.

Posterior to the deep perineal fascia, I believe organic stricture never exists, unless from traumatic cause, which may occasion it in any part, but such an occurrence must be extremely rare. Practically speaking, I am certain that it is so. Hence it is unnecessary to discuss here the propriety of making incisions behind the deep fascia.

Perineal abscess, extravasation of urine, dangerous febrile attack, pyæmia, and inflammation of the kidneys, are occurrences it has never yet been my lot to witness as a sequence of internal division of the kind recommended here. I do not doubt that they may occasionally occur, because any instrumental interference with the urethra, even the passing of a bougie, is apt in certain cases, happily very few in number, to give rise to these conditions. Indeed, no one who is experienced in the surgery of urinary organs can question that such occurrences must sometimes arise. Experience proves, however, beyond a doubt, that internal urethral incisions which are not deep, and which have been made from before backwards, are very unfrequently followed by the results in question. In relation to the practice of others which I have at different times observed, chiefly in Paris, where internal urethrotomy has long been largely employed, I can testify that unpleasant or dangerous results are very rarely met with after the method of operating which I have described. From the heroic treatment by deep incisions, which also has been practised there from time to time, disastrous results have happened, as might naturally have been anticipated; and such treatment I cannot too strongly deprecate. Such occurrences have had the effect of raising an unfortunate prejudice in this country against urethrotomy in any form.

[After the reading of Mr. Thompson's paper, Mr. Coulson made the following remarks:—]

Having adopted internal urethrotomy both in public and private practice, he was enabled to speak of its merits, and the plan of performing the operation. There was one indispensable condition, namely, that the operation should be preceded and followed by dilatation: without first dilating the stricture, the urethrotome could not be carried behind it; and after the operation was performed, unless dilatation were employed, the stricture would certainly return. The operation was, in fact, an aid to dilatation, which it rendered more easy, prompt, and effectual. The objection naturally arose, that if the urethra could be so dilated as to admit the bulbous part of the instrument behind the stricture, why should not dilatation be continued until the cure was effected by this means? But it was well known that cases often presented themselves in which dilatation could be carried only to a certain extent, and that far short of restoring the natural calibre of the canal; cases in which the contractility of the stricture was so great that retention would frequently ensue as the consequence of attempts to carry dilatation to the necessary point. The part of the urethra in which he (Mr. Coulson) had most frequently performed the operation was in the anterior four inches of the canal; sometimes close to the urethral orifice, and at other times further back; involving sometimes only a ring-like stricture, and at others a length of tissue two or three inches in extent. He would allude to one point which the author had omitted—namely, that the removal of the stricture, if elongated, might require the operation to be repeated on two or three occasions, as in such cases the whole of the stricture could not always be divided at one time, with prudence. He believed that the operation, when carefully and properly performed, was attended with very little pain, much less indeed than that attending the forcible use of the catheter or bougie; and he had not known any injurious results to arise from it. If any danger attended the operation it would be when it was performed at the bulb, and to strictures at that site he had not often applied it. The instrument used should be of simple construction, easily handled, and, above all, one which would readily divide the resisting tissues without dragging or lacerating the indurated parts or pushing before it the yielding walls of the urethra. He believed that the urethrotome of Civiale, which the author had exhibited, was best suited to the purpose. For contractions of the orifice, or stricture near the aperture, he considered that a closed bistoury, such as he then exhibited to the Society, was the preferable instrument. The incision should be generally carried along the inferior surface of the canal, but in some cases it was necessary to divide laterally in order satisfactorily to relieve the stricture; and after the operation a full-sized catheter should be introduced, and left *in situ* from twelve to twenty-four hours. Mr. Coulson repeated that this proceeding was only applicable, in his opinion, to those exceptional cases in which patient dilatation failed to effect a cure.—*Lancet, Oct.* 15 *and* 22, 1859, *pp.* 385, 396, 406.

71.—ON THE TREATMENT OF URETHRAL STRICTURE.

By **Robert Wade**, Esq., Senior Surgeon to the Westminster General Dispensary.

The means adopted by surgeons for the relief or cure of urethral stricture are usually classed under three heads:—1st. Dilatation; 2nd. The application of some escharotic substance to the diseased tissues of the urethra which form the obstruction; 3rd. Division of the stricture, either from within the urethral canal by some cutting instrument, as the lanceted catheter, and a host of other contrivances:—or, from without, by external incision, now commonly called perineal section.

These three methods might, it appears to me, be very properly classed together under the first head, as the two latter will seldom prove effectual without the aid of the bougie or other dilating instruments.

The term dilatation is, however, commonly restricted to the method in which dilating instruments only are used, that process being effected without the assistance either of escharotics or incision.

This unaided or simple dilatation is the method which, for a long time, has been principally relied upon by British surgeons for the relief or cure of urethral stricture.

There is, I believe, no better method of proceeding in a very large proportion of cases; and such has been its successful results, that many very able surgeons have discountenanced every other mode of treatment.

How, then, it may naturally be asked, does it happen that a plan of treatment so simple, which has proved so efficacious in the hands of some, should so signally have failed with others of equal surgical celebrity? indeed, so unsatisfactory with the latter has often been its effects as to cause them to resort to treatment more severe, and sometimes even hazardous.

This discrepance evidently admits but of one explanation, and that is the particular manner in which dilatation is conducted.

My own experience has convinced me that the great error with regard to dilatation, has been an attempt to do too much at a time, by which the disease has been aggravated instead of relieved. The urethra is, in fact, but too frequently treated as if it were an inert lifeless tube, and not a living structure, possessing more or less exquisite sensitiveness.

Who can wonder at the failure of dilatation, when that process is carried on by forcible attempts to stretch a diseased and sensitive part of the urethral canal? It is true, that the immediate effect of these forcible attempts may, by opening more or less the contracted channel, permit the urine for a time to pass in a better stream, an improvement, however, but of short duration; as the irritation and inflammation which are almost certain to follow will, in nine cases out of ten, render the condition of the patient rather worse than better.

It is sufficient to observe, that this forcible unscientific manner in which dilatation is occasionally practised is the abuse, and not the proper use of this most admirable of all methods of treating the generality of urethral strictures.

It may be taken for granted, that this treatment will be most successful in the hands of those who use all possible gentleness and caution in the introduction of instruments. Lightness of hand and delicacy of touch, as well as great patience and forbearance, are indispensible qualities in the surgeon to enable him to do justice to this simplest of all methods of procedure.

I think it will be admitted by those who are most experienced in urethral surgery, that the greatest difficulties they meet with in effecting dilatation principally originates in the previous abuse of instruments.

Many surgeons have their favourite instruments for dilating strictures, some giving a preference to the silver catheter; others, to the solid metallic sound; while the elastic gum, wax, and plaster bougies have also their advocates.

Upon this point I shall only observe that no judicious surgeon will restrict himself to the use of any one of these instruments, as they will most assuredly all be found to be more or less useful some succeeding best in one case, and some in another.

If, however, I was restricted to the use of but one kind of instrument, my selection would be the simplest of all, the wax and plaster bougie, varying from a consistence soft enough to receive the impression of a stricture, to such a degree of hardness as will not readily yield to pressure.

It must be evident that no rule as to the best method of commencing dilatation can be applicable to all cases. I believe, however, that the one most generally applicable when the contraction is considerable, is to commence with the common bougie, and to continue its use until some little progress has been made, when the irritability of the diseased tissue, as well as of the entire urethra, will have become so much subdued as to permit, without irritation, the employment of some more solid, and in many instances, more efficient dilators.

In highly irritable strictures, with few exceptions, the soft wax bougie is infinitely superior to any other dilator; and its use should not be discontinued until a considerable diminution of their irritability has been effected.

Dilatation is usually described under the two general heads of temporary and permanent. When effected gradually by the introduction, from time to time, of bougies, catheters, or sounds, it receives the former appellation. In the latter, dilatation is accompanied by the continued retention of instruments in the urethral canal for certain periods of time, of more or less length, varying according to circumstances, extending sometimes to several weeks' duration. The elastic gum catheter is mostly used in permanent dilatation, being much less likely to cause urethral irritation than metallic instruments.

The selection of either of these methods, and the best manner of carrying them into effect in particular cases, must, of course, depend upon the judgment of the surgeon.

There is another kind of dilatation to which the term "special" has been applied. Particular instruments of various kinds have been invented for the purpose of effecting dilatation more promptly than those in ordinary use.

We have had dilators of water, of mercury, and of air, which have been much lauded by their inventors.

I have as yet had no reason to believe these dilators to be preferable, or even comparable to those in ordinary use, such as bougies, catheters, and sounds, which are much more manageable than the former.

I must not omit to mention two modes of accomplishing prompt dilatation which have lately attracted considerable attention—those of Mr. T. Wakley and of Mr. Holt. The dilator of Mr. Holt is a modification of Perrève's instrument. Mr. T. Wakley's instruments are too well known to need description.

From the strong testimony of their good effects which both these gentlemen have brought forward, it cannot be doubted that in some cases these dilators may be used with advantage to the patient.

I shall merely remark, with regard to their employment, that from the great power and complete command over the urethra obtained by these dilators, they should always be used with the caution so strongly recommended by their inventors.

Urethrotomy, or internal division of strictures by the lanceted catheter, and other cutting instruments, has been but little practised in this country. In this method, division is effected either from before backwards, or from behind forwards.

In impermeable cases, the section is made from the front by introducing as far as the stricture a tube containing a sharp blade, which is then thrust forward so as completely to divide the obstruction.

To effect division from behind the stricture, various ingenious instruments, consisting of a canula, with one or two cutting blades, have been invented by the French surgeons.

Their employment of course assumes the permeability of the stricture, as in using them they must be passed through the obstruction, when, by causing the blade or blades to project, division is accomplished as the instrument is withdrawn.

Internal section was principally practised in this country by the late Mr. Stafford, whose instruments, invented for this purpose, are well known. Next to Mr. Stafford, this method was more frequently adopted by Mr. Guthrie than any other British surgeon. It has occasionally, however, been had recourse to by others; but the practice of internal incision of urethral strictures has never been generally adopted in England.

Division of the stricture from behind forwards is undoubtedly the best method of effecting internal incision, having, however, the great disadvantage of being applicable only to such obstructions as are permeable by instruments.

The latter mode of internal section is held in high estimation by many French surgeons whose opinions, however, differ considerably as to the extent to which it is desirable to carry the incisions. By some, very superficial incisions, or mere scarifications, are practised, dilatation being afterwards completed, either entirely by the bougie, or with the aid of an occasional application of nitrate of silver. Others practise the free or complete division of the diseased tissues forming the obstruction. The best practical information regarding internal section of urethral strictures will be found in the excellent works of Leroy D'Etiolles and of Civiale, who have had very great experience of its effects.

According to Civiale, to be successful, the incisions must include the whole of the morbid structure. Reybard, whose work on Stricture, which bears the stamp of great talent and originality, to which was adjudged the Argenteuil prize of 1852, boldly advocates the absolute necessity of dividing not merely the stricture, but also the urethral parietes at the seat of disease, leaving only the skin undivided.

Mr. Reybard's views on this subject cannot be understood without some explanation of his peculiar pathological views regarding urethral stricture. To use the words of his reporter, M. Robert, "Reybard, with all pathologists, considers blenorrhagia as the most common cause of strictures of the urethra; but he goes beyond the received ideas when interpreting the part played by these phlegmasia in their production. According to him, all strictures due to this cause are constituted in all epochs of their evolution by a tissue, anormal, or of transformation, and not by engorgement, by thickening, by induration of the urethral parietes, as is generally understood." M. Reybard considers that "strictures present one invariable structure, and that their diverse forms constitute but one and the same affection."

Most surgeons of much experience in the treatment of urethral stricture will scarcely, I think, admit the general applicability of M. Reybard's pathological views on this subject. It appears to me evident that this distinguished surgeon must have derived his pathological prototype of stricture from one particular form of the disease, in which elasticity and retractility are so strongly marked as to render the ordinary mode of treatment by dilatation unsatisfactory.

I believe, with other pathologists, that the peculiar transformation of the urethra into a stricture of new formation, closely resembling in its properties the tissue of cicatrices, is by no means, as M. Reybard supposes, an essential character of urethral stricture. My own experience induces me to believe that although the transformation of strictures into a cicatricial-like tissue may sometimes occur in their early stage, it does not usually take place until a considerable length of time after their formation. I have good reason to conclude that so far from this fibroid transformation being an essential element of the disease, many urethral strictures of several years' duration consist merely of an inflammatory condensation of the part affected. At all events, in many cases in which symptoms of stricture have existed for several years, the bougie has afforded no evidence of the cicatrix-like transformation. I may add that the satisfactory results of the use of the bougie in these cases is to my mind evidence enough that the new tissue which M. Reybard considers as characteristic of stricture, could not then have been formed.

Every surgeon conversant with the treatment of strictured urethra well knows the difficulty in effecting satisfactory dilatation in the truly elastic India-rubber-like stricture. I should not have dwelt so much on M. Reybard's pathological views were it not for the hazardous practice which they have led him to adopt and recommend as the only satisfactory method of treating urethral obstruction.

There appears to me no sufficient reason to believe that there is any peculiar transformation of tissue in urethral stricture which is not the effect of ordinary inflammation.

My own experience of internal section has been but little, and that little not the most favourable, as I have found great difficulty in keeping sufficiently open the aperture made by the knife. Except in cases of urethral contraction at the glans penis, in which, from its peculiar density of structure, free division by the knife is the proper and most effectual remedy, I have resorted to internal section only as a means of immediate relief in urgent cases.

Internal division of strictures at the bulbous portion of the urethral canal has not infrequently been attended with unfortunate results, such as profuse hemorrhage, extravasation of urine, and purulent infection. Mr. Guthrie has informed us that after this operation, "urine may be extravasated, matter may form externally, or be discharged in quantity from within:" he adds, "these are accidents which may befall any one, although they do not often occur; that in whatever manner the urethra may be divided, whether for a stricture only, or for a fistula in perineo, any and every operation may lead to the excitement of a fever resembling ague in its paroxysms, and to the formation of matter in different parts of the body, in a similar manner to that which I believe I was the first to show did often happen after amputation. It is a misfortune that cannot be avoided; and he is fortunate in whom these depositions of matter occur in parts not essentially vital."

If the contraction should be anterior to the bulb, internal section, when properly performed, appears to be attended with but little risk to the patient. My own practical experience of this method has, however, been so limited that it would be presumptuous in me to give an opinion on the subject, after the strong testimony adduced in its favor by Civiale and others.

External division, or perineal section, from its advocacy by Professor Syme, has now become so celebrated, its merits and demerits have been so fully discussed, that it will be unnecessary for me to make many observations on the subject.

It is well known that this operation was formerly, with some few very rare exceptions, practised only in impermeable strictures, being always regarded as one of difficulty and danger. Mr. Syme has certainly much improved the proceeding by the previous introduction of a grooved staff or director, upon which the stricture is divided. Surgeons were confidently told that all those difficulties hitherto met with in the treatment of urethral stricture were at an end; that a new and royal path had been discovered, which would lead to the cure of the very worst forms of the disease. Perineal section, as described by Mr. Syme, was a "simple and easy mode" of curing permanently the most difficult cases of urethral stricture, and unattended with danger to life. That "division of a stricture by internal incision is sufficient for the complete remedy of the disease in its worst form; that in cases of less obstinacy, but still requiring the frequent use of the bougie, division is preferable to dilatation, as affording relief more permanently and safely."

From such a description, from such high authority, it cannot excite surprise that the new proceeding became, for a time, the fashion. No operation has probably been more fully tested than perineal section. That the sanguine expectations regarding the extraordinary efficacy of this operation should have ended in disappointment is nothing more than has occurred with other remedial measures which have disappointed expectation.

[Mr. Wade remarks that it is to Mr. Whately we are indebted for the introduction and strong recommendation of potassa fusa, as a valuable therapeutic agent in the dilatation of the more obstinate forms of urethral stricture. Its effects are totally dissimilar to those of nitrate of silver, which causes an adhesive form of inflammation.]

I have lately seen expressed an opinion that strictures, in the dilatation of which potassa fusa has been employed, have a greater tendency to recontraction than others in which that remedy has not been used. On the contrary, the application of the potash, by removing more or less of the thickened tissues, has the effect of diminishing instead of increasing their contractile tendency. This statement is not lightly made; but from a knowledge that in some of the more aggravated forms of hard gristly strictures, in which I was enabled, by the use of potassa fusa, to effect full dilatation after its failure in skilful hands by the ordinary means, the patients experienced no difficulty in preventing a return of the contraction by adopting the precaution of occasionally passing for themselves a bougie or sound.

In my advocacy of the use of the caustic potash as an assistant to dilatation in the more aggravated forms of urethral stricture, I have had to contend with the prejudices of the day, which were all in favour of cutting, and so strongly against the employment of caustic, that to use it was regarded as heresy. Had it not been for this prejudice, the ample evidence adduced by myself and others of the efficacy and safety of the potassa fusa in urethral stricture would surely ere this have led to its more general use in bad cases. It may truly be said that there is much in a name; and had the potassa fusa been called a solvent instead of a caustic, the case would have been far different.

When the potassa fusa is employed as a remedy for the more obstinate forms of hard gristly strictures it must be as an escharotic, and applied much more freely than recommended by Mr. Whately, who gives the following directions for its employment:—"In every stricture, before we apply the potassa fusa, we ought to be able to pass a bougie into the bladder of at least a size larger than the finest kind. A small hole about the sixteenth part of an inch deep should be made at the extremity of a bougie, which should be just large enough to enter the stricture. A piece of broken caustic, half the size of the smallest pin's head, should be selected; the particle cannot, indeed, be too small for the first application. Let this be inserted into the hole of the bougie, and pushed down into it so as to sink the caustic a very little below the margin of the hole. The hole should be contracted a little with the fingers, and the remaining vacancy in it filled with hog's lard. The armed bougie is to be passed once or twice backwards and forwards through the stricture. At the end of seven days the application may be repeated. The operation should be repeated till the contracted part of the urethra is dilated, if possible, to the natural size. I do not in any case apply more of the kali purum at a time than a piece about the size of a common pin's head. If a bougie cannot be passed into the stricture, and it becomes necessary to apply a caustic to the anterior part of the contraction, I should certainly prefer the lunar caustic to the kali purum."

It will be seen that Mr. Whately did not recommend nor use the potassa fusa in impermeable strictures. It was in impermeable urethral obstruction that I first had recourse to caustic potash, and very soon became convinced of its superiority to nitrate of silver in such cases. I found that to be effective in old hard gristly strictures, it was necessary to employ it much more freely than recommended by Mr. Whately, and that this might be done with perfect safety. I have on former occasions stated that the caustic potash may be advantageously applied to strictures for two purposes; one, to allay irritation; the other, to remove the thickened tissues which form the obstruction.

The plan which I adopt is, to commence with a piece of the caustic, the size of a common pin's head, inserted into a hole made in the point of a soft bougie. If the points of the caustic are well covered with lard there need be no fear of its acting before it reaches the stricture. The bougie should be gently pressed against the stricture for a minute or two if impermeable, and then withdrawn. When the caustic is applied to permeable obstructions, the bougie should be passed several times slowly backwards and forwards over the whole surface of the stricture. When applied to permeable strictures, it usually happens that after one or two applications of the caustic the bougie will be found to enter the obstruction. To surgeons who object to the employment of potassa fusa from the fear that the common bougie is not a safe conductor for the caustic, I recommend the employment of the tubes or portes-caustiques, which I sometimes use where there are false passages. A description of these tubes will be found in the Medical Times and Gazette of April 15, 1859.

As all who read this paper may not have read my previous observations on this subject, it may be as well to state that the cases in which I have found the potassa fusa most advantageous may be generally described as:—1. Strictures having a cartilaginous hardness, impermeable as well as permeable. 2. Strictures which bleed more or less freely on the introduction of the bougie. 3. Irritable strictures.

To a long hard gristly stricture it will be necessary to increase the quantity of the caustic; which, however, should always be very gradually done. It cannot be expected that a piece of the size of a common pin's head can have much solvent effect upon a hard surface of half or three-quarters of an inch long.

Two or three mild applications to strictures disposed to bleed on the introduction of the bougie will generally remove their hemorrhagic disposition.

An occasional application of the caustic potash will be found highly useful in highly irritable strictures, much facilitating their dilatation. A case may, however, occasionally occur in which the use of potassa fusa and the bougie alone may disappoint expectation. An irritable stricture when greatly contracted, and complicated with perineal fistulæ, often proves very troublesome, and may baffle the best efforts of the surgeon so long as the patient goes about following his usual avocations. In such case rigors are apt to occur even after the most gentle use of instruments. As the difficulty in accomplishing dilatation in such a case undoubtedly arises from the urine passing over and through the fistulous openings in the urethra, which keeps up more or less irritation and inflammation at the seat of the disease, it must be remedied by the retention of the catheter, which, of course, requires the confinement of the patient to his room. With this addition to the occasional application of the potash success will generally be obtained. The case must, however, be closely watched, or the retention of the catheter, if not carefully attended to, may do harm rather than good. Time will not permit me on the present occasion to give cases in illustration of this method, or to enter into further details.

It will be seen that my views with regard to this method of treatment differ materially from those of Mr. Whately. I do not use the potassa fusa in all cases indiscriminately, but only in such as do not yield to simple dilatation. I have found it generally necessary to employ the caustic alkali in much larger quantities than he recommended, the minute portions used by him having produced scarcely any perceptible effect upon strictures which, however, yielded to its more free application. But its greatest value is in impermeable strictures, to which Mr. Whately did not consider it applicable. These observations cannot be regarded as disrespectful to the memory of that able surgeon, as all improvements in our profession are progressive.

In concluding this part of my subject, it may be as well to state that the method of treating urethral stricture by potassa fusa was brought forward by me in a paper read at the Westminster Medical Society, on February 15, 1840. It was only after having fully tested its great efficiency and perfect safety in many of the more intractable forms of urethral stricture, that I ventured to bring before the profession my observations on this subject, well knowing the prejudices with which I should have to contend.

The strong testimony which from time to time I have published regarding its efficiency and safety, has, I am happy to say, at length led to its employment by some of our best practical surgeons; and I entertain not the slightest doubt that the potassa fusa will at no distant period be regarded as one of the most valuable therapeutic agents in urethral stricture.

With regard to constitutional treatment, I shall only observe that strict attention to the general health is as necessary in the treatment of strictured urethra, as in other local affections of a more or less serious character; and that the surgeon will do well to bear in mind the principles inculcated in Mr. Abernethy's memorable "Observations on the Origin and Treatment of Local Disease."

In these days of "Conservative Surgery," it may be worth asking how far the employment of caustic potash as a substitute for the knife may be regarded as entitled to any honour in this question. If an operation is the "opprobrium of surgery," any remedy which prevents the necessity of such operation is, at all events, "conservative."

I leave others to determine how far the potassa fusa answers the purpose for which it is intended as a "conservative" agent.

The conclusions arrived at from the preceding remarks may be thus briefly summed up:—

That simple dilatation is the method which should commonly be adopted; and that it will, in the great majority of cases, succeed in affording satisfactory and permanent relief.

There are, however, numerous cases in which the relief obtained by this method will be neither sufficient nor permanent; and in these the patient must depend for any considerable improvement in his condition, either on the application of caustic to his stricture, or on its division by some cutting instrument.

That internal section, when practised anterior to the bulb, is attended with but very slight risk; but when had recourse to for obstructions at the urethral curve, it has not unfrequently proved dangerous.

That external division, whether practised according to the old method, in impermeable, or that of Mr. Syme, in permeable stricture, from its disastrous results, is an operation justifiable only in the most urgent cases.

That Mr. Syme's operation will very seldom be required, it being only applicable to permeable obstructions, as every surgeon knows, that after an instrument has been passed through a stricture, the greatest difficulty of the case ceases.

That in whatever manner a stricture may be divided, to preserve the opening made by its division, in most cases, it will be necessary to have recourse to the regular use of the bougie, or other dilating instrument, for a considerable length of time afterwards.

That in intractable cases, as a general rule, I believe that the use of the potassa fusa will be attended with the most beneficial results, rendering it unnecessary to resort to the knife. I am fully convinced it is a perfectly safe proceeding.

In concluding these few brief comments on the treatment of urethral stricture, I can truly say that nothing but my knowledge that the use of the potassa fusa, in bad cases of that disease, will, in most instances, prevent the necessity of resorting to operations attended with more or less danger, has induced me so perseveringly to bring before the profession my views with regard to its employment.

I now again recommend my professional brethren to give the potassa fusa a fair trial in the more intractable forms of urethral obstruction. It must be recollected that, in recommendation of this plan of treatment, it involves no danger; that it is impossible to do harm when properly applied; and that, even should it be deemed necessary to have recourse to the knife, the surgeon will, at least, have the satisfaction of knowing that he has done all in his power to save the patient from a formidable, dangerous, or even fatal operation.—*Med. Times and Gazette, June* 18 *and* 25, 1859, *pp.* 625, 650.

72.—ON LONG-STANDING DISEASE OF THE PROSTATE AND BLADDER.

(Under the care of Mr. Henry Thompson, University College Hospital.)

Enlargement of the prostate is an affection which appears only in advanced life, notwithstanding the impression entertained by many surgeons that it is a disease very commonly met with when the hair begins to turn grey. In relation to this question, Mr. Henry Thompson states, in his recent work "On the Enlarged Prostate," that although it never appears but at the period we have mentioned, yet "it is not, therefore, a natural or necessary concomitant of age. It is, on the other hand, a complaint which the very large majority of elderly men escape. Contrary to the generally received opinion, its occurrence is not normal, but exceptional." (p. 65.) Mr. Thompson's facts prove very clearly that prostatic enlargement, so far from being an invariable or usual change in the aged, is an exceptional condition. We refer to this important fact here because the mistake is often made of attributing retention of urine to hypertrophy of the gland when the patient has reached a certain age. In the following case, however, in which the prostate was four times larger than natural, it produced retention of urine; and we remember a very similar case under Mr. Curling's care, at the London Hospital, some months back.

In the present instance, besides the enlargement, there was much disease, which was participated in by the bladder, producing retention of urine to the extent of five pints. The precaution adopted in this case of drawing it off at intervals, to prevent a fatal syncope, is an essential point which cannot be too much insisted upon; for we have seen, on more than one occasion, a fatal result ensue, in an aged patient, by completely emptying a distended bladder at a single sitting. Mr. Thompson, so far as we are aware, appears to be the only writer who specially draws attention to the subject, and the value of his remarks is our excuse for quoting them entire:—

"In very rare instances, the removal of a large quantity of urine, amounting to several pints, has been followed by fainting and depression, from which the patient has never rallied. When the extent of vesical dulness is very considerable, it is therefore prudent to afford relief in a gradual manner, and, supposing that the catheter is retained, this may easily be accomplished. The removal of some thirty or forty ounces will probably afford complete ease, and after the lapse of half an hour or an hour, another portion may be withdrawn; in this manner the bladder may be gradually brought to adapt itself to the normal condition of contraction, which subsequently, as a rule, must be ensured at least once or twice a day." (p. 180.)

The interest and importance, therefore, of the subjoined case will be at once recognised, for the patient was not only old, but weak and debilitated. A fatal syncope was averted by the treatment adopted, but, owing to other causes, he succumbed nine days afterwards.

An extremely infirm old man, aged seventy-eight years, was admitted for relief of retention of urine on the 27th of April, 1859. Several attempts had been made before his admission to pass a catheter, but without success. Mr. Thompson, being in the ward at the time, examined him immediately, and found the bladder forming a large tumour, reaching to the umbilicus. The patient was suffering great agony, and was much exhausted. He passed a No. 9 silver catheter without difficulty into the bladder, when a quantity of dark-coloured and fœtid urine flowed. The patient showing signs of increased weakness, and the pulse, which was carefully examined throughout, becoming very feeble, thirty ounces only were withdrawn, and the catheter stopped, and tied into the bladder—Mr. Thompson remarking that a large quantity of urine remained in the bladder, and that in such a subject it was extremely dangerous to withdraw more than a moderate quantity at a time. He stated that he had known death from syncope to occur through neglect of this precaution. The prostate was found to be considerably enlarged by rectal examination, and the bladder pressing down into the bowel from extreme distention. An hour afterwards, he withdrew rather more than a pint, and four or five hours afterwards, more than two pints and a half, which emptied the bladder. More than five pints had thus been withdrawn in the same number of hours. The catheter was left in. Stimulants and strong fluid nourishment to be taken freely.

April 28th. The silver catheter was exchanged for a gum catheter. The patient much better, and expresses himself as greatly relieved.

29th. The gum catheter having slipped out in the night, it is found that he has no power to void any urine by his own efforts. It is replaced, and a piece of India-rubber tube, about four feet long, is attached to the mouth of the catheter, the other end of the tube being placed in a vessel beneath the bed. Mr. Thompson is in the habit of adopting this plan, which effectually prevents the bed from being wetted, and carries off offensive urine to any distance from the patient which the condition of it may make desirable. In this case, the urine is extremely fœtid, and loaded with muco-purulent matter.

[The patient gradually sunk, and died on the 5th of the next month. The prostate was found enlarged and indurated, the bladder sacculated and hypertrophied; kidneys congested; ureters not enlarged.]

[The following is from Sir B. Brodie's Lectures on the Diseases of the Urinary Organs (p. 205, fourth edition, 1849.) He strongly enforces the necessity of avoiding the sudden emptying of the bladder in cases of great distention of that viscus. He observes :]

"The resemblance between the effects produced by the use of the catheter, in the way and under the circumstances which I have just endeavoured to describe. and those which follow the operation of lithotomy in a patient similarly circumstanced, is too obvious to be overlooked; and I conclude they are to be referred to a common principle. The system suffers from the shock of the operation in one case. and in the other case it suffers in the same manner from the impression made on it by the sudden emptying of the over-distended bladder, and consequent removal of the pressure which is made, through the medium of the dilated ureters, on the glandular structure of the kidneys.

"Here, then, arises an important practical question. The patient has no chance of recovery without the use of the catheter. Are we to leave him to his fate? or are we to empty his bladder at certain intervals, at the risk of hastening his dissolution? I have no doubt that we may, in many instances at least, obtain the good and avoid the evil, by a slight modification of the treatment. Let the catheter be introduced at first, so as to draw off only a portion of the contents of the bladder, and let several days be permitted to elapse before it is completely emptied; care being taken at the same time, to uphold the general health by the exhibition of ammonia, quinine, and other tonics, exhibited according to circumstances, and combined with the prudent use of wine or brandy, and a plain, but nutritious diet."—*Lancet, June* 18 *and* 25, 1859, *pp.* 610, 639.

73.—ON SOME OF THE DIFFICULTIES ATTENDING UPON THE TREATMENT OF STRICTURE OF THE URETHRA.

By Henry Smith, Esq.

In the Medical Times and Gazette for August 21 of last year, I detailed at length, a case of obstinate stricture of the urethra, which was remedied by dilatation and careful general treatment, after the patient had been condemned to undergo the operation of external division of the canal by a surgeon of large experience, under the idea that there was not any other method of relief. At that time I made some observations, referring chiefly to a feature of difficulty which the case detailed particularly illustrated, namely, the almost insuperable rebelliousness to dilatation, which is every now and then met with in certain instances of this affection. I was enabled to show, that a case of the most obstinate and unpromising nature might, by careful treatment, be made to yield, without recourse being had to the knife, although both the patient and his surgeon were convinced that it was the only remedy.

[Though generally the improvement in the stream of urine is commensurate with the mechanical progress, yet occasionally cases occur in which there is an absence of improvement in the power of passing the urine, although dilatation has been carried on in a satisfactory manner.]

Next to extreme irritability of the urethra, this one feature of absence of improvement in the stream, has been a source of greater difficulty and annoyance than anything else. Among a considerable number of very severe cases of stricture which have lately been under my care, my attention has therefore, been much directed towards ascertaining its cause, and removing the symptom, if it may be so termed. In some cases it has not been difficult to account for it, as where one or more fistulous sinuses exist; when such is the case, the bladder may be healthy, and the urethra be fairly dilated, yet the patent condition of the artificial openings, which take so long to close, allows the greater portion of the urine to percolate through them, little comes through the urethra, and that little in any volume. In instances of this kind the patients may be assured that the stream of urine will increase as the sinuses close up, slowly as that event is too often known to take place.

There are, however, other cases of severe stricture, uncomplicated with fistulous openings in the perineum, where the surgeon has been able to carry on dilatation in a satisfactory manner, and to such an extent that a good-sized catheter may pass, and yet there is either no improvement at all in the stream, or, if any, it is so slight as to be almost inappreciable. This obtains, also, not only in instances of stricture in persons of advanced age and of debilitated power, but it is occasionally met with in patients either in the very prime of life, or in those not much above adult age. It is in some of these latter cases that it is extremely difficult to understand the meaning of this symptom.

It is considered by some men of large experience, that it is the bladder alone which is in fault; that it has become weakened and dilated by the persistence of the obstruction in front; and that, even when this latter has been removed, the viscus does not recover its tone sufficiently to expel its contents in an effectual manner. That this is the true solution of the question in some instances there cannot be much doubt; but I am induced, by careful observation, to believe that in a considerable number of these cases the loss of power in the bladder is more apparent than real, and that the impediment to the volume and flow of the urine is in the urethra itself, notwithstanding that dilatation has been carried on to a measure with which the passing of urine in a small stream, or in drops even, seems almost incompatible. I have noticed that this very distressing feature has existed both in instances where the stricture has been exceedingly difficult to penetrate, the canal not being especially irritable, and in those cases where there has not been very much difficulty in overcoming the obstruction; but the urethra has been extremely sensitive. I have, moreover, noticed, especially in the latter cases, that more than a single stricture has existed; that there has been one near the meatus, or one or two inches from it; and as a rule, with but rare exceptions, the anterior stricture or strictures have been found to be most irritable and unyielding.

In such instances as these it is extremely difficult to overcome the irritable condition of the urethra, and to dilate the canal; and then, after this has been accomplished to such an extent that a No. 8 or 9 catheter is introduced, there is the mortifying result of little or no increase in the stream of urine, or even of a diminution in size. Now, I believe that in such instances, especially where the patients are young or the stricture has not lasted long, the fault is not in the bladder, but that that organ expels the urine with its wonted power, and that the fluid coming in contact with the irritable portion of the canal—although it has been dilated to the extent mentioned—causes it to contract forcibly, and thus produce the fine stream observed. Careful examination of the urine in these cases shows absence of any disease of the bladder; and moreover, it will be seen that, small as the stream is, it is expelled with considerable force and continuously; which circumstance will not be produced, I apprehend, by the mere action of the urethral muscular fibres, whether voluntary or involuntary. In a very well marked case of simple loss of power in the bladder lately under my care, in the person of a very fine young officer, all the symptoms of stricture were present; but there was hardly any impulse at all given to the urine as it was being evacuated, and the patient was compelled to strain violently. There was not the least obstruction in the urethra itself, and by well emptying the bladder artificially, and by attention to the general health, this viscus gradually regained its tone, as evidenced by the increasing size and force of the stream.

In the treatment of the cases I have been considering, much patience is required, both on the part of the surgeon and the sufferer; for, as I have before stated, the latter is too apt to be greatly disappointed at the little improvement which is perceptible, and in his distress is too liable to be misled into undergoing some heroic treatment which may either destroy his life, or give relief merely for a brief period. It is only by the continuous dilatation of the diseased canal to as great an extent as it will admit of, that the result so much desired will be obtained. If the patient is not advanced in years, or has not had stricture for a long time, he may be assured that a persistence in the treatment will be attended with satisfactory results; sometimes the desired relief will happen suddenly, at other times, and most frequently, the increase in the power of urinating will be only gradual, but will not be marked until an instrument of considerable size has been introduced. When, however, the features of the case indicate that the want of stream is due to a loss of power in the bladder, a long time elapses before the organ recovers itself, although the urethra has been well dilated by the surgeon, and is kept patent by the sufferer himself. It is especially important in these cases to attend to the general health; for it will not unfrequently be found that this has suffered much, and that the loss of power in the bladder is but a symptom as it were of constitutional debility, rather than the mere result of an obstructed urethra.

Sometimes it happens that in a severe case of stricture the earlier attempts to dilate are attended with an improvement in the stream which is satisfactory to the patient. Then this improvement ceases, and does not show itself again for a long period, although the dilatation may be steady and progressive. When such a circumstance occurs, there can be no doubt that the bladder is not in fault; but that it depends upon the irritability of the urethra, or an insufficient dilatation of the stricture. Without going into details, I may mention a case of a patient, aged 45, who was under my care from December to April; the stricture was of fifteen years' standing and situated in front of the bulb. Treatment at the hands of various surgeons of experience had been pursued, one of whom had performed internal incision on two occasions. The urethra, moreover, had unfortunately been wounded on one occasion through the stilet of a gum-elastic catheter escaping from the instrument. I commenced the treatment of this difficult case by dilating with silver catheters. The size and power of the stream of urine increased in proportion with the instruments, until I had got up to No. 4 and 5; but subsequently to this there was a good deal of irritation, and when a No. 8 or 9 had been introduced, the stream of urine was not larger than when a small instrument had been passed. Dilatation was, however, patiently persisted in, and after No. 10 had been introduced the size of the stream again increased and continued doing so until I had arrived at No. 13, when I dismissed him, able to pass his water in a good stream.

This case is interesting, as illustrating not only the particular point I have been dwelling upon, but it shows also how utterly useless is the practice of internal incision, in remedying a stricture for any length of time.

The practice of external division of stricture, which a misapprehension of some of the symptoms and an insufficient reliance upon the power of dilatation and other agents, was the means of bringing into use a few years since, has now been proved beyond all doubt to be so dangerous to life, and so inefficient to produce the complete remedy once so vaunted, that it may hardly seem necessary to consider the subject here.

I have before enunciated the opinion that not only does this operation not prevent the return of the stricture, but that the re-contraction becomes more severe than before.—*Med. Times and Gazette, Aug.* 13, 1859, *p.* 157.

74.—ON THE EMPLOYMENT OF EXTRACT OF BELLADONNA IN THE TREATMENT OF IRRITABLE BLADDER.

By Henry Behrend, Esq.

[Cases of irritable bladder seem to be on the increase, especially among the wealthier classes of society—the disease being much predisposed to by enervating and luxurious habits. Numerous authors within the last two or three years have borne testimony to the efficacy of belladonna in the treatment of this affection. The success of the treatment in the following case is striking.]

The patient was a married lady, without family, about thirty years of age. Some five or six years ago she had suffered from acute dyspepsia, but shower-baths and horse exercise had completely cured her, and she had enjoyed uninterrupted good health until about two years ago, when she was suddenly, and without any assignable cause, attacked by the complaint for which she first consulted me in August, 1858. Previously to its commencement, which was in May, 1857, she had always slept remarkably well, and had seldom or never been disturbed during the night; but during the last fifteen months, the irritability of the bladder had been so great as to render the evacuation of its contents imperative at least three or four times during the night, and often as frequently as seven or eight times, or even more. During the day, there was little or no irritability, and the quantity of urine passed was normal, or nearly so; but in the course of the night, two or three times the natural amount was passed, pale, insipid, and, when tested, free from sugar, albumen, or other abnormal constituents. The combined effects of the loss of rest and the drain of fluid from the system had materially affected her general health. She had lost flesh, and suffered much from thirst, headache, and nausea, especially upon rising in the morning. She was much depressed in spirits, and took a desponding view as to the ultimate result of the malady. I prescribed successively the tincture of the sesquichloride of iron, compound tincture of valerian, tincture of hyoscyamus, liquor potassæ, dilute mineral acids, sea bathing, and change of air and scene, without the least amelioration of the symptoms; and upon her return to town at the commencement of October, I decided upon giving the extract of belladonna a trial. She began taking it in doses of the twelfth o a grain three times a day in the form of a pill, and was at this period always disturbed four or five times in the course of the night, and often much more frequently. The belladonna was at once increased to the third of a grain three times a day, or a grain in all, as soon as I found that its use was not forbidden by any peculiarity of constitution. These doses were continued for about six weeks (with the occasional intermission of a day or two), at the expiration of which period its toxical effects began to manifest themselves; for though the pupils were not dilated, yet vision was not normal: black spectra appeared: the mouth and fauces were parched and dry, and there was occasional nausea. Already the improvement in the symptoms was decided: my patient slept better, and was never disturbed more than three times in the night.

As it is a recognised fact, that in order to obtain the full amount of benefit from the belladonna, it must be pushed until its specific symptoms are quite established, I now increased the daily amount taken to a grain and a half, in the proportions of half a grain in the morning, and one grain at nine P.M. In the course of three or four days, the pupils became dilated, the nausea extreme, and there were repeated efforts to vomit, for the most part ineffectual, but occasionally followed by a little glairy mucus. The irritability of the bladder became almost entirely subdued; she was disturbed once only, or at most twice, throughout the night, and the quantity of urine passed was normal, or only occasionally slightly increased. The belladonna was at once discontinued, the general health rapidly improved, and during the past six months the cure has been permanent, and my patient has continued perfectly free from any recurrence of her distressing complaint, except that a slight tendency to irritability of the bladder manifests itself now and then, for one or at most two nights in succession, but passes away of itself, and is not of sufficient consequence to require any treatment.—*Lancet, June* 25, 1859, *p.* 630.

75.—*On Amputation of the Penis.* By THOMAS P. TEALE, Esq., Surgeon to the Leeds General Infirmary.—[In a note on this operation, referring to the difficulty which is not unfrequently experienced after amputation of the penis in keeping the orifice of the urethra open, Mr. Teale remarks:]

To obviate this difficulty the following simple plan has been adopted, and, as far as I have observed, invariably with success. After the operation of amputation of the penis has been performed in the ordinary way, a director is introduced into the urethra, and by the aid of a bistoury the urethra and the skin covering it are slit up to the extent of about two-thirds of an inch. A single suture is then placed on each side of the slit uniting the mucous membrane to the skin. Perfect patency is thus given to the orifice, which is of a long oval form; and, after cicatrization is complete, there remains a free opening into the urinary canal. Hereby, the free discharge of urine is secured, without any mechanical aid being required, not only during the days immediately after the operation, but also after the cure is complete.

I have adopted the same plan with equal success in the treatment of obstinate stricture at the glans penis. Cases of this kind are occasionally met with in hospitals, resulting from the cicatrisation of sores within the urethra near its orifice. These strictures are readily relieved by bougies, and, the patient instructed in the use of these instruments, might easily keep the passage open; but he neglects to do so; the disease relapses, and he presents himself again and again at the hospital. Such cases I have seen permanently relieved by slitting up the urethra to the extent of two-thirds of an inch, and uniting the mucous membrane and the skin on each side by a single suture.—*Med. Times and Gazette, Oct.* 8, 1859, *p.* 354.

76.—*Silver Wires in Hydrocele.*—We have previously referred to the subject of the treatment of hydrocele by the passage of wires through the sac, which plan of radical cure has proved successful in several cases under Mr. Pollock's care, at St. George's Hospital. On the 12th of August, he tapped a very large hydrocele of the right side in an elderly man, and withdrew about thirty ounces of fluid, containing a large proportion of cholesterine. A needle and wire were then passed through the canula and out of the sac. The canula being withdrawn, the wire was then tied, and thus formed a seton. A second case of hydrocele of the right side was likewise tapped, six ounces of fluid evacuated, and the same process adopted of introducing a wire seton.

These two made the fourth and fifth cases thus treated, and with success.

The first instance was an example of encysted hydrocele of the cord, five months ago, the patient being in the hospital a week. No irritation was caused by the wire, but it produced an amount of consolidation of the parts sufficient to obliterate the cyst. The second and third cases were those of ordinary hydrocele. In these, however, there was not the same power of bearing the wire, and it had to be removed in forty-eight hours; in one it nearly produced suppuration; but at the same time the sac in each was obliterated.

Mr. Pollock considers the advantage of using the wire seton to be, that the patient suffers much less pain than when the sac is injected with iodine; in other respects, the results are about the same.—*Lancet, Sept.* 10, 1859, *p.* 266.

DISEASES OF THE SKIN.

77.—ON THE USE OF PIGMENTUM ALBUM IN SOME CUTANEOUS MALADIES.

By Alfred Freer, Esq.

I wish to call the attention of the profession to the great value of white paint as a remedial agent. The preparation itself is nothing more than a mixture of linseed oil and carbonate of lead, rubbed up into a semi-liquid substance. I first became acquainted with its great efficacy in the treatment of erysipelas by my late father, and by my brother. It is, indeed, in this disease that the most striking benefit results from its application. I have never yet met with a case of this nature where it has not done immense good. I find it far superior to lead lotions, mucilage, hot fomentations, nitrate of silver, or collodion. After erysipelas, the paint proves of the greatest service perhaps in eczema in its several forms. In chronic eczematous eruptions of the aged it affords much comfort, and often speedily effects a cure. Of late years I have extended its employment to other complaints of the skin, including herpes in its several forms. I have tried it in some cases of small-pox, with the view of diminishing the number of vesicles on the face, and of controlling their size. The latter indication it seems likely to fulfil; but I cannot speak with confidence about the former, the papules being already numerous at the time of my visit. I have also used it in several cases of carbuncle and furuncle. The first was in an instance of a huge carbuncle, situated on the loin of a man, and rapidly extending, notwithstanding free incisions, linseed poultices, and appropriate constitutional treatment. I applied a thick, wide circle of paint round the swelling, and dressed with resin ointment and cotton wool. There was no advance of the disease from that time, the centres rapidly broke up, and recovery took place. It is, however, probable, that the omission of the warm poultice may have contributed to the improvement, for I have often observed that warm poultices, however well made, seem to foster and spread carbuncular inflammations.

The paint seems to act in two ways: first, and chiefly, as an efficient excluder of the air—that great irritant to the cutaneous surface when disordered; and, secondly, as a direct sedative to the sentient nerve filaments, rendering them less prone to become involved in inflammatory action. In boils it relieves the painful tension, and favours resolution. In some forms of painful ulcers of the leg, of a small size, it gives great relief. In galling of the skin, where anasarca is present, it is also of use, and is the best application that we have in burns of the first and second degree. But it is in erysipelas that its triumph is most manifest; the patient soon finds the comfort of it; the tight shining skin soon becomes wrinkled and shrunken; indeed, the inflammation very rarely extends after the second or third painting.

All my friends to whom I have recommended the pigmentum album speak highly of it; and one, who is a surgeon in the Peninsular and Oriental Company's service, has used it for the last two years with great success.

The manner of applying it is by means of a feather; painting the affected parts, and *a little beyond*, and laying on a fresh coat every two hours or so, until a thick layer is obtained, and then sufficiently often to maintain a covering. In erysipelas, it peels off in a week or so with the shed cuticle, leaving beneath a smooth, clean, healthy surface. Patients are struck with the benefit they derive from its employment.—*Lancet, June* 18, 1859, *p.* 610.

78.—ON INCISIONS IN ANTHRAX.

By Maurice H. Collis, Esq., Surgeon to the Meath Hospital, and County Dublin Infirmary.

The incision into anthrax, whether made early or delayed till sloughing has done part of the surgeon's work, must be deep rather than extensive. Usually it is said anthrax is a flat swelling. The fact of its flatness, or rather of its extent, hides the real amount of elevation, which is, in most cases, considerable. Hence incisions into anthrax seldom go down *through* the inflamed skin and areolar tissue. But even if they did go down to the fascia, they would fail in effect unless they also went through it. The fascia is highly inflamed in anthrax; in fact the essential difference of anthrax from furuncle consists in the inflammation being deeper and implicating the fascia. When fascia is inflamed, much plastic exudation takes place, both in its substance and under it; and the tendency of anthrax to spread indefinitely is to be thus accounted for. The pent-up plasma, quickly producing pus and slough, can get no vent until there is an adequate opening in the fascia, and this opening should be made by the surgeon as early as possible, if he would avoid the unpleasantness of useless and repeated cutting, and the extensive sloughing which will occur if he neglect to make it. Plastic exudations find great facility in travelling under the fascia, dissecting and destroying its vascular connexions, and ultimately causing much of it to perish. This is well known, as a general principle of surgery, and it is strange to find it overlooked as the cause of the spread of anthrax. We readily acknowledge the mischief it does in periostitis, in diffused inflammations of erysipelatous character or connected with paronychia, and in many other analogous cases; but books of surgery are, for the most part, silent about it in the case of anthrax. And yet every one must have observed phenomena which can only be explained by it. The extent and mode of extension of the swelling, the real depth to which the surgeon must cut if he is to do good rather than harm, and the fact of large flakes of fascia ultimately coming away as dead core (in addition to areolar tissue), leaving the underlying muscles bare, must have been often observed, and must often, doubtless, have had their influence on the practical observer; but the junior surgeon and the pupil have not been shown their practical bearing. The rule I have given above, to cut deep rather than wide, is founded on the observation of these facts, and will be found satisfactory, saving the surgeon the opprobrium of cutting twice or oftener without benefit to his patient. It is very easy to know when we are deep enough; by taking hold of the flaps made by our crucial incision, we feel if they are quite loose. Our incision is not deep enough unless we can lift up the point of each flap with ease from the parts underneath. This cannot be done unless our knife has gone through the fascia, and made a crucial incision in it almost as extensive as in the skin. The wounds we have made should be almost as deep at their extremities as in the centre, where they intersect. If we have made our incisions early, before actual sloughing has commenced, as we sometimes, though rarely, have an opportunity of doing, the flaps will curl up if the wound is deep enough, and will leave a widely gaping wound; but if we do not see the anthrax until more or less of the skin is undermined and dead, the gaping of the wound will not be so marked, and the best test is the one I have given above, of lifting the flaps with a forceps, and proving that they are loose. If this rule is followed, we shall have few cases in which we must come and cut again.—*Dub. Quarterly Journal, Aug.* 1859, *p.* 205.

79.—*Chlohydric Acid in Cutaneous Affections*. By M. KLETZINSKI.—From a great number of experiments made with different agents, it results, according to the author, that none provokes or stimulates the cutaneous perspiration more than chlohydric acid. A portion of the skin wet with this liquid perspires in the same time, and under the same circumstances, 27 to 30 per cent. more carbonic acid, and, what is remarkable, 7 to 12 per cent. less water, than the same portion not wet. The author concludes that,

1. The chlohydric acid re-establishes the circulation of the blood when it is periodically interrupted, and accelerates it. It consequently cures chilblains, and even prevents them.

2. It diminishes the uncomfortable sweatings of the hands and feet, and causes them to cease entirely after a long-continued use of it.

3. It is employed with success for the following diseases of the skin: *acne sebacea*, *nævi*, *condylomatas*.

4. It does not injure the skin in any way, if properly used; on the contrary, it softens it, and can be regarded as a real cosmetic.

When employed, it should contain neither iron nor chlorine. It is used as concentrated as the patient can bear without feeling a burning sensation, and after thirty to sixty seconds the wet part is washed at first with pure water, and then with soap and water. For application to very sensitive parts of the skin, the acid should be greatly diluted with glycerine, and neutralized in a very short time.—*Echo Médicale.*—*American Med. Monthly*, *June* 1859, *p*. 460.

80.—DISINFECTANT POWDER FOR GANGRENOUS SORES.

[The authors of the discovery of the powder, which has been pretty extensively used in Paris are MM. Corne and Demaux, the former a medical practitioner in the south of France, and the latter a veterinary surgeon.]

This disinfectment, or absorbent, is composed of two very simple substances in given proportions, viz., 100 parts of common plaster of Paris, with from one to five parts of coal tar. These two substances are triturated in a mortar, or by means of some other suitable mechanical apparatus, the result being a greyish powder, having somewhat the appearance of prepared chalk, and possessing a moderately strong bituminous odour. It may be applied to the surface of sores simply in the shape of powder, or it may be used in the form of an ointment produced by combining the powder with a certain quantity of olive oil, the consistency being determined by the greater or less quantity of oil employed in its preparation. The oil (unlike water, which would set the plaster,) causes the particles simply to cohere in such a manner that it, by the gradual elimination of the oil, still retains the property of absorbing the pus so long as it remains in contact with the suppurating sore: a certain portion of the ointment is spread over a piece of rag proportionate to the dimensions of the sore, and is applied to it either directly, or a layer of some thin texture is made to interpose, according to the nature of the case or the object contemplated. Its direct application to a sore does not produce the slightest irritation. According to Velpeau, this application, besides absorbing the pus and other morbid products engendered on the surface of sores, possesses the double advantage of disinfecting them, and so favours cicatrization. I have followed MM. Corne and Demaux in their experiments at La Charité, and have carefully observed the effect produced on different kinds of sores by their new mode of dressing; and on the whole, I have been very favourably impressed with it. I do not, however, wish to be rash in forming my opinion as to its merits until it shall have been more severely tested. One thing I have remarked, viz., that its application to sores of the most unhealthy and fœtid description, is immediately followed by the disappearance of all smell; but whether this is owing to its chemical action on the morbid product, or simply to its absorbent power, or to its bituminous odour predominating over every other, I cannot say, this being a question which remains to be settled by practical chemists. I have seen the powder applied to anatomical specimens in an advanced state of putrefaction, and the consequence has been the apparent arrest of the putrefactive process, together with the destruction of the peculiar odour which accompanies it.—*Med. Times and Gazette, Sept.* 3, 1859, *p.* 237.

81.—*Lupus Superficilias.*—We have watched with some interest the treatment of a case of the superficial form of lupus in a young man twenty-two years of age, who has been an inmate of the Charing-cross Hospital since the 22nd of March, under Dr. Willshire's care. The superficial layers of the dermis of the entire face and cheeks were effected, and at one time the disease was present on his neck also, the duration of it altogether being fourteen years. As our readers are aware, the skin assumes a red and angry look, with exfoliation of the cuticle and gradual thinning of the integument. It is characterized, too, by the absence of tubercles and scales. The treatment pursued here consisted of the internal administration of arsenic, dulcamara, elm-bark, and of cod-liver oil, the latter being likewise used as a local application. A really wonderful effect has been produced in the disease in a short time: the redness is diminishing and slowly disappearing, exfoliation has almost ceased, and the healed-up skin is assuming a thin and shining appearance, somewhat resembling ambustial cicatrices. Some authors think this form of lupus essentially scrofulous.

Whilst on the subject, we may refer to another case, in an old woman, under Dr. Willshire's care, at the Royal Infirmary for Women and Children, wherein the disease has assumed the more severe form of lupus exedens. Arsenic is given internally, and the cod-liver oil is freely applied externally, with benefit, as the ulcerations are healing up. Dr. Willshire is using the extract of larch bark, in many different skin affections, in five-grain doses; it is a remedy of much value.—*Lancet, Aug.* 20, 1859, *p.* 186.

82.—ON SLOUGHING BED-SORES IN CASES OF PARAPLEGIA.

By Dr. BROWN-SEQUARD. (Being part of a lecture on Paralysis.)

It is remarkable that sloughing often occurs at a part in which there has been no pressure. We have hitherto considered sloughing to be due to the pressure on the paralysed part, but this explanation of sloughing cannot be admitted. If it were only pressure that was the cause of sloughing, we should find it coming on after a long time—after fifteen days or a month: whereas, we know that in fact it sometimes comes on a very few days after the injury to the spine, when it could not possibly have arisen from pressure. Neither can it be caused by the urine having come into contact with the part. Sloughing, therefore, must arise from some other cause, and that cause must be irritation of the cord—a fact which I have ascertained by experiments on animals. I have observed also a case in which, in consequence of a severe blow on the forearm, the patient suffered something similar in the formation of phlyctaenæ on the hand, which, on breaking, became ulcerated. In my experiments on animals, I found that injury of the cord was followed by this sloughing in parts not subject to pressure. Seven or eight cases of this kind have been recorded. I have also ascertained, by similar means, that this sloughing may be prevented; or if it has already occurred, that it may be cured, sometimes with great rapidity. The means I have employed and found to be most successful to prevent the formation of sloughing in an animal, when it has begun and not gone very far, are very simple; they are quite in accordance with the views I entertained on the subject, and have fulfilled more than I expected from them. My treatment is, alternating hot and cold applications. I apply sometimes eight or ten times a day morsels of ice to the parts threatened or affected, and then after the application of the ice, which of course causes contraction in the blood vessels, I apply a kind of poultice which is very hot, and of course produces a reverse effect

By these alternations of extreme cold and heat, I produce a change in the circulation of the part, and a consequent change of the nutrition. This mode of treatment I have found to be effectual in preventing sloughing from irritation of the spinal cord. I have seen sloughing in a dog, which had gone almost to the bone, cured in two days. The sloughing was stopped in its progress, and from that time there was no recurrence of it. In other animals, where the disease had not progressed so far, the cure was very rapid indeed. Another means which I have employed, and which I have found to be efficacious, is the galvanic current. This does much good, but it has not the power of the alternative treatment with the ice and hot poultices of which I have spoken.—*Dublin Hospital Gazette, July* 15, 1859, *p.* 212.

83.—ON THE IDENTITY OF PARASITIC FUNGI AFFECTING THE HUMAN SURFACE.

By Dr. Wm. Tilbury Fox, London.

Two statements I am going to make will be received with suspicion; I therefore do not utter them without being perfectly satisfied of their correctness. Any reason to doubt their truth would at once prevent my stating them.

Twice, in hunting about between the epithelial lining and the hair in severe tinea tonsurans, where the sporules were plentiful, I met with some largish oval sporules (achorion?), accompanied by certain bodies (five or six) which resembled the *sarcinæ ventriculi*, and seemed to be produced by the junction of four cells. (I had before observed occasionally two or three sporules joined together, and the effect of the reagent has been to swell them up, and obliterate in great measure their distinctness as separate cells.) So much did the resemblance to sarcinæ hold good, that had they been found in vomited matter, no one would have hesitated in instantly pronouncing them sarcinæ. It was the peculiarity of their seat alone which gave rise to doubt as to their nature. They were, perhaps, small, and the angles slightly more rounded than usual, if there was a difference.

Again, on one occasion I observed an appearance exactly resembling the torula, sprouting from the bottom of the follicle.

Arnsted, of Christiana, seven or eight years ago described a fungus as occurring in favus, which, from its likeness to the corn parasite, he called puccini favi. Mr. Hogg has noticed it in tinea tarsi. I have been unable to find it at present, but have noticed a condition of mycelium not unlike it, and which might be mistaken, I can conceive, for the puccinia. The mycelium sometimes *seems* to increase in breadth at the expense of the length, so to speak, and hence it becomes very short and broad—as though it were growing in a confined space, and unable to elongate. There is nothing in the microscopic history of the parasites of the human surface which contradicts the reasonableness of the opinion which regards them of one common nature (identical); and the minute differences may be readily explained by the variation in the several concomitant conditions of secretion, heat, and the like. Viewing them generally, as a whole, the tineæ do form a tolerably perfect series; and, whatever may be said to the contrary, there exist undoubtedly connecting phases between many of the separate varieties.

This mode of reasoning may be deemed deficient, but it may still be urged that these different appearances are really insufficient to establish the existence of different fungi, which occur (promiscuously) in the various forms of tinea. Well, what does clinical evidence say? Will one variety produce another? Are the tineæ mutually producible?

First, in regard to tinea favosa.—I remember Dr. Jenner telling his clinical class, some time ago, that at one time was admitted into the dren's Hospital a case of favus. No one caught the disease, though the children played together. But by and by in came a case of herpes circinatus. Several children were attacked by it, and at once two of them had

T. favosa. Well, this looks as though the herpes circinatus were contagious. The parasite in the herpes circinatus was better adapted to grow on the soil it found than the more developed condition of fungus in the favus; and increasing in the usual manner (circularly) in an apt subject, herpes circinatus resulted. The achorion (favus) required a better soil than existed before the admission of the herpes circinatus. It would be interesting to know if there were any case of severe eruption in the hospital at the time the favus was there; if so, we should have *expected* favus to have been produced in the eruptive subject. The facts show the relation which favus bears to the other varieties, and contain suggestions for future inquiry. A case has been mentioned of T. tonsurans, in which, by producing secretion by irritation, a favus crust was formed. Hebra believes that favus and tinea tonsurans are identical (different stages of one disease). Müller, Retzius, Remak, and Lebert, classify the achorion with oïdium.

Now as to tinea tonsurans.—Mr. Hutchinson's observations tend to show that the growth of the trichophyton may produce chloasma. He asserts that he has traced the communication from the scalp of infants at the breast affected with tinea tonsurans, and the proof seems sufficient; but I believe also that chloasma may be produced by the implantation of the oïdium albicans. Guersent, I think, some years ago, hinted that "thrush" might be communicated from the child to the mother's breast. I recorded a case in my former paper, which was observed very closely from the outset, and which, I believe, arose from the growth of the oïdium. The case was that of a young woman, a patient in the General Lying-in Hospital. No tinea was or had been present, but almost all the infants in the hospital suffered *severely* from thrush, and the child of the patient in question was, amongst others, attacked. Taking all the facts into consideration—the seat, the mode of onset, and the negative evidence—the conclusion that the chloasma of the mother was caused by the implantation of the fungus of the thrush seemed inevitable and certain. The patient herself was a little "out of sorts," and perspiring freely.

Herpes circinatus (*parasitic variety*).—We know favus may spring up in a patch of herpes circinatus. The latter may also give rise to T. tonsurans. Very recently a case of H. circinatus (parasitic) of the forehead and temple, in a young woman (out of health), came under my notice. The minute characters of the patches where those of T. tonsurans, which are always displayed by parasitic herpes circinatus. Now, curiously enough, the disease extended upwards, and at length reached the scalp, where it altered its aspect, assuming all the naked-eye and minute appearances of T. tonsurans. The herpes circinatus, reaching the scalp, became, so to speak, T. tonsurans. Be as sceptical as one might, the case seemed to have left no room for doubting the intimate relation existing between the two affections. From what I have seen of herpes circinatus, I think there is good reason to believe that it may give rise to sycosis. The case above seemed to exemplify this in a singular manner. It so happened, that on my own lip were two or three vesicles of *simple herpes*, just disappearing at the time the case came to me. I spent a good deal of time in examining the patches closely with a common lens, from day to day, and the fungus, by some accident, became implanted upon my face—at least I presume so, for the herpes became very irritable and inflamed, and pustular; indeed, unless I had used pretty active treatment, a promising patch of sycosis would have sprung up. The hairs, at their *cut ends*, were actually split up by the fungus, which was making its way down the follicle. The whole disease consisted only of three or four pustules, still the effect and presence of the fungus were diagnostic.

Again, "sycosis" in its turn may be produced from tinea tonsurans. Some time ago one or two members of a family became affected with well-marked tinea tonsurans: the father subsequently had not tinea tonsurans, but sycosis, (in all probability produced by contact with the children.) Dr. Lowe has given evidence of the production of favus and sycosis from the implantation of the yeast-plant. But recently (April) the grandmother of a numerous family, seven of whom were the subjects of tinea tonsurans, presented herself with a large patch of parasitic herpes circinatus on the left arm (unsymmetrical), the hairs of the part being infiltrated with sporules of the trichophyton; and subsequently the *mother* of the family herself has been affected like the grandmother, they all living together. *En passant*, I may mention the instance of a white cat, a great pet with the children of a family of nine, which evidently contracted the "*mange*" and tinea tarsi from T. tonsurans, which attacked five of the children. (The fungus of "mange" is the tricophyton.)

Plica polonica.—Of this I have no experience. The observations of Dr. Bidder make it the same as the ordinary ringworm of this country, which is confirmed by the occurrence of the achorion in it, as lately stated by M. Raciborski. I have stated that I have seen large oval spores (achorion) in T. tonsurans. I have been as brief as possible in my remarks, perhaps not explicit. I hope to have shown, if not the certainty, at least the probability, that parasitic diseases are mutually convertible; that differences of seat, soil, moisture, heat, and the like, account for the differences in observed appearances; that there is nothing in the microscopic history of the diseases which establishes any essential divisions between the different varieties; that their clinical history confirms this view; that T. favosa probably can be produced from T. tonsurans, the latter from herpes circinatus, and *vice versa*, (case of mother and grandmother quoted;) that herpes circinatus may give rise to sycosis. (Mr. Hutchinson has shown that chloasma may be produced from T. tonsurans;) that chloasma may be also produced from the implantation of the oïdium; again, that sycosis may be produced from T. tonsurans; again, T. favosa and sycosis from the yeast-plant, (Lowe;) and last, I believe I have succeeded in saccharine solutions in producing the oïdium from the torula. The fact of finding the *sarcinæ* and *torula* in the instance before mentioned must not be forgotten in appreciating the question of the identity of parasitic disease. The nail fungus, according to Virchow, is an aspergillus; and, according to Kückenmeister, an aspergillus or oïdium. Meissner makes it achorion. The ear fungus is said by Robin, to be aspergillus: a mucor by Slayter. These statements indicate some close relation between the different fungi.

Such are the facts I have at command at present. Do they justify the inference I have drawn? The demand for fuller explanation cannot be satisfied in this paper. Nothing but facts have been dealt with, and the material, if meagre, is none the less suggestive for future inquiry.

To conclude—

1. *Tinea* (the generic term for parasitic affections of the surface), which is disease of the hair, and not an eruptive one, must be regarded as essentially and primarily caused by the growth of a fungus, since the characterstic effects (upon the hairs) are never produced without such growth.

2. There exists but one parasite common alike to several so-called distinct kinds of tinea.

3. The variations are mostly in the *external* character of the tineæ—in the superadded rather than in the essential conditions of the disease; for the parasitic growth varies but little, and that only in *degree*, not in *kind*.

4. The superadded concomitant states (especially eruption, seat, and the like), by their variation, fully account for the observed differences in physical and minute appearances.

5. A certain soil is requisite for the growth of the tinea vegetation; and that furnished by the non-specific eruptive diathesis is the *necessary one*.

6. The treatment consists of general measures to correct the soil; and of local measures to destroy the parasite.

It is not improbable that future experience will show that parasitic growths of the mucous membrane are derived from the same source as those of the surface, the difference of habitat, &c., fully accounting for the varied results. They are in this latter situation correctly, practically regarded, *per se*, as of little moment beyond the indication that the type of the accompanying ailment is adynamic, and that the condition of soil is the disease demanding attention, (there is not present any structure like the hair of the scalp upon which they may produce perceptible and serious result.)

It would be far from desirable to alter the nomenclature now in use if it were correct to do so, since the treatment varies according to the aspect the tinea bears, and it is as well to have some mark to indicate the same.

Any one of the ordinary skin diseases may become complicated by the growth of a parasite which attacks the hairs of the part, however small and few they be; and the affection is then a complicated one—a parasitic eczema or parasitic herpes, &c., &c., being set up. Hence the distinction of skin diseases into parasitic and non-parasitic is not altogether an unimportant one; for, in the former class, general treatment is resisted, or ineffectual, perhaps, on account of the presence of a local cause of irritation which requires direct treatment.—*Lancet, Sept.* 17, 1859, *p.* 283.

84.—*Glycerine Ointment for the Itch.*—M. **BOURGUIGNON**, so well known in Paris by his successful researches on "the acarus scabiei," has published in the Gazette Médicale the following formula. One general friction, not preceded by soap ablutions, is sufficient:—Yelks of two eggs; essence of lavender, lemon, and mint, of each seventy-five drops; essence of cloves and cinnamon, of each 120 drops; gum tragacanth, half a drachm; well pounded sulphur, twenty-six drachms; glycerine, thirty-two drachms. Total weight, nearly eleven ounces. Mix the essences with the yelks of egg, add the gum tragacanth, make a good mucilage, and then add very gradually the glycerine and sulphur.

Many cures have been obtained by this preparation, which has the advantage of giving no pain.

The well-known Helmeric ointment being really useful, M. Bourguignon has modified it, and substituted glycerine for the axunge. In the altered form the preparation is not any dearer, as efficacious, and less painful than the original ointment. It does not grease the clothes, and has an agreeable perfume. Gum tragacanth, fifteen grains; carbonate of potash, thirteen drachms; well pounded sulphur, twenty-six drachms; glycerine, fifty-two drachms; essence of lavender, lemon, mint, cloves, and cinnamon, of each fifteen drops. Total weight, nearly eleven ounces. Make a mucilage with the gum and one ounce of glycerine, add the carbonate, mix until it is dissolved, and then gradually add the sulphur and glycerine; lastly, pour in the essences. With this compound, M. Bourguignon advises two general frictions of half an hour, within twelve hours of each other, and followed, twenty-four hours afterwards, by a simple warm bath, as the glycerine is soluble in water. Two-thirds of the preparation should be used for the first friction, and the other third for the second.—*Lancet, Aug.* 27, 1859, *p.* 218.

85.—*Pommade de Jaser for Scabies.*—According to M. **DELAHARPE**, physician to the hospital at Lansanne, of all the pomades proposed for the treatment of itch, one of the most efficacious is that of Jaser. Mons. D. has employed it for a number of years, with constant success.

The following is the formula for its preparation:

Sulphur Lotum,	16 grammes.
Zinci sulphas,	6 "
Veratrum album, (pulv.,)	4 "
Sapo niger,	32 "
Axungia porci,	64 "
Tinct. ol. Carui	1 gramme.

It will require about 250 grammes (about 8 ounces) of this pomade to effect a cure. A slight attack disappears after a single friction, preceded by a soap bath. When, however, the eruption is general, there should be two, three, or four applications made morning and evening.

In this preparation, the lard and the potassa soap modify each other, although by increasing the proportion of the soap, a risk would be incurred of making the pomade too irritating. The sulphate does not act here as a corrosive agent, as one might suppose, since it is entirely decomposed by the soap, resulting in the formation of a small quantity of sulphate of potash and sulphide of zinc. The powdered hellebore is necessarily the most active anti-psoric element, along with the tincture of caraway. Delaharpe believes that the proportions given above of the different constituents are the best adapted for the manufacture of the pomade.—*Journ. d'Anvers.*—*American Med. Monthly, June*, 1859, *p.* 459.

DISEASES OF THE EYE AND EAR.

86.—OBSTRUCTIONS OF THE LACHRYMAL PASSAGES: A NEW MODE OF TREATMENT BY DILATATION.

By Edward Charles Hulme, Esq., Assistant Surgeon to the Central London Ophthalmic Hospital, and Surgeon to the Blenheim Free Dispensary.

[At the large ophthalmic hospitals cases depending on obstruction to the lachrymal passages, are very numerous: they present themselves in various stages of development. Simple mucocele is often treated by tonics, with counter-irritation over the sac.]

Mr. Bowman's plan of probing the passages, after slitting up the canaliculus from the punctum to the caruncle, is the most scientific and rational mode of treating the strictured part; but even this necessitates continual daily attendance and much expenditure of time on the part of the patient; and however practicable and advantageous in private practice, the poor find difficulty in giving such regular attendance as is necessary to ensure success.

I believe it is generally admitted, and it is in accordance with the results of my own observations, that the greater majority of these lachrymal obstructions have their origin in chronic inflammation of the lining membrane of the nasal passages, which extends upwards into the nasal duct, and so continuously into the sac; a thickening of the mucous lining takes place, a thick, glairy secretion is poured out, which unable to escape of itself through the nasal duct below, or through the apertures of the canaliculi towards the eye, accumulates, fills, and distends the lachrymal sac, and by this distension the entrances of the canaliculi become altered in position, pressed upon by the contents, and become thickened and obstructed. This mucous secretion gradually becomes muco-purulent; it generally retains this character; and the sac can with more or less difficulty be emptied by pressure, the contents escaping either over the globe through the canaliculi, or into the nostril by the nasal duct. Independently of this common cause of obstruction, the canaliculi themselves, as shown by Mr. Bowman, may be primarily obstructed; and I have satisfied myself by numerous dissections that the canaliculi have not always that regular entrance into the sac that they are described as possessing. I believe, therefore, that in the treatment of these cases the first point of importance to attain is the free and continued patency of the canaliculus in its whole length, and also at its entrance into the sac, and then to commence the treatment of the obstruction of the nasal duct, both of which points are fulfilled by the method I proceed to describe. Having provided myself with a set of wires, made of "virgin silver" (which, by its flexible nature, is very easily bent), of various diameters, from that of a hair probe to that of one-fifteenth of an inch, I slit up the canaliculus, on a fine director, throughout its whole length, past the caruncle, on up to the sac. If the entry of the canaliculi is patent to the passage of a probe, I prefer not to carry the incision into the sac, as I think this should be avoided, if possible; but should no probe be able by the most careful manipulation to be introduced, then the incision must be carried on, and it is preferable to open the sac, when it is distended with its contents, the escape of which proves the entry of the knife into the sac. I then with a medium size wire, try and find the entry into the nasal duct. If successful in passing it directly through the nasal duct, I bend the wire over the edge of the lid, and cut it off with a pair of nippers, leaving a bend of about ¼ of an inch hanging externally over the lid. This end may be kept in its place by a piece of plaster, and it will be found to ride quite easily close in at the angle of the lids to the inner side of the caruncle, where there is hardly any motion of the lids, from the action of the orbicularis muscle. Should I not be successful in passing the medium size wire, I attempt the same plan with a finer one, which I bend in the same manner. Whichever sized wire be introduced, it should be worn for three or four days, at the end of which time I re-introduce a larger one, and continuing this gradual introduction of wires of a larger diameter, it is found that in two or three weeks the duct will take as full a sized wire as is necessary, *i.e.*, one of from one-twentieth up to one-fifteenth of an inch. Should I fail in passing any wire at all into the nasal duct, which is often so firmly blocked up as to render the passage of any probe impossible I get through the obstruction with a knife I have for the purpose, or it may be more correctly described as a fine trocar of a one-fifteenth of an inch in diameter, without a canula, and having a slight bend in it to accommodate it to the projection of the brow, and for this more or less force is required, according to the nature of the obstruction. Having withdrawn it, a full-sized wire is passed, which is also bent and cut off in the manner above described.

I have been led to adopt this mode of treatment in my attempts to try the "bent styles" described by Mr. Bowman in the Ophthalmic Hospital Reports, No. I., p. 19. The difficulty experienced in keeping the horizontal, thin portion of those styles in the tube of the canaliculus, the irritation their finer ends produced on the globe of the eye when not accurately adjusted, and the ulceration produced in that part of the canaliculus which was not slit up, *i.e.*, between the caruncle and the entrance into the sac, induced me to extend the incision and to give the style a bend outwards. I speak from the experience of fifteen cases, in no one of which has any irritation been produced sufficient to induce me to withdraw the wire, or has even any patient complained of them; after slight inconvenience on the first day, it seems to be well tolerated. In one patient, in whom, from the dense nature of the obstruction, I was obliged to use force, the relief from wearing this wire has been such, that after six months' continued use of it, she will not give it up; and, I might here add, that this patient had worn an old-fashioned style inserted through the skin, without benefit, inasmuch as the stricture was at the entrance of the canaliculi into the sac, as well as there being a dense obstruction in the duct. I think I may say that by this plan a speedy dilatation of the obstruction is attained; that the attendance of the patient is not required so frequently as when undergoing dilatation by probing: that the entrance into the sac is kept patent as well as the nasal duct; that the advantages of it over the old style inserted through the skin are very evident, and that its principle of restoring the course of the tears through their natural channel is correct. I am aware that there is one objection, but which, I think, may apply to all attempts at attaining a permanent cure of any obstruction in the mucous canals of the body, viz., the tendency to relapse. To obviate this, in a few of my more intelligent patients, I am instructing them to introduce these wires themselves after a full dilatation is obtained; and so, by occasionally wearing them, to keep up the patency of the passages. Anyhow, it must be granted, that if relapse should occur, the patient remains in a favourable condition for a re-introduction of the wires, or for the practice of probing.—*Med. Times and Gazette, May* 21, 1859, *p.* 522.

87.—DISEASE OF THE LACHRYMAL DUCT.

(Under the care of H. Walton, Esq., Central London Ophthalmic Hospital.)

Any swelling of the mucous membrane of the lachrymal duct is, in consequence of the unyielding bony canal around, sure to be attended with more or less interruption to the escape of the tears into the nose and to the natural mucous secretion, and to produce the well-known symptoms of obstructed duct. If we except chronic inflammation of the conjunctiva, commonly known as granular eyelid, it would be difficult to name an affection more troublesome to treat than the above. Every few years some new suggestion or plan is brought out, and invariably it is supposed for a time to be the thing needed, each in turn giving place to the other.

But we will proceed with the characteristics of Mr. Walton's treatment. He employs constitutional measures, we may say almost always, because he says in nearly every instance is there unmistakeable evidence of general indisposition and want of general power. He calls to his aid also mechanical means in the great majority of the cases that come to him, because they are in an advanced state, and need it. Rarely does a hospital patient seek relief till the disease is of long duration, and the same may be said of private patients, who seldom go to the ophthalmic surgeon in the first instance.

When a patient applies with acute inflammation of the duct, manifested in swelling and redness of the parts at the corner of the eye, and more or less œdema of the eyelids, Mr. Walton does not attend to the external symptoms merely, but at once directs his attention to the cause of them, and makes an incision into the upper part of the duct, and pushes the narrow knife through any resistance that may exist; and if there be marked obstruction, he passes a probe of sufficient size, and allows it to remain for an hour or two. This is more in accordance with the principles and practice of modern surgery, whereby the products of inflammation are allowed to escape, than merely to leech, and to apply topical applications, but it has the greater advantage in being the more potent plan. as Mr. Walton finds that with general treatment of a tonic nature a cure often ensues.

The last case we saw treated at the hospital was of this nature. A young man had for several weeks been troubled with irritation and unhealthiness of the duct, when an acute attack supervened. There was so much swelling that the inner edge of the orbit could not be felt, and withal, excessive tenderness. The use of the knife gave exit to pus, and vitiated mucous secretion. As there seemed to be little if any obstruction to the passage of the instrument, the probe was used. This is the most favourable kind of case, and there is abundant evidence of the disease being often arrested here. Not so, however, when it gets a stage further, when frequent acute attacks have produced structural changes, or thickening of the duct has been the result of long-continued subacute morbid action. According to Mr. Walton's observation. a tolerably correct idea of the obstruction of the duct may be gathered from the condition of the periosteum and the tissues immediately around, in the vicinity of the head of the duct. So long as there is no thickening, and the outline of the bones can be distinctly made out, there is no need for mechanical treatment; the unhealthiness of the mucous membrane is amenable to general measures, with local leeching and blistering, but in proportion as there is thickening of the parts and induration, so is there a corresponding change throughout the duct, and the treatment by dilatation becomes necessary, and a cure is the less certain.—*Med. Circular, Sep.* 7, 1859, *p.* 117.

88.—CORNEAL FISTULA CURED BY CAUTERIZATION WITH COUNTER PUNCTURE.

(Case under the care of Mr. DIXON, Moorfields Ophthalmic Hospital.)

An elderly woman, who had been operated on for cataract (by extraction) two years before, was admitted a few weeks ago under Mr. Dixon's care at the Ophthalmic Hospital, with the complaint that her eye was failing her. Only one eye (the left) had originally been operated on, and she had obtained very good sight from it. It was only within a few weeks that her power of vision had been failing. On carefully looking at the eye Mr. Dixon discovered that near the middle of the cicatrix, which was rather broad, was a minute fistula, through which the aqueous humour slowly drained away. The anterior chamber remained nearly as full as usual, and the place of the iris was vertical, the process of resecretion supplying the loss by the fistula. The latter might, indeed, have been easily overlooked. It was only by absorbing the moisture adjacent to it by blotting-paper, and protecting it from the tears from above, that the welling up of aqueous humour through it could be positively demonstrated. Mr. Dixon remarked that he had no doubt that this fistula had opened only recently. If it had remained from the time of the operation, the patient would not have had such good sight as she had enjoyed until quite latterly. His experience of corneal fistulæ was, that however completely the resecretion of fluid might keep pace with the loss, yet sight was always rendered very imperfect. Probably, in this instance, the cicatrix had from the first been thin and stretched, and now, from some accidental cause, it had given way at its thinnest spot. To cure the fistula was plainly the object of treatment, and its almost invisible minuteness rendered this no easy matter. Mr. Dixon at first tried touching it with a fine probe coated with nitrate of silver. This was done twice; and on the second occasion for a few days it was thought to have been successful; but ultimately, the little channel again opened itself. On the third occasion, Mr. Dixon made a counter puncture at the lower part of the cornea, and let out the aqueous fluid, having just before again applied the caustic to the orifice. The eye was kept quiet by strips of plaster. It was hoped that during the time the cornea was flaccid, from the removal of the contents of the anterior chamber, union might ensue. The result justified this expectation; and when we last saw the patient, the fistula appeared soundly healed, and the woman's sight was improving.—*Med. Times and Gazette, August* 27, 1859, *p.* 213.

89.—CASES OF PTOSIS AT THE ROYAL LONDON OPHTHALMIC HOSPITAL.

One congenital case was cured (the pupil uncovered) by removing an oval piece of skin from the upper lid, and bringing together the wound with sutures.

Another case, caused by accumulation of fat behind the orbicularis, was cured by removing most of the fat and a small oval slip of skin.

A third case, J. T., aged 42 (patient of Mr. Bowman), had four times been operated on for ptosis, by removal of portions of skin; both upper lids hang straight down from the orbital edge and cover the upper third of either pupil; the palpebral apertures appear small, and the outer lateral palpebral ligaments are much lengthened.

29th of March, 1859. With a needle the two ends of a thread, were at a distance of about four lines from each other, thrust through the outer angle of the upper fibro-cartilage, near the outer lateral ligament, carried out through the skin, and brought together over a small piece of sticking plaster. The upper lids were thus stretched and dragged out and upwards, and the whole of the pupil became visible.

12th of April. The lids cover again the pupils, though the threads had several times been shortened. Mr. Bowman, then, by a new operation, displaced the lower edge of the orbicularis of both upper lids. A narrow oval slip of skin was first removed, so as to expose the lower margin of the orbicularis; the thin skin was with a cataract knife separated from the anterior surface of the lower third of the orbicularis. A very fine silk thread was introduced through the skin beneath and close to the orbital edge, above the outer and inner angles of the oval skin incision; and between the skin and the orbicularis, carried to the lower margin of the latter; there it was stitched through the margin, so as to enclose some of the muscular fibres, and was then brought between the fibro-cartilage and the posterior surface of the orbicularis and carried back through the skin at a spot which was about four lines distant from the point of introduction. Each orbicularis was thus suspended in two loops of silk; both ends of the silk were then tied over a small roll of plaster, to an extent sufficient to raise the lid and the orbicularis behind the detached skin; the orbicularis would thus, it is supposed, undergo adhesion, and stand higher in reference to the margin of the lid, than it did originally.

A good deal of swelling and catarrhal ophthalmia followed the operation, but subsided after a week, when the threads were removed.

19th of April. Both pupils are uncovered, the skin of both upper lids is thrown into folds.—*Ophthalmic Hospital Reports, July* 1859, *p.* 111.

90.—*Cases of Nævus of the Eyelid, at the Moorfields Ophthalmic Hospital.*—Three nevi, two in the skin of the right upper, the other in that of the left lower lid, were treated by drawing floss silk, steeped in perchloride of iron, through them. The following inflammation was slight; the threads were removed the third day. One of the nevi was cured within a month, one in three weeks, and a small remaining portion of the third was removed by ligature.

A hazel-nut-sized nevus in the thickness of the right lower lid (first observed as a small red point seven days after birth), in a child, five months old, was treated by dividing subcutaneously the tissue of the nevus, and then injecting a few drops of pure tannic acid, which rendered the nevus hard; a few drops of blood escaped, and a bandage was applied; no bleeding ensued. The next day, the child had diarrhœa and fever; the former stopped the following day, the latter increased, and the child died on the fourth day with convulsions. The parents refused a post-mortem examination.—*Ibid, p.* 113.

91.—CANCEROUS ULCERATION CLOSE TO THE INNER CANTHUS, TREATED WITH SULPHATE OF ZINC PASTE.

By Dr. William Mackenzie, Glasgow.

[The cancerous ulcer in this case was situated on the side of the nose, close to the inner angle of the left eye. It was fully the size of a fourpenny-piece, of irregular shape, covered with a scab, and surrounded by hard and elevated edges. The disease seemed to be extending in the direction of the carunculæ lacrymales.]

I felt no doubt that were the ulcer left to itself, it would gradually spread, and prove rebellious to all applications of a soothing description. Having repeatedly found, also, that extirpation of such a disease by the knife, even when care was taken to cut into the sound integuments, although followed by a firm cicatrice and an apparent cure, was succeeded after a time, by a renewal of the scirrhous hardness and intractable ulceration, I determined in this case, to try as an escharotic the sulphate of zinc, as recommended by Dr. Simpson of Edinburgh. I was partly led to this course, too, by observing that I could not extirpate the diseased part without removing the lower papilla lacrymalis.

Having driven off by heat the water of crystallization of a few grains of the sulphate of zinc, and reduced the residuum to a fine powder, I mixed it with a little glycerine, so as to form a thick tenacious paste. Taking a little of this on the point of a bit of stick, I applied it over the scab and over the hard edges of the ulcer, and covered the part with dry lint.

Next day I found that the application had given very little uneasiness; but that it had acted in destroying almost entirely the hard edges of the sore, and left the whole of its surface free from scab, and of a florid, healthy colour.

Two or three times subsequently I covered the edges again with the zinc paste; after which I left the wound to cicatrize, under the application of dry lint. I touched the conjunctiva and papillæ lacrymales occasionally with the four grains' solution of nitrate of silver, under which application they speedily freed themselves of inflammation.

On the 24th March, the patient called on me, with a firm cicatrice, of a healthy hue, in the site of his former disease.—*Opthalmic Hosp. Reports, April*, 1859, *p*. 5.

92.—*Strabismus*.—[The plan of applying a suture to the incision in the conjunctiva is a good one, but is generally neglected by most operators.]

Mr. Walton makes a very small opening in the conjunctiva within about a line of the margin of the cornea; he then introduces the hook, sweeping it partly round the globe of the eye, and, having caught the muscle, draws it to the opening, and divides it at the side of the hook next the cornea; then with a small curved needle he passes a fine silk thread through the edges of the incision, and brings them together, tying the ligature in an ordinary knot.

It is surprising how soon the wound heals when treated in this way, and it is not liable to have those troublesome granulations spring up in the corner of the eye, which so often retard the cure, when the wound in the conjunctiva is left gaping, and when made nearer to the inner canthus.—*Medical Circular, Sept*. 7, 1859, *p*. 117.

93.—ON THE STRUCTURE AND MOVEMENTS OF THE IRIS.

By Dr. Letheby.

[The following results are from investigations pursued some years since, for the purpose of ascertaining whether the movements of the iris are due to muscular contraction or to vascular turgescence.]

1st. A microscopic examination of the iris failed to discover the presence of muscular fibre, except at the outer or ciliary boundary; for the orbicular and radiating fibres had none of the appearances of muscular tissue. That which composed the circular margin of the pupil had the properties and structure of ligamentous tissue; and the rest of the iris was chiefly composed of capillary vessels.

2nd. When the iris was subjected to the stimulus of galvanism, which is so powerful an agent in exciting muscular contraction, there was no movement of it after circulation had ceased. This is very unlike the behaviour of muscular tissue; for in all other cases the circulation of blood is not essential to muscular contraction; in fact, the removal of muscular fibre from the body, and therefore the cutting off of all vascular connection, has no influence on its contractility when it is subjected to such a stimulus. But if the eye of an animal is removed from the orbit and galvanized, the iris shows no sign of contraction, whilst the recti muscles are strongly affected. So again, a few minutes after death the iris ceases to contract, for generally this is sufficient time for the arrest of the circulation in the eye, and then no movement can be produced in it.

3rd. Again, if the iris be made up of two sets of muscular fibres—the radiating and circular, intimately blended together, how is it that in the living animal, one set only, namely, the circular, contracts under the influence of a stimulus? or conversely, why is it that belladonna, and the active principles of the solaneæ generally, act only on the same set? It may be answered, that the circular only are muscular, and the radiating elastic; but of this we require proof.

4th. And why, too, is it that in every condition of the system, when the vessels of the head are gorged, as in apoplexy, the pupil is contracted, and in opposite states, as during syncope and epilepsy, the pupil is dilated? This points to the fact that vascular turgescence has something to do with the phenomena.

5th. The chemical reactions of the iris are more like those of elastic tissue than muscular fibre; for if the iris is enclosed between two pieces of glass, together with fragments of muscular and ligamentous tissue, and then boiled in water, the former like the latter is so changed that the structure is obliterated, and gelatine is produced from it; but nothing of the kind occurs with muscular fibre.

6th. If the contraction of the pupil is due to circular fibres, it is manifest that when there is a radial slit in the iris, the contraction of it should cause the sides of the slit to be drawn asunder, but experiments on animals show no such a result; on the contrary, when the iris is cut through the pupil contracts as usual, and the edges of the slit are brought closer together.

7th. If we turn to comparative anatomy for help in the inquiry, it will be found that the pupillary aperture in the eyes of some animals is reduced in size, not by a circular contraction of the pupil, but by the falling down or pushing up of a curtain. This is well seen in the eye of the skate, and it is proof that the movement is not due to muscular contraction, but to the unfolding of the curtain-like membrane by some other cause. Again the irides of most fish, and of many reptiles, are immoveable, although they have the same structure as those of mammals, excepting the vascularity. So also in tracing the development of the iris from the lowest class of animals to the highest, it is found to be nothing more than a prolongation of the vascular choroid.

Lastly. Injections of the iris show it to be very vascular, and to be made up almost entirely of an erectile plexus. Nay more, if we inject the eye with water from the carotid, we can effect a contraction of the pupil with every force of the syringe; and this may be done long after death, when muscular irritability has entirely ceased.

My conclusions, therefore, are that the movements of the iris in mammals are due to vascular turgescence, and not to muscular contraction, indeed I might point to the fact that the existence of muscular fibre in the outer or ciliary margin of the iris, may afford an explanation of the phenomena, for if there were muscular pressure on the returning capillaries of the iris the tissue would become gorged with blood and the pupil contracted; but of this I had not sufficient proof because of the difficulties of the inquiry.—*Ophthalmic Hosp. Reports, April,* 1859, *p.* 17.

94.—ON SYPHILITIC INFLAMMATIONS OF THE EYE.

By Jonathan Hutchinson, Esq.

[Having detailed no less than 64 interesting cases of inflammations of the eye of a specific nature, the author proceeds to analyse them. He divides the task into three parts: firstly, the natural history of the disease hitherto known as strumous corneitis; secondly, he endeavours to show that it is a direct consequence of hereditary syphilis, and occurs almost solely in the children of parents one or both of whom have suffered from venereal disease: lastly, the treatment, which should be more directed against the specific nature of the disease, than is usually the case.]

1. *Age.*—It would appear that the greatest proportion of cases of this form of keratitis occurs in patients between the ages of 10 and 15. Thus we find that in nine instances the disease began before the age of 5, in eighteen between 5 and 10, in twenty-five between 10 and 15; in nine between 15 and 20; and in the remaining two between 20 and 25; the average age for the whole sixty-four being 10. It would appear to be comparatively rare in early childhood, and still more so subsequently to the full establishment of puberty. I have never seen it commence in any one beyond the age of 25. With regard to several cases in the series in which it is stated to have begun in infancy, I feel some doubt as to the correctness of the history, as I did not see the patients until some years after the commencement of the disease. No doubt the eyes were inflamed at the date assigned by the mothers of the patients, but whether the affection was from the first interstitial keratitis is open to some question. I have never myself witnessed its occurrence earlier than the age of two years.

2. *Sex.*—It would appear that girls are more liable to this disease than boys. Thus in forty-one of the cases before us the patients were females, and in twenty-three males, a ratio of one of the latter to 1·8 of the former.

This coincides with what I have ascertained respecting the acute iritis of syphilitic infants. Of the latter disease I am in possession of the particulars of sixteen cases in which the sex is specified, and of them twelve were female infants, and only four males.

3. *State of health at the time of outbreak.*—In none of the preceding cases is it stated that the outbreak of keratitis had occurred during recovery from small-pox or any other exanthem, nor is there any note of other causes of ill health supposed to have acted as predisponents. In none of the cases was there any reason to suppose the patient to be the subject of phthisis or any other tuberculous affection, and amongst the coincidents "conspicuous by their absence" supposing the affection to be "strumous," is enlargement of the lymphatic glands. In only two cases did any affection of the cervical glands exist. On the other hand, the series presents very few exceptions indeed to the following statements: *a. That the patients were of peculiar pallor.* In most the complexion was of a pale leaden or sallow hue, without a vestige of colour, and in none of the exceptional cases was there any degree of the excessive floridness so commonly seen in the subjects of glandular struma. *b. That the skin generally, and that of the face especially, was thick, coarse, and flabby.* This condition is intended to be comprised whenever the term "syphilitic physiognomy" is used. I have never employed the latter expression except to denote a striking and remarkable condition, such as would, from its peculiarity, have arrested the attention of the most cursory observer. *c. That the bridge of the nose was wide and depressed.* This state also is included wherever the above terms have been used. *d. That in the skin of the face there were numerous small pits and scars, and about the angles of the mouth the radiating scars of former ulcerations.* The common non-specific eruptions of childhood, impetigo, porrigo, and eczema, leave no perceptible scars, whilst their syphilitic congeners almost invariably do so. Small-pox, chicken-pox, and herpes, undoubtedly do cause pits and scars which are undistinguishable from those of the syphilides. The conjunction of fissures at the oral angles with pits in the face, a history of no one of these three affections being obtainable, is, however, very suspicious. *e. That, in those who had cut their permanent set, the condition of the incisor teeth was very peculiar, both in form, colour and size.* As diagnostic of hereditary syphilis, various peculiarities are often presented by the others, especially the canines, but *the upper central incisors are the test teeth.* When first cut these teeth are usually short, narrow from side to side at their edges, and very thin. After awhile a crescentic portion from their edge breaks away, leaving a broad, shallow, vertical notch, which is permanent for some years, but between twenty and thirty usually becomes obliterated by the premature wearing down of the teeth. The two teeth often converge, and sometimes they stand widely apart. In certain instances in which the notching is either wholly absent or but slightly marked, there is still a peculiar colour, and a narrow squareness of form, which are easily recognised by the practised eye. In a considerable number of the cases cited no mention is made of the teeth, the notes having been taken before I was aware of the value of their structure as a symptom. Since I have made it a rule always to look into the mouth, however, I have not met with a single example of well characterized interstitial keratitis in which the teeth were of normal size and shape. Indeed there can be no doubt whatever as to the truth of the assertion that malformed upper incisors (permanent set) are all but invariably coincident with this disease. A few months' observation at any large Ophthalmic Institution will satisfy any one of this clinical fact.

The following special affections were coincident with the keratitic disease. Large scars in the soft palate and pharynx in six instances. Deafness consequent on otorrhœa in eight. Nodes in six (tibia four, radius two). Psoriasis on the face in four. Destruction of the nose by erosive lupus in three. Pains in the bones in three. Suppurated glands in the neck in two. Laryngeal disease in two. Tinea tarsi in two. Swelling of knee-joints in two. Inverted eyelids in one. Lacrymal abscess in one. Cellular abscess in one Exfoliation of alveolus of upper jaw in two.

4. *Previous history, more especially as regards infancy.*—In thirty-one of the cases, or rather more than half, a clear history of the occurrence of symptoms of inherited syphilis in infancy (rash, sore mouth, ulcers at anus, prolonged snuffles, &c.) was obtained. This number would probably have been much increased, but that in many cases I was unable to see the patient's mother, or any one who could answer questions on this head. In several, which I have not included in it, there was a history of one or more very suspicious symptoms; the group, however, was not sufficiently complete to allow of a confident opinion. In eleven instances the mothers admitted that others of their children had also in infancy suffered from similar symptoms. Many of the cases in which I was unable to obtain a history or to make inquiry as to infantile symptoms, are those in which physiognomy, teeth, &c., were most typically characteristic. The proportionate frequency of otorrhœa, ulceration of the palate, &c., has already been stated.

5. *History of syphilis in parents.*—Those who have engaged in similar inquiries will feel no surprise at the fact that in nineteen cases only did I obtain from the parents a free admission that one or both had, prior to the birth of the child, suffered from venereal disease in a constitutional form. Of these, in twelve instances, the mother had been infected by her husband, and both were consequently diseased; in four the father was known to have had the disease, whilst the mother averred that she had never suffered; in one the mother had had syphilis before marriage, and believed her husband to be healthy; and in the remaining one there was a statement (probably untrue) about the communication of the disease direct to the infant by a tainted nurse. In thirty cases, or nearly half, I either had no opportunity of asking questions on this score of either parent, or did not avail myself of it. In five instances syphilitic symptoms existed at the time that the notes were taken in one or other parent, and this number includes some in which, notwithstanding, all history of primary disease was denied. In two cases, in which I could obtain no confession, the mother admitted that other medical men who had attended her children in infancy had asked the same questions as myself.

6. *Estimate of viability of the patient's family.*—Very important information may be reflected upon many of the questions connected with inherited tendencies to particular forms of disease by data as to the mortality which has prevailed amongst the brothers and sisters of the subjects of them. If the rate of juvenile mortality has been excessive, there is ground for believing that the taint is of a kind which materially diminishes the vital power, and predisposes to attacks of fatal diseases.

Fifty-three mothers of subjects of interstitial keratitis (the latter being at the time of inquiry of the average age of nine and a half) had borne families averaging seven in number, but which had been reduced by death to an average of 3·3. Fifty-three mothers had borne a total of 371 children, and of these only 179 remained alive. In other terms, fifty-three patients suffering from interstitial keratitis pass before us, and we find, on asking, that, taking one with another, they have all lost in early life more than half of their brothers and sisters. There can be little doubt, despite the many fallacies to which statistics expose us, that this rate of mortality is very high.

7. *Position of the patient amongst his brothers and sisters.*—We have seen that the 53 families give us a total of 179 living children, an average of 3·3 to each family. Now, supposing the 179 children put together and a group of 53 drafted from them without any selection, this group ought to contain, of the eldest of their respective families, a proportion of only 1 in every 3·3. The subjoined tabular statement will show how different is the result in the cases before us, and will demonstrate that interstitial keratitis in choosing out its victims, has some principle which guides its selection.

Of the 53 cases the patient was,

The Eldest	in 38	instances, a proportion of	1 in 1·4
Second	in 8	,, ,,	1 in 6
Third	in 5	,, ,,	1 in 10
Fourth	in 1	,, ,,	1 in 53
Fifth	in 1	,, ,,	1 in 53

Thus it would appear, that not only does the disease select the eldest in the large majority of instances, but that it proceeds downwards by the same rule, preferring the second to the third, and so on. This statement of fact, strong as it is, would be yet further strengthened if two sources of fallacy could be removed: 1st, that in certain instances, the primary disease had probably been contracted by the parent after the birth of part of his family, and that thus the patient, although not the eldest of the whole, was the eldest of those born subsequent to the event alluded to.—2nd, that in two instances in the series, both the eldest and the second child suffered from keratitis, and are included in the table.

Why the first-born should suffer most often, and most severely, from a disease consequent upon syphilis in the parent, we can easily understand; it is in keeping with all that we know respecting the transmission of that disease. On the "strumous" hypothesis, however, to the exclusion of inherited syphilis, I think I may fairly challenge any one to offer a shadow of explanation of the remarkable facts just adduced.

8. *Phenomena of the attack.*—The phenomena of interstitial keratitis have been well described by several authors. I have given a brief *resumé* of them in vol. i. page 232, and need not here attempt any lengthy description. The series under consideration, however, supplies us with several cases, in which less usual, and hitherto but little noticed, conditions were presented. The cases in fact divide themselves into four groups, according as one or other of the special symptoms of inflammation were apparent.

Group A includes the more common cases, in which interstitial deposit, without any great degree of sclerotic or superficial vascularity, is the prominent symptom.

Group B comprises those cases in which, in addition to the interstitial deposits of lymph, crescentic fringes of capillaries are seen spreading from the circumference over the surface of the cornea. These fringes usually commence at the lowest part, but subsequently encroach from all parts of the cornea, and often nearly, or altogether, meet in the centre. In the latter event, I have seen produced a remarkably vivid colouring of the whole surface. The degree of lacrymation and of intolerance of light, present in any given case, will usually be found proportionate to the extent of these fringes.

Group C is illustrated by three or four cases only in the present series. In it there is a large effusion of lymph, in all probability from the posterior surface of the cornea, moulding itself in the concavity of the latter, and causing complete blindness. Hitherto I have never seen the superficial vascularity characteristic of group B, coexistent with this state of things. After this form of disease, I suspect that the eyes are always more or less damaged permanently.

Group D has for its characteristic the punctuate effusion of lymph, in circumscribed dots on the posterior layer of the cornea. This condition is often seen in iritis, consequent on acquired syphilis; it also constitutes a most characteristic feature of what is known as aqua-capsulitis, as distinct from interstitial keratitis. In a few cases of the latter affection, however, it occurs as the first stage, to be followed sooner or later by more anterior effusions into the substance of the cornea itself.

Although I have mentioned these varieties as distinct groups, yet it must be understood that they not unfrequently stand in the relation of stages one to the other. The more severe conditions included in groups B and C are, for instance, rarely produced, without either those of A or D having preceded them.

The occurrence of iritis, as a complication in cases of interstitial keratitis, although not unfrequent, is, I believe, far from being usual. The obscuration of the cornea is commonly so quickly produced, that it becomes impossible to inspect the state of the iris, after the first week or two of the attack. During that period I have very rarely indeed been able to detect any evidence of the iris being affected. On recovery, the pupil is usually quite round and mobile, though not unfrequently the iris structure itself has lost some of its lustre, and looks dull and leaden. I have very rarely indeed seen the pupil occluded. When iritis does occur, it is usually of very slight severity, and attended with but little tendency to effusion.

In most cases interstitial keratitis affects both eyes, and with almost equal intensity. Both were involved in fifty-six of the sixty-four cases under consideration; in five the left alone, and in three the right alone. Out of twenty-five, in which the notes inform us as to which was first attacked, we find the left to have been so in fifteen instances, and the right in ten. Of twenty-six, in which like information is given, as to which was most severely affected, it is the left in seventeen, and the right in nine. It would seem, therefore, that the left eye is the most often attacked alone, is most often the first to be affected, and usually suffers the most severely.

As in most other symmetrical diseases, it is rare that the two organs are attacked quite simultaneously. The second is, I believe, usually affected from a few days to a few weeks subsequently to the first. Now and then, however, the interval is much longer. Thus is Case 43, a period of two years intervened, and in Case 31, one of four months. Case 26 is interesting as an instance of acute relapse in one eye, two years after the beginning of the attack, and when both had seemed to be nearly recovered. Several other instances of relapses, more or less acute, are scattered through the series, but they are decidedly exceptional. Ordinarily, when once the process of clearing has set in, it is remarkable how steadily it advances.

I will now place in concise juxtaposition the chief reasons which induce me to regard interstitial keratitis as a direct result of inherited syphilis:

1st. From its being a very well-marked and peculiar form of disease, it is *à priori* probable that it acknowledges some single and definite cause.

2nd. Its subjects are almost invariably of very peculiar physiognomy, and usually bear the most marked similarity to one another.

3rd. Its subjects almost invariably have their upper central incisor-teeth of the permanent set dwarfed and notched in a peculiar and characteristic manner.

4th. In most cases the features alluded to under the last two heads bear no resemblance whatever to those of "struma" properly so called. The subjects of true struma, on the contrary, usually have large white teeth, and are often of florid complexions.

5th. I have not yet seen a single case in which the patient was the subject of phthisis, and very few in which enlargement of the glands of the neck had occurred.

6th. It affects by preference the eldest living child of the family, a circumstance to be expected under the syphilitic hypothesis, but wholly inexplicable under that of struma.

7th. It affects female children in preference to males, and occurs in families in which a large infantile mortality has usually occurred.

8th. It occurs in all classes of the community, the well-fed and under-fed, and the residents in the most healthy situations (sea-side, &c.) as well as those of crowded cities.

9th. In nineteen out of thirty-one cases in which I was able to make inquiries on the subject, I obtained a confession that one or other parent had suffered from constitutional syphilis prior to the birth of the patient.

10th. In the thirty-two instances out of thirty-eight in which I obtained information as to the health of the patient during early childhood, a clear history of the usual symptoms of infantile syphilis was given.

11th. In eleven instances there was a clear history of symptoms of infantile syphilis having been observed in brothers or sisters of the patient.

12th. Whilst, as above observed, enlargements of the lymphatic glands (two cases) are unusual, other affections far more closely connected with syphilis than with true struma, such as nodes (six cases), ulceration of the palate (six cases), and erosive lupus (three cases), are not infrequent in the subjects of this disease.

9. *Treatment.*—The treatment which I usually adopt consists of the cautious use of mercurials and iodides, at the same time supporting the system

by tonics and a liberal diet. The mild mercurial ointment rubbed in behind the ears, in the neck, or under the axillæ, every night at bedtime, is the best mode of employing that agent, and one which in these cases I never omit. A mixture containing iodide of potassium, iodide of iron, and tincture of nux vomica is also usually prescribed at the same time. If the patient be very feeble and if the case be one belonging to group B, that is, with much superficial vascularity, more direct tonics, such as quinine and the pyrophosphite of iron, are indicated. The induction of ptyalism ought certainly to be avoided. Although in one instance I witnessed most rapid improvement coincident with its occurrence (Case 12), yet I feel sure that it is unwise to run the risk of so much reducing the patient's strength. Unless, indeed, the surgeon is certain that his patient is well fed and well protected from cold, the utmost caution ought to be used in ordering mercury. In case 48 a rapidly induced ptyalism in a half-starved feeble girl certainly did harm. If the intolerance of light be great the occasional employment of blisters behind the ears may do good, but some of the worst cases I have seen had become so in spite of setons which had been introduced into the temples. To cases 14, 31, and 49, I would appeal in proof of the superior efficiency of a combined tonic and specific plan of treatment over a merely tonic one. I had at first intended to attempt a detailed comparison of the two modes, but on further examination of my data am reluctantly induced to content myself for the present with the above observation.—*Ophthalmic Hospital Reports, July* 1859, *p.* 94.

95.—ON EXTRACTION BY LINEAR INCISION.

By Benjamin Bell, Esq., and Dr. Patrick Heron Watson.

[The word Linear is used here in contradistinction to the flap, or ordinary operation of extraction, which involves nearly one half of the cornea. The operation might be called extraction through a small section.]

The chief advantages of the operation seem to be these two:—1. It affords an easy and expeditious mode of getting rid of soft cataracts, to which alone it is applicable, for it is well known, that when these are treated in the ordinary way, by being broken up with a view to solution, they are often dissolved very slowly, and not without ultimate injury to the visual powers of the eye. Ophthalmic sugeons are familiar with the observation, that the process of solution is not unfrequently followed by a marked impairment of vision, as if the deeper and more important textures had, somehow or other, been interfered with in their nutrition, by the prolonged and exhausting effort to dissolve and remove the opaque lens. But by removing the cataract at once, as in the operation by *linear incision*, the organ is spared this effort at solution, and the likelihood of vision being restored is greatly increased. And even if a portion merely of the cataract escapes through the opening in the cornea, the remainder dissolves and disappears more rapidly and with less injury to the eye, than if the whole lens had been allowed to remain in the chambers of the aqueous humour, after being broken up by the needle. 2. The smallness of the incision of the cornea—which, however, the cataracts being soft, is amply sufficient—renders the operation both less dangerous and more easily performed, than the ordinary method of extraction for cataracts of hard and firm textures. Such cataracts always require an ample section of the cornea, and, under ordinary circumstances, and in suitable cases, no other operative procedure, notwithstanding the difficulty of its performance, can bear a comparison with it.

The operation by linear incision may be performed in the following manner. But, first of all, is chloroform to be administered? There seems to be less objection to its use in this operation than in ordinary extraction with a large division of the cornea; because, in the event of sickness and vomiting being induced, the risk of injury to the eye is obviously smaller. Moreover, unless the self-possession and steadiness of the patient can be relied on, there will be considerable advantage from inducing anæsthesia, if the scoop should require to be frequently employed in removing broken portions of the lens from the anterior chamber. Preliminaries being over, the incision of the cornea is made by means of a triangular-shaped knife, sharp at the point, keen on both edges, and about two and a half or three lines broad at its base. It should enter the cornea near its outer margin, and pass horizontally in front of the iris, until the whole length of the cutting edges has penetrated the anterior chamber. The knife is then withdrawn, care being taken that as little as possible of the aqueous humour be allowed to escape. The next stage of the operation consists in passing a fine cutting needle into the opening, freely dividing the capsule, breaking up the substance of the lens, and bringing the fragments into the anterior chamber. Most of the latter will, in all probability, be carried out by the aqueous humour, as it gushes through the wound of the cornea; but, if this should not be the case, we withdraw the needle, and with a small silver scoop, made for the purpose, endeavour carefully and gently to effect the same object. But, as we have already hinted, there is no serious objection to our allowing small fragments of the cataract, which is supposed to be of soft texture, to remain in the anterior chamber; for the wound in the cornea speedily heals, the aqueous humour is re-secreted, and the remaining portions will rapidly disappear. It is better far to trust to this course of events, than to give way to the *nimia diligentia chirurgorum*, which, by bruising the edges of the wound, might prevent it from healing, and perhaps occasion serious inflammation of the iris with all its consequences.

Some prefer breaking up the cataract, in the first place, with a very fine cutting needle, so accurately made as not to let out the aqueous humour, and then making the incision of the cornea in the manner already described. The objections to this are of a practical nature; the extreme difficulty of withdrawing the needle without allowing a little of the aqueous humour to escape, and then the ulterior difficulty, of cutting in a satisfactory manner with the triangular knife the imperfectly distended cornea. To obviate this latter difficulty, the second part of the operation might be postponed until the following day, when the aqueous humour would be re-secreted; but then there might be some risk of inflammatory action being kindled by the portions of lens which had been brought into the anterior chamber. No general rule need be laid down. The principle of the operation being kept in view, the details may be modified in individual cases, according to the judgment of the surgeon.—*Edinb. Med. Journal, Sept.*, 1859, *p.* 250.

96.—ON THE USE OF FORCEPS IN EXTRACTION OF CATARACT.

By Mr. France.

[There is, perhaps, no operation more variable in facility of execution in different cases, than extraction of cataract. The great point in the operation is, that first performed—the section of the transparent cornea.]

What creates the main obstacle to the ready accomplishment of the section in any given case, and renders arduous, perhaps, in a plump model globe, that which may be found comparatively easy in an ill-formed, deeply-set one, should the condition referred to not exist? Undoubtedly the principal impediment the surgeon has to encounter, is the irregular mobility of the eye, with its restless twitchings and sudden glancings under the excitement and apprehension of the eventful moment; together with its occasional rapid inversion towards the inner canthus (perhaps without previous warning from unsteadiness, and notwithstanding the opposing finger's pressure) when the section is in actual progress, and when this movement imperils to the utmost its normal completion. Hence this operation, in the same hands, varies to an extreme in difficulty, owing to the differing degrees in which these movements prevail in the organ submitted to manipulation. Accordingly, in the dead-house the process is easy enough: probably under chloroform, were its exhibition admissible, (which, on distinct grounds, is not the case,) it would be the same. So, again—to take a parallel illustration:—a thousand sempstresses in London thread their fine needles fifty times a day, with scarcely a conscious effort, because, minute as is the orifice they aim at, they have the implement at perfect command; but let the work-woman try to attain her end, while the needle is held by her aged mother with paralysis agitans, or by her sister with chorea, and the delicate task would become nearly impracticable. Just in the same way with this operation; the great desideratum is steadiness and fixity of globe, because any irregular or convulsive movement, by jeopardizing the precision and continuity of the section, the retention of the aqueous humour to the last, and (contingently thereupon) the safety of the iris, enhances so seriously, despite the highest skill, the danger of this critical stage.

I have elsewhere shown how early, in the history of extraction, this truth was recognized; how various have been the contrivances which the exigency has at different times called forth: and how all have successively become obsolete, on account of some prevailing disadvantage attending them. On the same occasion I recommended the employment of simple artery forceps, as conducing in a marked measure to fix the eye steadily in the wished-for position, and as exempt from any of the drawbacks which had caused previous suggestions to fail. The recommendation was supported by the detail of twenty cases, proving the practicability and utility of the plan proposed. Winter has since intervened, and I have consequently but seven additional cases to supplement the former list. In all of these, however, the forceps was used with as good effect as in the preceding instances; the fixed cornea afforded no uniform resistance to the knife; the section was accomplished with proportionate ease without casuality (save, in one example, wherein, from the extreme prominence of the globe, some vitreous escape was predicted, and took place accordingly), and the patients are now, without exception, enjoying good vision.

My present purpose is to confirm what was before said in advocacy of this appliance; and to notice two or three minutiæ, which should be observed in adopting it: as, it is to be hoped, many, during the ensuing season, may be induced to do. In order not to exceed due limits in this communication, I will condense my remarks into the briefest compass:

1. As to the form of forceps: an ordinary pair of artery forceps, without spring-catch, is the most convenient. It should be rather long, with broad nibs, and prominent or projecting teeth, so as the better to take hold of the subconjunctival fascia simultaneously with the mucous membrane itself.

2. With this view, at the commencement of the operation, the surgeon, in affixing the forceps, should make pressure with its nibs against the sclerotic, at some little distance beneath the cornea, 'ere closing them with the fold of tissues in their grasp. This is an essential point, as the lax conjunctiva, if seized alone, affords a much less firm anchorage to the globe.

3. The assistant to whom the forceps is delivered, should be instructed to rest his hand quietly upon the patient's face, and maintain the cornea in the central position as passively as possible,—by gentle traction if requested; but scrupulously to avoid either dragging or pushing with the instrument.

4. The operator, raising the upper lid with his forefinger, places that finger in the usual manner on the sclerotic above the cornea, and his middle finger on the sclerotic at the inner canthus, and thus completes his command of the globe.

It will be observed that this method of operating does not involve the *substitution* of one means of fixing the eye for another, but comprises the recognized and general plan, *plus* the effectual auxiliary I am describing. It will be observed, too, that the forceps, while maintaining the globe in the most convenient position, performs incidentally the subordinate function of keeping the lower lid depressed.

5. The incision of the cornea is made in the regular way; but when coun-

ter-punctuation is fully effected, just before the final completion of the flap, the forceps should be gently disengaged. Should there be evidence of spasm, an interval may be here interposed before the last bridle of cornea is severed, as the adequacy of the section and the safety of the iris are now insured; but this is seldom necessary. The remainder of the operation proceeds in the customary mode.

Two objections have been advanced against the use of forceps. The more important is, that it might tend to excite spasm, and consequent rupture of the hyaloid membrane, and escape of vitreous humour. The other, that it must inflict an injury on the conjunctiva, which might prove seriously detrimental to the ultimate issue of the case. With regard to the first: as a matter of fact, it will not be found that spasmodic action is created by the instrument, except, perhaps, at the first moments of its application; when every nervous patient's eye, open it how we will, is apt to be thus affected from alarm. Moreover, if the forceps is detached before the final completion of the section. as it always should be, it is the surgeon's option, should spasm actually arise, to pause for its subsidence before proceeding; and until he proceeds to divide the remaining corneal isthmus, no mischief can be produced by the occurrence.—The second objection is a very natural one, but entirely at variance with daily experience. I use the forceps in keratonyxis as well as extraction: it is pretty generally employed in London in cases of artificial pupil; sometimes in removing extraneous bodies from the eye; and, in short, on every occasion (extraction hitherto excepted), when it is desirable to fix the globe; yet mischievous results from the contusion of the conjunctiva are quite unknown.

I will add no more than the expression of my earnest hope, that the use of forceps, in extraction of cataract, may be submitted extensively and impartially during the approaching summer to the ordeal of practice. It will then be found that, by this simple means, the section of the cornea may be accomplished with a precision and certainty unattainable before; and the habitual employment of forceps will be recognized as a signal improvement and safeguard in this operation; making it in expert hands still surer—in those of the less skilful or less experienced comparatively sure.—*Oph. Hosp. Reports, April,* 1859, *p,* 21.

97.—NIGHT BLINDNESS IN CONNEXION WITH SCURVY.

By Dr. Alexander Bryson.

[On carefully examining the eyes in this affection, no alteration whatever can be perceived in the tissues anterior to the retina. It seems fair, therefore, to suppose that the disease lies in the retina, or else in the nerves and brain behind it. Further, as this curious affection rapidly disappears when proper diet has been obtained, it is evident that the changes are not of a permanent or organic nature.]

Night-blindness, most unquestionably, occurs much more frequently in scurvy than is generally supposed, but in consequence of the simultaneous existence of some more serious symptom, or of symptoms of a less ambiguous character, it frequently passes unnoticed. In fact, in the naval service, patients are generally placed on the sick list before it occurs, or at all events before it becomes so decided as to induce the patient to complain of it. Still it is almost certain that scurvy occasionally shows itself without any concurrent defect of vision, while there are other instances in which nearly every second case is accompanied by it.

The following is a remarkable instance of the prevalence of the disease, in connexion with scurvy. The crew of her Majesty's brigantine "Griffon," employed on the west coast of Africa, for the prevention of the slave-trade, had been victualled on salt rations for five months consecutively, when several of the men began to complain that they could not see to do their duty on deck at night, though the moon shone brightly at the time. Amongst themselves it was called moon-blindness, consequently it did not cause much alarm, until out of about fifty white men twenty-two were affected, and immediately after the sun went down, they had to be led about on the upper deck, in a helpless state of blindness. There was now just cause for alarm, as the vessel, with so many men unfit for night duty, was hardly a match for any of the well-armed slavers so common on the coast at that period. Fortunately a man presented himself, complaining of a sore, and a swelling on the calf of one of his legs, the true nature of which there was no mistaking; on examining his gums, they were found to be swollen and spongy. He was one of those affected with night-blindness; the other men similarly affected were next examined; as well as those whose vision by night was still good, when it was ascertained that all the former presented the most unequivocal symptoms of scurvy, and a few of the latter had spongy gums. The officers, five in number, were not attacked either with scurvy or night-blindness, but they had a more varied diet than the men. To obtain fresh meat, fruit, and vegetables, the vessel anchored at Prince's Island, and three days afterwards nearly the whole of the hemeralopic patients had entirely recovered their vision.

The ship's company of the "Dolphin," another African cruiser, had, in the year 1851, been a long time without obtaining any supplies of fresh meat or vegetable, consequently scorbutic symptoms made their appearance, and at the same time there were ten cases of night-blindness. The medical officer mentions, that nearly all these men were able to distinguish objects at the distance of three yards by candle-light, and even to read a book held close to the candle, but the instant they went on the upper deck, they gradually began to lose sight of surrounding objects, and had to be led about from place to place. A few could plainly distinguish the stars and the top-gallant masts and yards, but below that plane they could not see anything. Two cases occurred in the "Dart" while employed in the Mozambique Channel, for the cure of which blisters and various other means were used; one improved, but the other resisted various modes of treatment, until the vessel anchored in Simon's Bay, and fresh provisions were issued to the crew, when the patient, to the astonishment of the assistant-surgeon, suddenly got well.

Scurvy made its appearance in several vessels in the Black Sea in the year 1854, and, with hardly an exception, it was accompanied with night-blindness. In the "Vengeance," eleven men were put on the sick-list for scurvy, but many others were suffering in a less degree. Eight cases of night-blindness occurred at the same time; some of these patients showed scorbutic symptoms, while others were apparently free from them. None of the officers were affected with either of these diseases, which were simultaneously removed by a better diet.

In 1855 there were upwards of seventy cases of scurvy in the "Euryalus," but two only of night-blindness were noticed. Her Majesty's ship, "Brisk," was seventy-two days in making a passage round Cape Horn to San Francisco, consequently during all that time no fresh meat or vegetables could be procured. Before she reached the above port, the surgeon examined the men, and found twelve with scorbutic symptoms; seven cases of night-blindness had occurred during the passage, and four of these were the worst cases of scurvy. They all recovered immediately after they had obtained fresh meat and vegetables.

One of the most remarkable instances of scorbutic night-blindness occurred in the "Winchester," while she was returning from India. Twenty cases were entered on the sick-list between Trincomalee and the Cape of Good Hope, but they all recovered when the vessel anchored at Simon's Town, and fresh provisions were issued to the crew; on the passage between the Cape and England, however, when the men had been for some time victualled on salt provisions, the disease reäppeared in the same persons, and continued until they had obtained fresh provisions at Spithead.

In convict-ships proceeding to Australia, both scurvy and night-blindness have frequently made their appearance, but the latter often escapes notice in consequence of the prisoners being sent down into prison either at, or a little after sunset. In the "Marquis of Hastings," which conveyed prisoners to Hobart Town in 1841, many cases of scurvy occurred, and there were ten of night-blindness, which presented no other symptoms of scorbutic disease.

In all these, and in many other instances of a similar nature, the appearance of the disease simultaneously with scurvy, and its sudden disappearance when a better diet is obtained; the absence of the disease in vessels in which the men have a sufficiency of fresh meat and vegetables, even though employed in the same localities with others in which it has made its appearance, shows that it is entirely dependent on an improper or erroneous diet.—*Ophthalmic Hosp. Reports, July* 1859, *p.* 40.

98.—*Acute Rheumatic Ophthalmia: recovery under Morphia Treatment.* (Case under the care of J. Z. LAURENCE, Esq., South London Ophthalmic Hospital.)—[The patient was a middle-aged woman, admitted into the hospital, suffering from a severe attack of acute rheumatic ophthalmia. The sclerotic was intensely injected, the sclerotic zone being well marked. The pain was intense, but the vision not materially impaired.]

She was ordered to foment the eye frequently with warm water, and to take a quarter of a grain of hydrochlorate of morphia every third hour.

November 6th. She took the morphia regularly up to 4 P. M. yesterday, when she took the last powder. Towards the evening of the 4th, the pain in the eye began to abate: now, the severe shooting pains have entirely left her; she experiencing only an aching in the eye when it is exposed to the light. The sclerotic vascularity has considerably diminished.

The eye now rapidly recovered under the treatment of an ordinary case of conjunctivitis.

Mr. Laurence has now treated a number of cases of inflammation of the sclerotic and iris by morphia, without the use of mercury or blood-letting (usually employed in these cases). He finds that the majority yield to this very simple (morphia) treatment, especially those in which intense pain is a prominent symptom; but that a certain number of cases remain, in which the old plan of treatment is the better to pursue.—*British Med. Journal, July* 9, 1859, *p.* 538.

99.—ON THE SYMPTOMS, DIAGNOSIS, AND SURGICAL TREATMENT OF GLAUCOMA.

By J. W. HULKE, Esq., Assistant-Surgeon to King's College Hospital, and to the Royal London Ophthalmic Hospital.

[At p. 265 of vol. xxxvii. will be found an article on this subject by Mr. Hulke. He fears that the adoption of Graefe's operation of iridectomy in glaucoma is not so much employed and valued as it ought to be; much difference of opinion prevailing as to its value.]

The extreme differences of opinion respecting the value of iridectomy in glaucoma, expressed by those who have personally tried the operation, have no doubt arisen from the indefinite nature of the cases which have been classed under this denomination, a greenish colour of the pupil having been taken as the distinctive character. The value of any operation in a given class of cases can only be correctly estimated when the character of the cases has been previously fixed and determined; and this has been done for glaucoma by Dr. A. von Graefe, who has so fully and accurately defined its symptoms, that he has left little to subsequent observers but to confirm his statements. He, in common with all surgeons, recognises two forms of this disease—the chronic and the acute—which differ in degree but not in kind; between these extremes there is a large proportion of intermediate cases.

Symptoms, Premonitory Period.—In seventy-five per cent. or more, both of chronic and acute cases, the more obvious outbreak is preceded by a premonitory period (prodroma), which may be extended over a space of several months, or weeks, or be limited to a few days only; the symptoms are in some cases so slight as hardly to excite attention, and this especially amongst the lower classes, who pay but little heed to trifling ailments. These premonitory symptoms are occasional dimness, with pain in the eyeball, and sometimes headache. They are at first transient, often happen towards evening, and occur at long intervals. Afterwards accessions become more and more frequent, till, at length, the dimness is persistent and the pain chronic; but still with remissions and exacerbations. The subconjunctival veins begin to enlarge and become varicose; the pupil is rather dilated and sluggish, and the globe feels hard. Meanwhile the field of vision has undergone a remarkable contraction, progressing from the periphery towards the centre. Patients still see objects placed directly before them fairly; but cannot see things which lie on one side.

Active Stage of Chronic Glaucoma.—In chronic glaucoma the premonitory merges gradually into the active period. The ciliary region is congested, the radicles of the varicose subconjunctival veins encircle the cornea with arches. The aqueous humour becomes serous, the pupil muddy, and adhesions form between it and the lens. The size of the anterior chamber diminishes, the iris being pushed forwards towards the cornea partly by the lens, and partly by the collection of serum behind it. The cornea loses its lustre, resembles a piece of glass which has been breathed on; its epithelial layer is raised in minute vesicles, and its sensibility much impaired. The sclerotic has a dirty leaden and staphylomata form, generally just behind the insertions of the tendons of the recti muscles. The globe is now very hard, and sight is limited to bare perception of light, or wholly abolished. We must be careful not to be misled on this last point, because luminous spectra often happen long after all sight is lost, and we may be unintentionally deceived by patients reluctant to believe themselves blind.

Acute Glaucoma.—The acute differs from the chronic form mainly by its terrible intensity; it is preceded by the same train of premonitory symptoms; but the outbreak is often so sudden and so violent that the earlier warnings are forgotten in the present storm. The attack often takes place at night. The patient wakes with violent throbbing pain in the eye, intolerable headache and sickness, and rapid blindness ensues. The pupil is widely dilated, fixed, and the globe very red. Patients have not unfrequently ascribed their blindness to a violent attack of bilious headache. After some hours, perhaps, a remission of the symptoms occurs; the pain abates, sight partially returns, and the patient is full of joy at his improvement; but his hopes are soon shattered by a fresh paroxysm. It was only a temporary improvement, and fresh accessions sooner or later bring total blindness.

Ophthalmoscopic Signs.—The ophthalmoscopic signs of glaucoma are, excavation of the entrance of the optic nerve, the presence of visible pulsation in the retinal vessels, and often hemorrhagic extravasations into the retinal tissue and vitreous humour. Excavation of the entrance of the optic nerve is indicated by a peculiar tint, and by a very remarkable arrangement of the vessels which are abruptly bent, or seem suddenly to disappear at the margin beyond which they cannot be followed to the centre of the entrance; this is particularly the case with the veins. When the tension of the globe is much increased a visible pulsation in the retinal vessels takes place, and if not present spontaneously a very slight pressure upon the globe with the finger will produce it instantly. The excavation of the optic nerve comes on gradually with the increasing tension of the globe. This part offers less resistance than the tougher sclerotic, and yielding before the excessive intraocular pressure is bulged outwards; a wasting of the nervous fibrils accompanies this important change. From this it will appear that in glaucoma the excavation of the optic nerve is due to two causes, one mechanical, the other vital, pressure and wasting,—the second consequent on the first. Now it has been very recently denied that this change in the shape of the entrance of the optic nerve in glaucoma is ever due to pressure, it being alleged that a degree of pressure which could push the entrance of the optic nerve outwards must necessarily also push the lens and iris forwards, and that extreme excavation of the optic nerve has been observed in cases in which excessive tension does not exist, and in which there is no history of its ever having been present, in which also the anterior chamber preserves its natural dimensions. Such cases do occasionally occur, they were actually described by Graefe in a Memoir on Glaucoma addressed to the French Institute, 1857, and in the "Archiv für Ophthalmologie," Bd. iii. Abth. ii., under the designation of "Amaurosis with excavation of the optic nerve." The morbid anatomy of these cases has been investigated by Heinrich Mueller, and I have myself dissected the hollowed optic nerve-entrances of eyes which certainly at the time of their removal, during life, were not over-tense, and in which over-pressure had probably never been present. In these cases the tubules of the optic nerve have more or less completely disappeared; the surface of the hollow is formed by the membrana limitans (hyaloid membrane) resting upon the connective tissue of the lamina cribrosa and that of the framework, derived from the inner layer of the sheath of the nerve, which separates and supports the bundles of nerve-tubes and which is normally continued upwards as a delicate nucleated fibrous tissue, even through the lamina cribosa to the hyaloid membrane. This, then, is excavation of the entrance of the optic nerve from wasting and disappearance of its tubules, a change which has occurred without the agency of pressure, but it does not legitimately follow from this that the hollowing out of the entrance of the optic nerve in glaucoma, in which similar atrophy also occurs, is also independent of pressure. Wasting of a tissue may be induced in several ways, and constant pressure is a most efficient cause before which the hardest and softest tissues alike disappear.

But over-tension is the leading feature of glaucoma, and it seems only natural to consider the hollowing and wasting of the optic nerve as one of its effects. To these signs, excavation of the optic nerve-entrance, and visible pulsation of the retinal vessels, a third is often added, especially in acute glaucoma. Minute ecchymoses, points of capillary hemorrhage appear in the retinal tissues by the rupture of the over-distended capillaries. Often the membrana limitans gives way, and small filmy bloodclots form in the vitreous humour, which becomes tinged with the hæmatine, and in specimens which I have dissected the stain extended to the lens, which had a yellow colour by transmitted light though during life it was greenish. Later in the disease the vitreous humour becomes clouded, and the lens and cornea opaque, and the ophthalmoscope can no longer be used.

Prognosis.—The prognosis is allowed by all to be a most unfavourable one. Blindness is the natural termination, and the pain often persists long after the sight has been entirely lost,—pain sometimes too of the most wearing kind.

Treatment.—Every candid person must confess that all known medical treatment fails to cure glaucoma, because it fails permanently to relieve the over-tension of the globe. General antiphlogistic measures are inapplicable, because the subjects are feeble, and local depletion by cupping or leeches affords only very slight and very transient relief. Those who trust to measures of this kind let their patients drift into blindness. But practice has put it beyond doubt that the removal of a portion of the iris of an over-tense eye does not permanently lessen the excessive tension, and, therefore, does cure glaucoma. The fact is not to be denied though a satisfactory explanation of its *modus operandi* may be still wanting. For the details of the operation itself. I must refer to a paper in Medical Times and Gazette (Retrospect, vol. xxxvii., p. 265). I have nothing more to add to what I then said, except that I believe an ordinary Beer's extraction knife to be a safer instrument for incising the cornea, than the lanced-shaped knife of Jaeger, with which, as its point is directed inwards towards the centre of the anterior chamber, and therefore towards the vertex of the lens, there is greater danger of wounding this structure. In about two hundred operations performed with Beer's knife, which in making the corneal wound, only skirts the margin of the anterior chamber, the lens has, I believe, only been injured in three or four cases. In two cases the suspensory ligament gave way, vitreous humour was lost, hemorrhage into the globe took place, and the eyes suppurated. The immediate effect of the operation is to lessen the tension of the globe; the wound heals in a couple of days, during which the aqueous humour drains away, but when union is quite complete, and the integrity of the anterior chamber is restored, the globe still continues naturally soft The most complete results are obtained in acute cases, when the improvement of sight is very rapid and striking, and the cessation of pain equally so when the operation has been early performed. In chronic cases the improvement of the sight is gradual, extending over some months; its degree depends on the progress which the structural changes in the retina and optic nerve had made previous to the operation, and in these cases they advance *pari passu* with the growing pressure; but even where the sight is not improved, the operation generally arrests the disease and preserves what sight the patient had at the time of operation. In chronic glaucoma, too, the pain does not always cease at once, but sometimes subsides gradually, just as in tic douloureux it frequently persists for several days after division of the affected nerve.

Alleged Objections.—Those who oppose iridectomy in glaucoma do so on the alleged grounds (1) of its futility; (2) of the danger to be apprehended from operating on an eye in a state of intense congestion; (3) of the difficulty of so delicate an operation, for which they would substitute paracentesis of the cornea or sclerotica. These objections are of small value, because they are not founded on practice, from which alone true inferences can be drawn. I will take them seriatim.

1. Facts abundantly prove, that iridectomy does cure this disease. I have been told that patients who had been operated on in the Royal London Ophthalmic Hospital have been afterwards seen at other Hospitals perfectly blind; and, indeed, it would be surprising if this were not the case, because the operation has been frequently done simply to relieve pain at a time when no restoration of sight could be looked for and no expectation had been held out.

In a few exceptional, very advanced cases of chronic glaucoma, the overtension of the globe has returned after the operation, and the pain has not been relieved. In these cases the tissues of the globe are so unsound, its vascular system is so completely deranged, and the excitement of the ciliary branches of the fifth nerve has become so habitual that no measure short of extirpation will afford complete and permanent relief.

2. Practice shows that iridectomy is the most efficient measure with which we are at present acquainted for reducing the intense internal congestion, which is present in acute glaucoma.

3. The difficulties of the operation are not insurmountable; they may be overcome with a little care. They were pointed out in the paper to which I have already referred, and directions were, at the same time, given by which they may be avoided. Less extensive removal of the iris is insufficient; its whole breadth must be taken away so as to expose the edge of the lens and the suspensory ligament. Paracentesis of the cornea affords but an incomplete and transient relief; and it is futile to tap the sclerotic, because the vitreous humour is too firm to flow out, even through a considerable wound.

Lastly. It has been and still is urged that the operation has been very indiscriminately performed. . This allegation does not affect the operation itself, but insinuates a want of discrimination on the part of the operator. I would state it in another way—the operation has been experimentally tried in a number of cases, which though not true glaucoma, showed an affinity with it; or where there were local signs of a relative excess of pressure, as in staphylomata of the sclerotic and cornea; and lastly in a few cases of acute phthsis bulbi, where the rapid wasting of the vitreous humour shown by the softness and collapse of the globe indicated a sudden arrest of nutrition.

The results confirmatory of Graefe's statements have proved, that in recurrent iritis and choroido-iritis, where there are numerous synechiæ, the tendency to fresh accessions is lessened and destroyed by iridectomy; that considerable staphylomata subside under its influence; and that soft and collapsing globes become plump and acquire a natural tension. Who then will assert that in these cases the operation has been indiscriminately performed? How can the value and scope of any remedy be ascertained except by a series of experiments? Without these progress would stand still and therapeutics become a stationary art.—*Med. Times and Gazette, Aug.* 6, 1859, *p.* 127.

100.—NEW OPERATION FOR THE RELIEF OF THE PAIN IN ACUTE GLAUCOMA.

(Case under the care of H. Hancock, Esq.)

[Von Gräfe gives no theory whatever of his operation for the relief of acute glaucoma; yet we must acknowledge, notwithstanding its empiricism, that its performance is frequently followed by relief. The plan resorted to by Mr. Hancock is, however, preferable to Gräfe's for reasons mentioned in the details of the subjoined case. The patient, a boy of fifteen, two years previously, injured his eye by a blow from a knife: the sight was destroyed, but there was no apparent breach of continuity. Two months before admission, pain and swelling of the eyeball came on; at first gradually, then more rapidly, till the globe became twice the size of the other; masses of the capsule of a disorganized lens were visible. The right eye was healthy.]

On the 9th of September, Mr. Hancock introduced a Beer's cataract knife at the junction between the cornea and the sclerotic, the blade being inclined downwards, the point proceeding inwards and backwards. The place of puncture was the commencement, on the outer side, of the lower semicircumference of the cornea. The point of the knife, now having traversed obliquely the layers of the cornea, was pushed backwards toward the interior of the globe, thus dividing the ciliary ligament in a portion of its extent. On the removal of the knife, a quantity of discoloured fluid escaped.

Mr. Hancock, considering that the great pain was due to tension of the iris and ciliary ligament by accumulated fluid, the product of the disorganized parts behind, believes that all the indications for the relief of the suffering will be fulfilled by this operation. And its advantages are:—

1. By the situation and oblique direction of the incision, a free drainage of the fluid is provided for.
2. The iris is very slightly wounded.
3. The pupil is preserved, of its original size, and in its normal situation.
4. The operation is very simple, and is performed remarkably quickly.

Sept. 10th. Except for an hour or so after the operation, the patient has suffered no pain. The operation gave relief. He has slept perfectly well; before this, he has been frequently awakened by the pain induced by pressure on the eyeball from turning in bed.

11th. Eye perfectly easy during the whole day.

12th. Not the slightest pain, even on considerable pressure; the eyeball feels much less tense; the pupil is less dilated: no prolapse of iris through the wound.

21st. Has gone on uninterruptedly well since last report; there has not been the slightest pain; the size of the globe has sensibly diminished; the pupil is far less dilated than it was before the operation; and the globe, instead of being hard and resisting, now easily yields to the touch; pressure upon it occasions no pain.—*Lancet, Oct.* 29, 1859, *p.* 435.

101.—*Laudanum in Weak Vision of the Aged.*—Professor Nascar, of Naples, says, that in case of aged persons whose sight is becoming enfeebled and requires the aid of convex glasses, great advantage is derived, supposing no nervous lesion exists, from painting every evening the eyelids and brow with laudanum, and allowing this to remain on all night.—*Med. Times and Gazette, July* 9, 1859, *p.* 42.

102.—NIGHT-BLINDNESS WITH SCURVY IN THE CRIMEA.

By J. C. Ovens, Esq., (9th Regiment.)

Private John Grady, of the 9th Regiment, was admitted into the Regimental Camp Hospital before daylight on the morning of 13th February, 1855.

He stated that he formed one of the advance on the Woronzoff road on the previous night, and that he and a comrade were directed to keep a sharp look-out, which they agreed to do by turns; that, about half-past three, he fell asleep for a few minutes, and on awaking was surprised to find that he had totally lost his sight.

He was immediately taken up to camp, where he was attended to; he stated that he had no pain or ailment with the exception of the total, and to him unaccountable, loss of sight. When a candle was brought into the tent, some vision returned, but he could not see the flame distinctly, and said it seemed as if surrounded by motes. The pupils were very much dilated, more particularly the right; both acted freely; no abnormal vascularity of any of the textures of the eye was visible. The entire cornea seemed prominent, but this was said to be their normal state.

As the day-light returned, he recovered his sight, but all through the day he complained of a sort of haze and a dizziness, with constant symptoms of muscæ volitantes.

The nights at the time were very dark, and there was about four inches of snow on the ground.

There were no remarkable symptoms preceding his attack, which came on regularly as night approached. On very bright moonlight nights he could see a little, but could not discern any small or dark object, and the looking at them caused him severe pain in the eyes for rather more than six weeks. It then began gradually to wear off, but he had occasional relapses both on dark nights and when exposed to a very bright light. He was always near-sighted, and this did not seem to be increased by the attack.

This man, in common with his comrades, was, and had been for three months, enduring the greatest privations, exposed to constant night-work in the trenches; his clothes were scarcely ever dry, and his food consisted for the most part, of salt meat and biscuit. He had been suffering from that diarrhœa which in the Crimea was considered scorbutic, as it was neither true diarrhœa nor dysentery, but a state of the bowels which was induced by the bad living.

The treatment consisted in the administration of quinine, ipecacuanha to try and promote the action of the skin; blisters to the temples, with belladonna round the eye; but good living, with fresh meat and vegetables being soon afterwards procurable, these seemed to benefit the patient more than any thing else.—*Ophthalmic Hosp. Reports, July*, 1859, *p.* 37.

103.—*The Ophthalmoscope.* (From notes of Mr. Haynes Walton's practice at the Central London Ophthalmic Hospital.)—The ophthalmoscope continues to be carefully attended to, and we take this opportunity of alluding to a fact in connection with it that may be useful to our readers. As a matter of course, when the pupil has been dilated for the examination of an eye with tolerable or nearly good sight, considerable imperfection of vision ensues, and nothing is more common than for persons to declare that the use of the ophthalmoscope has been detrimental to them. This is, of course, owing to their ignorance of the subject; but the false impression may be generally evaded by forewarning the patient of the influence of the atropine, and the inconvenience may be reduced by using a very weak solution—a quarter of a grain to the ounce, so that, although the dilatation is but slowly produced as compared to the stronger formula, the effect passes away much quicker, and the adjustment of the other eye is not interfered with. Besides this, whenever the eye can be examined, as it often can, in the natural state of the pupil, this should be done, and it may be effected for all practical purposes more frequently than is supposed. Mr. Walton is in the habit of first ascertaining whether it can be effected, and not till it cannot does he dilate the pupil. We saw a case there about a fortnight ago in which incipient cataract was discovered by the use of the ophthalmoscope, after full dilatation, which could not be observed in an ordinary examination. In this case the opacity was at the circumference of the lens; and when viewed by means of the ophthalmoscope, presented a few dark spots at the margin, which gradually faded as they approached the centre, and appearing as if composed of a number of dark lines, converging toward the centre. On bringing this patient to the light, and looking carefully towards the circumference of the lens, the opacities could be seen as small white specks, that might easily escape detection with the naked eye.—*Med. Circular*, *Sept.* 7, 1859, *p.* 117.

104.—*A New Ophthalmoscope.*—An ophthalmoscope, fitted with adjusting tubes, rests for the patient's head, &c., by which the merest tyro may be enabled to see the deep structures of the eye, is now in use at the Ophthalmic Hospital. It is made by a Berlin optician, at the suggestion of M. Græfe and his assistant. Unlike the one hitherto in use, it is a large cumbrous affair, and requires to be fixed to the table or elsewhere before use. The patient's head being fixed against a rest, the telescope slides of the instrument are adjusted to a proper focus, and this once effected, a dozen observers in succession may look through the eye-piece, and all of them see exactly the same part of the retina without any trouble. It is, indeed, like looking through the tube of a microscope; the object never gets out of focus, and the proper adjustment having been effected by a skilled hand, any one can see the object. The common hand reflector and lens require a long training before they can be effectually used. For purposes of demonstration to a class, the new instrument will doubtless soon throw the other out of use, since it prevents the loss of time and risk of annoying the patient's eyes, which a succession of inspections involves. To one well trained, however, so that he can find the optic entrance, yellow spot, &c., with perfect ease, we doubt whether the new instrument will add much. Its cumbrous size will confine it to the consulting-room or public institution, but at the latter for class purposes it promises to be invaluable. To the artist, also, it is a great relief, since it leaves the hands at liberty; and to draw from the ophthalmoscope is, with its aid, just as easy as to draw from the microscope. Its cost, as at present made, is we believe, about five guineas. No doubt it will soon be to be had in the London shops.—*Med. Times and Gaz. July* 9, 1859, *p* .34.

105.—*Removal of Foreign Bodies from the Ear.*—Dr. Archibald recommends the following as a very effectual procedure:—Upon the back of a circular piece of isinglass or court-plaster, two lines in diameter, a piece of thread twelve or fifteen inches long is to be attached by a very narrow strip of the same material, placed at right angles over the thread. When this is dry, a piece of muslin or cotton cloth is to be torn, two inches long, and broad enough to be rolled around both arms of the thread into a cylinder of required size and firmness. The ends of the thread should next be drawn so as to bring the plaster upon the extremity of the cylinder, around which one end of the thread may be wrapped, to keep it from unfolding. The meatus should now be freed from moisture, by means of a little lint or cotton; and the surface of the plaster should be wetted and applied to the foreign body. Adhesion will take place in five or ten minutes, and then the body may be removed with the plaster.—*Med. Times and Gazette, Oct.* 8, 1859, *p.* 367.

MIDWIFERY,

AND THE DISEASES OF WOMEN, ETC.

106.—ON THE PHYSICAL DIAGNOSIS OF SPURIOUS PREGNANCY.

By Dr. J. Y. Simpson, Professor of Medicine and Midwifery in the University of Edinburgh.

[After touching upon Auscultation, which is merely a negative sign, and Percussion, which is of much value in many cases, especially in discrimination of cases of mere tympanitis, the author passes on to tactile examination, and observes, that there is in many of these cases a firm, unyielding swelling of the abdomen, which might be supposed to be due to enlargement of a gravid uterus, but is in reality due to a tympanitic condition of the bowels, combined with a peculiar tonic condition of the abdominal muscles, which are so firm and tense as to render it utterly impossible to make out the size and contour of the uterus.]

Chloroform will generally solve the difficulty completely, if only given deeply enough. When the patient is fairly put to sleep with chloroform, the tense abdominal muscles become perfectly relaxed, and, on pressing on the abdomen, you will find that the walls will give way before your hand, and sink backwards till you can feel the spinal column quite distinctly, and you then find the uterus to be of normal size. The phenomena presented by that phantom, tumefaction of the abdomen, while the patient is being anæsthetised, are very singular. When the patient lies down on her back, and the abdomen is uncovered, it is seen to be projecting, swollen, rounded, and defined, like the abdomen of a pregnant woman; but generally, as I have said, with an appearance of unusual constriction around the lower edge of the ribs. No change occurs during the first stage of the administration of the anæsthetic, and until the period of excitement has passed over, the swelling continues, and the muscles remain rigid and tense as at first; but gradually as that stage passes off, and the respiration offers to become sonorous, the muscles begin to be drawn in, and the abdomen slowly flattens, until it assumes its proper size, or even becomes depressed and relaxed, like the abdomen after delivery. So long as the patient remains in a deeply anæsthetic state, you can make the most complete and satisfactory examination of the state of the uterus, and, indeed, of all the abdominal organs; and you may have recourse to this expedient with perfect safety and success in doubtful cases of real pregnancy also. But when she comes out of her sleep again, in a case of spurious pregnancy, the muscles begin to arch up and to become tense as before, so that by the time the patient is fully awake the abdomen is as large and rounded as ever, and the necessary examination again becomes painful. For, as I have already hinted, the patient has sometimes in pseudocyesis a degree of tenderness in the abdomen that renders her very intolerant even of a slight amount of pressure. The patient having wakened up and found the apparent tumour still present, fails herself to be convinced of the fact that it had, for a time, been dispelled. But you may, perhaps, convince some of her friends of the absence of any real tumour, and their corroborative assertion may go far to bring her to a sound and proper belief afterwards. I had once a poor peasant's wife, from Berwickshire, with spurious pregnancy, who bothered all her friends, and kept them in a state of continued anxiety and trouble, because she was always oing into labour, until she had arrived at a period which corresponded in her reckoning with the thirteenth month of utero-gestation. She was one of those persons whom it was utterly impossible to convince by any argument of the true nature of her affection; and her great confidence in the reality of her pregnancy had imposed on her friends, and led them for long to share in her kind of monomania—for, after all, the mind is really in such a morbid state in some of these cases as to deserve the name of monomania. Having put her under the influence of chloroform, I called her sister into the room, and made her feel the spine through the collapsed abdominal walls, and succeeded thus in demonstrating to her entire satisfaction that there was no child in her sister's abdomen. But the patient waking up, and finding no change in her condition and form, might have remained unshaken in her belief, and, indeed, was still for stoutly affirming that she was pregnant, when her sister shut her up with, "Haud your tongue, woman! You've naething in your wame, for I felt your backbane myself with my ain hand!" I have no very satisfactory explanation to offer you of the nature of this very strange abdominal swelling, and of the peculiar phenomena observed in it, when the patient is in a state of anæsthesia. Some years ago I made a number of observations on some of our hospital patients, to try and solve the difficulty. Some medical friends who had been told of the remarkable effect of the chloroform were quite certain that the swelling must have been due to distension of the bowels with gas, which, they averred, must have escaped unobserved when the sphincter was relaxed during the deep sleep induced by the drug. But that this was not the proper explanation we easily proved by introducing a tube into the rectum, and putting the free end of it under water, and then finding that no bubble of air escaped during the anæsthetic subsidence of the swelling. I believe that the phenomenon most probably depends on some affection of the diaphragm, which is thrown into a state of contraction, and pushes the bowels downwards into the abdominal cavity. I am the more convinced that this is the true explanation, from the fact that you can sometimes make the abdominal swelling disappear for a second or two, by getting the patient to take a deep inspiration, and then suddenly breathe out again. But, whatever be the explanation, the value of anæsthesia as an adjuvant in aiding and establishing a correct diagnosis of such cases cannot be overrated.

[Before proceeding to the treatment of this disease, Dr. S. premises a few remarks as to its pathology. This is still involved in great obscurity. In the greater number of cases we find on local examination no trace of uterine or ovarian disease whatever; in some we find slight engorgement, a little displacement, or ulceration. Yet from the fact of nothing being discoverable in the other cases we must not regard these as essential causes, only as coincidences. So that, on the whole, the disease would seem to depend rather on some disturbance of the ordinary functions of the generative organs than on any organic disease. In the treatment of the affection our first anxiety must be to raise or restore the standard of health, which is generally impaired. Secondly, to counteract any existing uterine or ovarian disease; and, thirdly, to administer uterine and ovarian sedatives. In the bromide and iödide of potassium we have remedies likely to act as direct sedatives to the generative organs.]

I know that by the administration of these drugs, and particularly by the administration of the bromide of potassium, I have often succeeded in cutting short the disease, and in causing the train of symptoms to be suddenly broken down, after they had been in existence for a few months only, instead of allowing them to go on and annoy the patient till the completion of a period corresponding to the usual term of utero-gestation, or even for a greater length of time; and this striking effect of these remedies I am disposed to attribute to some specific sedative action exerted by them on the uterus and ovaries. And there are other facts known with regard to the use of these drugs, which go far to confirm the idea that they are possessed of such an action as I refer to. It is on this supposed action, for example, that Sir Charles Locock founds his recommendation of the bromide of potassium for the cure of epilepsy connected with menstruation. There is, as you are aware, a form of epilepsy which is liable to attack females, and more particularly young girls who have just reached the age of puberty, and who become subject to an attack of epilepsy immediately before, during, or after the recurrence of every menstrual period. The onset of the epileptic fits seems to be directly connected with some obscure kind of irritation set up at these periods in the organs of generation; and according to the experience of Sir C. Locock and others, this type of disease, which rarely yields to any other mode of treatment, may sometimes be cured by the use of bromide of potassium, which probably acts by quieting the excitement of the uterus. From these, and such like observations as to the action of the bromide of potassium on the uterus, I was first led to try the effect of it in cases of spurious pregnancy; and from what I have seen of its efficacy in these cases, I can recommend you very strongly to have recourse to its administration. Give five or six grains of it three times a day, either alone, or with the addition of from two to three grains of iodide of potassium, and I feel certain that you will often succeed in checking the progress of the morbid symptoms, and so in relieving your patient's mind of much anxiety, and in saving her much needless trouble and prolonged distress. The remedy, let me add, has this further recommendation, that it often proves a good tonic, so that its use is not contra-indicated by any constitutional debility on the part of the patient. The bromide of potassium, let me add, may also be applied locally to the uterus by having it made up in the form of a medicated pessary, and its action may then be aided or supplemented by the admixture of some other sedative, such as morphia or belladonna. Or you may employ other local sedatives. Thus I have sometimes attempted to allay uterine irritation in such cases, and more particularly where it was attended by, or amounted to, a certain degree of neuralgia, by the introduction of a stream of carbonic acid gas, or of that gas combined with the vapour of chloroform, into the vagina, in the manner I explained to you when treating of the palliative treatment of carcinoma of the cervix uteri. Sometimes, also, I have used leeches locally with a similar view. By the mere application in this way to the uterus of local sedatives you can never, perhaps, expect to produce such a powerful effect on that organ as will lead to the cure of the disease; but I believe you will often find them most useful adjuvants to the internal remedies.—*Med. Times and Gazette, Sept.* 10 *and* 17, 1859, *pp.* 250, 278.

107.—CASE OF ABORTION BETWEEN THE THIRD AND FOURTH MONTH, ENDING FATALLY FROM INTRACTABLE VOMITING AND DIARRHŒA.

By Dr. T. Herbert Barker, Bedford.

[The following case shows that even abortion may not be sufficient to save life in cases where the irritation produced by pregnancy has excited severe systemic disturbance.]

On August 28th, 1858, Mrs. H., aged 42, the mother of a large family, aborted in the fourth month of her pregnancy. She was not under my care at the time of the abortion, for she had left this neighbourhood to visit London. I learned, however, that a week before the abortion, she was seized with violent diarrhœa and vomiting, which continued unchecked up to the period when the fœtus was thrown off. The abortion had been preceded and followed by considerable hemorrhage. Recovery was never fairly established, the intestinal irritation continuing all the time. Unfortunately, she determined to return home while yet suffering from the disturbance of the alimentary system and consequent depression, and was conveyed directly from her bed-room to the railway station. The last seven miles of her journey was in an open gig, and she was exposed to heavy rain.

As soon as she reached home, eight days after the abortion, I was sent for, and found her suffering purely from gastro-intestinal symptoms. She was frequently vomiting, the vomited matter consisting of ingesta, with a considerable quantity of bile of the usual green colour. No food could be retained, and her exhaustion was extreme. But the most intractable symptoms was diarrhœa. This was persistent, in spite of every treatment. The motions were as frequent as ten or twelve in the twelve hours; and the matters excreted were copious, liquid, somewhat deeply tinged and offensive, occasionally containing shreds of mucous. She complained of but little pain, except a burning sensation in the epigastrium; and occasionally, in the cardiac region, of acute darting pains, to which she had been liable for some years. There was no tenderness on pressure of the abdomen. The pulse was 112, small and feeble. The tongue was moist, slightly furred, and red at the tip. The skin was moderately warm. Thirst was excessive.

In this case I used every variety of treatment, relying chiefly on astringents. In the list of remedies given, with every care, were catechu, kino, iron in the form of tincture and the persesquinitrate, the mineral acids, and opium in every shape. Opiate suppositories, and starch and laudanum enemata, were freely used, but without avail. Food, in any form, and medicine, appeared sometimes rather to increase than to lessen the symptoms. The vomiting was distressing, and the diarrhœa profuse, as though the entire system were being drained of its fluids. The countenance became indescribably pinched and anxious. In spite of a liberal supply of brandy, carbonate of ammonia, ether, &c., she gradually sank, and died from sheer exhaustion, precisely fourteen days after the abortion.

Remarks.—In my last reported case, we saw that abortion saved the life of the patient by stopping the gastro-intestinal mischief. In this case, although abortion occurred, a fatal result ensued from the intestinal flux and the vomiting. The case, therefore, tells rather against than for the practice of producing abortion. It is nevertheless to be borne in mind, that the patient was subjected to unfavourable conditions after the delivery; that is to say, she travelled a great many miles, and was exposed to rain, while in a condition but ill fitted to bear these risks.

The second point of interest is, that medicinal measures of the astringent type—pushed to the fullest extent and with every care—proved utterly unavailing. They neither arrested the vomiting nor checked the diarrhœa. Some of the medicines, particularly the cretaceous astringents, seemed to increase the mischief and to cause pain. Indeed, it has never been my lot to witness a case in which all medicinal and diatetic treatment was more tenaciously resisted.

If—as one of our judges recently intimated in his most extraordinary charge to the jury—inamenability to all treatment constitutes a ground of suspicion of foul play. here was a case for the gravest suspicion. Nothing can be weaker, more illogical, or more dangerous, than such a dogma.

This case differs slightly from the previous one in regard to symptoms. In the preceding case, the symptoms were of the dysenteric type. In the present they were, I should say, rather of the choleraic type. In both, nevertheless, shreds of membrane were thrown off. In the preceding case there was tenesmus; in this case that symptom was absent, the matters ejected being passed without effort or pain. In the preceding case, the patient made no complaint of burning pain in the stomach; in this case that symptom was a distressing one. These are the differences; they are minor in kind, but deserve attention. In one grand respect the cases are intimately allied; that is to say, the gastro-intestinal symptoms had their origin in uterine irritation, and in that alone.

There is every reason to believe that the connection between really serious and even fatal disturbance of the gastro-intestinal mucous membrane and pregnancy, is more common than has been suspected. Since my former case was forwarded to the Journal, several recorded cases have been referred to. In a letter by Drs. Richardson, Thudichum, and Webb, upon the case of Isabella Banks, these authors show that this connection has been so frequently observed on the other side of the Atlantic, that it has led to the announcement of this subject for prize competition. This prize was awarded to Dr. David Hutchinson, for an excellent essay, in which he has brought together many valuable facts. The connection of dysentery and pregnancy has been noted by Guillemeau, Mauriceau, Shaw, Denman, Burns, and others of our early obstetric writers. In 1848, Dr. Churchill, published the history of a case, which in some points, is like the one which occurred in my practice. A case by Mr. Edmunds was fatal after the miscarriage; the diseased action had been thoroughly set up, and the dysenteric symptoms continued in spite of everything. Another fatal case has occurred in the practice of Dr. Freeman, of Plymouth. Vomiting alone, without either diarrhœa or dysentery, has proved fatal during pregnancy. Dr. Tyler Smith has met with such cases. M. Cartaya, in an essay, *Vomissements incoercibles pendant la Grossesse*, has collected fifty-eight cases, twenty-four of which has proved fatal to the mother. Professor Stoltz, of Vienna, records eleven fatal cases, In 1855, a discussion took place in the Academy of Medicine in Paris, on the propriety of producing abortion in these extreme cases.

The resemblance of these cases of sympathetic gastro-intestinal mischief to irritant poisoning, has been so striking in some instances, as to have led to suspicion. In two cases, indeed, referred to by Dr. Tyler Smith and Dr. Richard Quain, the suspicion was so strong, that the ejecta was submitted to chemical examination. These facts remind me that two years ago a lady was repeatedly under my care, with severe attacks of sickness and diarrhœa, during the very earliest period of pregnancy. Several circumstances in this case rather puzzled me, and led me to suspect that the gastric symptoms were not solely the result of the pregnancy. This lady had never suffered from sickness during any previous pregnancy; it was not in the least restricted to the morning, but common at all times, but mostly after food; and whether at rest, or otherwise. The sickness was not accompanied with acidity in any shape; but it invariably preceded severe diarrhœa, as though the stomach rejected some offending material, a portion of which afterwards passed through the intestinal track, and caused mischief there. These attacks were perfectly amenable to treatment. A soothing mode of medicinal treatment, diet, and regimen, with quiet, always relieved; the attacks were, nevertheless, frequently repeated, and severe, producing considerable constitutional disturbance. Within the first four months and a half of pregnancy, this lady was under my care as many as seven times with attacks of sickness and diarrhœa of the character described above. After I had seen her through four attacks, the idea of irritant poisoning from some source or other certainly crossed my mind; and I mentioned my suspicion to the husband. We carefully examined the kitchen, but could not find anything improperly used for culinary purposes, and there our suspicions ceased. Every subsequent attack was regarded as a not common phase of sympathetic stomachic and intestinal affection, which would probably cease before long. This was the result; for, after the first half of pregnancy, she never had another attack.

With these facts before us, it behoves us to act with great caution where pregnancy is present; for there can be no doubt that now and then we meet with physiological and pathological effects, which are quite beyond the common beaten track of observation, and which can only be duly appreciated when the fact that pregnancy exists is itself known.

It has been urged by some professional men, that while these peculiar symptoms incident to pregnancy are possible, they are not common. I admit the fact; and I admit it on twenty years of active experience. But it strikes me, from the same experience, that cases of irritant poisoning are much less common: and I have sufficient confidence in my countrymen and women to believe that this view is correct. Any way, I am very happy that in my cases, I brought no one up to trial for a life; and I submit that medicine will soon be called Moloch, if many similar cases, because they resist remedies, are to be set up as illustrations of slow poisoning by felonious administration.—*Brit. Med. Journal, Oct.* 1, 1859, *p.* 793.

108.—HEMORRHAGE AFTER ABORTION.

By R. Jones, Esq., Strefford, Shrewsbury.

The perusal of Mr. Humphrey's case of Uterine Hemorrhage in the Journal for December 4th, induces me to relate briefly a very similar case, which occurred in my practice some years ago. The subject of it was the wife of a blacksmith residing near this village, over thirty years of age. She was a woman of good constitution; but the tone of her system had been considerably reduced by privation and anxiety, induced by the prolonged and serious illness of her husband. She had hardly completed the third month of utero-gestation, when she experienced a sudden fright, and abortion was the consequence. The ovum was reported to have been expelled; and nothing alarming happened till some days afterwards, when she suddenly jumped out of bed to assist her husband in an emergency. Profuse loss followed; and I found her in a fainting state, with copious gushes escaping every two or three minutes. A plug applied put a stop to this at once. For the next month or five weeks, the same recurred, in spite of all treatment suggested, including the mineral acids, metallic salts, vegetable styptics, opium, and ergot, with cold affusions and astringent vaginal injections. On many occasions, the timely use of the plug appeared to rescue her from impending dissolution: all other treatment was of no avail; and such was the fearful condition to which my patient was reduced, that I looked upon her case as all but hopeless. As a *dernier ressort*, I decided to try the effect of mercury. One grain of calomel and half a grain of opium was, therefore, given every six hours. The bowels were relieved occasionally by cold water injections, and active counter-irritation was applied to the sacrum. No serious hemorrhage happened afterwards, and my patient made a slow but perfect recovery.

I have adopted the same treatment, with the same success, in several obstinate cases of the same character since; but of late years I have placed much confidence in another remedy for such cases, and have seldom been disappointed by it. That remedy is oil of turpentine.

[There is nothing so much to be depended upon in these cases as the plug *well applied*. We have the bleeding almost at entire command; Mr. Jones found calomel and turpentine very effectual. We think well of turpentine, but in these cases gallic acid or tannin will be found even more efficacious. The turpentine cannot be continued week after week in hemorrhagic cases, but gallic acid or tannin can. You may give five or ten grains of gallic acid every few hours, or tannin two or four grains for a long time. We have continued one or the other of these medicines for weeks without any bad effects. The acetate of lead is sometimes more efficacious still, when all other styptics have failed. Of course we assume that the fœtus and deciduous membrane or placenta have come away.—Ed. Retrospect.]—*British Med. Journal, Dec.* 18, 1858, *p.* 1047.

109.—ON THE USE OF OXALATE OF CERIUM IN THE VOMITING OF PREGNANCY.

By Dr. J. Y. Simpson, Edinburgh.

Cerium is one of those rare and little-known metals which were first discovered in the early part of the present century, and is found chiefly in the Scandinavian mines, combined in small proportions in various minerals. I believe that any of the other preparations of the metal would fulfil the indication equally as well as the oxalate, which is used simply because it is the most easily procurable salt of cerium in the market; oxalic acid being used to separate the cerium from the metal with which it is most generally combined in nature, namely, didymium. The action of cerium on the stomach seems to be that of a sedative tonic, resembling in some degree the action of the salts of silver, and bismuth; and I have seen it succeed in curing the most obstinate cases of vomiting so much oftener, and so much more speedily than any other remedy, that I have come of late to have great faith in its employment. I would not lead you to suppose that by the administration of a quantity of oxalate of cerium you will succeed in curing every case of vomiting, or even in alleviating it in every case; but I am certain that you will find the remedy successful in a larger majority of instances than you will find in any other one drug. You may give one or two grains of it, three times a day or oftener, in the form of pill, or mixed with a few grains of gum tragacanth, in the form of a powder. The vomiting usually ceases after a few doses have been taken; but in some cases it does not abate till the remedy has been persevered with for several days. The effect is sometimes instantaneous. I had a patient some time ago from the west of Scotland, and when her husband first came to ask me to visit her I was engaged and could not go, but after hearing his account of the case, I gave him a prescription of cerium pills, which I desired him to administer to his wife till I could get to see her. He came back next morning, asking what the medicine was which I had given him, for the effect of it had been like magic. The vomiting, which had been going on almost incessantly, and which nothing seemed to have any power of alleviating, ceased upon the administration of two doses of the cerium. In a previous pregnancy in this patient it had been made a question for a medical consultation whether abortion should not be induced, to save her from the effects of uncontrollable sickness and vomiting. But the good result is, unfortunately, not always so immediate. One of the earliest cases in which I employed it was in the case of a lady who came from Greenock, when she was pregnant for the fourth time, and had arrived to between the third and fourth months of gestation. For these three or four months she had been always vomiting many times a day, and often during the night also; and that whether the stomach was empty or full. She could take but very little food, for she always sickened at the sight of it. It had been the same in all her former pregnancies; and on the occasion of the first of them the vomiting was so severe as to bring on a miscarriage, and the patient's own life was despaired of. She got, first of all, one grain of oxalate of cerium, but vomited three hours afterwards. She was then told to take a grain every three hours for a day, and afterwards one grain thrice a day. This was successful in checking the vomiting, and a few days afterwards she left Edinburgh, feeling quite well, eating her meals heartily, and free from all sickness. Everything had been tried by different medical men in the west which afforded any prospect of relief, as creosote, prussic acid, bismuth, lime-water, ice, champagne, opium, blisters, &c., but all without effect. The only thing from which she ever experienced any benefit, and that was only very transient and temporary, was calcined magnesia. Yet it required only a very few doses of oxalate of cerium to produce a perfect cure. Shortly afterwards I saw with Dr. Craig, of Ratho, a case of severe and persistent vomiting in pregnancy, where he had tried everything; but in vain. She, too, was cured by a few doses of cerium. When the propriety of entering this and other modern remedies in the new Pharmacopœia to be published by the Medical Council was lately debated, it was objected that so little of the drug is used that it is not worth while classifying it among the other recognised medicinal agents. But on making inquiry lately at the drug shop of Messrs. Duncan, Flockhart, and Co., in this city, I was told that they had sold as much as sixty-four ounces during the preceding twelve months, and I feel assured that it only requires to be more widely known to make it more extensively esteemed and employed as a general metallic sedative tonic.—*Med. Times and Gazette, Sept.* 17, 1859, *p.* 280.

110.—*On the Induction of Premature Labour by Uterine Catheterisation.* By Prof. Braun.—Professor Braun, of Vienna, states that he has for several years given a preference to this mode of inducting premature labour; inasmuch as it is very certain, operates rapidly and safely, brings on the pains with gradual energy, gives rise to no ill-consequence, such as congestion or injury of the uterus, or detachment of the placenta, and is performed by the single application of a simple instrument. One disadvantage of the procedure is, that the membranes may become somewhat easily ruptured, especially in primiparæ. In order to prevent this accidental rupture, the author softens in hot water the end of a well-oiled catgut bougie, a foot long, and from two to three lines thick, and passes it along the index finger with a twisting movement into the uterine cavity, until only a portion, equal to two fingers' breadth, remains in the vagina. The bougie so passed always excites pain in from six to twenty hours, does no injury to the membranes, and is to be removed only just before the discharge of the waters, or the birth of the child. The employment of a gum catheter, having a very thin flexible stilette, is usually also attended with good effect. Its application is difficult, however, when the vagina is narrow, and deviates from the pelvic axis. During the session 1857 and 1858, Professor Braun employed catheterisation 12 times, 11 children being born alive, and 5 dead—three being twin-births. Of the mothers 8 recovered, and four died during the puerperal process, pneumonia, tubercle, and Bright's disease, having been respectively the causes of death. The labour was terminated at an average period of twelve hours after the introduction of the catheter.—*Wien Med. Wochenschrift*, 1858, No. 46.—*Med. Times and Gazette, June* 11, 1859, *p.* 606.

111.—NEW TESTS FOR THE KIESTEINE OF PREGNANCY.

By Dr. John Braxton Hicks, Assistant Physician-Accoucheur to Guy's Hospital.

[The chief reason why the presence of kiestiene in the urine is not more frequently resorted to, as an aid to the diagnosis in doubtful cases of pregnancy, is the length of time occupied in its spontaneous separation from the urine.]

As there is no amorphous deposit thrown down from the urine previously clear, within two or three days, which is not easily dissolved by heat (lithates) or by dilute acetic acid (phosphates and carbonates), excepting from that of pregnant women, it is evidently not necessary to wait till the decomposition has gone on to form the white pellicle which ensues upon protracted exposure; but if a deposit takes place varying in colour according to that of the urine (but naturally white) unchanged by the above-named tests, then we may safely conclude that the urine contains kiesteine.

This deposit varies from copious troubling to that of small flakes falling to the bottom, and probably results from the natural acid of the urine, or the formation of lactic acid in it, coagulating the newly-altered keisteine: and the time at which the troubling takes place varies also, probably, with the rapidity with which the change by the air goes on and the condition of acidity.

If, then, we add rennet to urine of pregnancy, we shall find that in nearly every case the deposit above alluded to appears at an earlier date than if it is not employed: in some cases, within an hour (this is uncommon); in others (especially if the urine be recently passed), in from twelve to twenty-four hours. In the majority of instances, the change produced by the above-named agent has been in advance of the usual method by about half the time, and the quantity of the deposit has been decidedly greater.

Now, the greasy-looking pellicle which has always been waited for as a sure sign of kiesteine, consists of a small quantity of amorphous matter (kiesteine); occasionally a few fat-globules, but not constant; numerous crystals of the triplephosphate, amorphous carbonate, and phosphate of lime, which incrusts the numerous so-called vibriones, thereby preventing their peculiar movements till released by the addition of acetic or other acid. All these are produced by the process of decomposition, and form, therefore, but a crude test, being also somewhat imitated by the decomposition of albuminous and diabetic urine.

Rennet, I have found, has no action on healthy, diabetic, albuminous, or phosphatic urine. A slight deposit sometimes takes place, looking something like mucus; but I think urine giving off but a slight precipitate should be held doubtful in respect of kiesteine.

If, after the deposit is well formed, we add to, say, half an ounce of the turbid urine (taking the lower portions) a few drops of strong solution of ammonia, and boil for a minute or two, we shall find the deposit is formed into a semi-mucous mass, so that the urine becomes almost tremulous. When this occurs, it is, I think, characteristic of kiesteine. It can be produced without boiling, but the change is slower, and not so complete. If the deposit be scanty, the above appearance is not so well marked; but by careful watching, it may be observed amongst the flakes which are formed.

This test for kiesteine can be employed equally well with or without the previous use of rennet. The quantity of phosphates thrown down by the ammonia may be known by adding gradually acetic acid, so as to slightly acidulate. What remains undissolved is the kiesteine.

In employing rennet, I find the best method is to mix about two teaspoonfuls (as described below) with about three ounces of the urine, if it be recently passed; but if it has stood some time, and the kiestine is about to be deposited, I like to pour it quietly in, so that it may fall to the bottom. The deposit is then clearly shown at the juncture of the two fluids.

Alkaline urine should be accurately neutralized by acetic acid, and should pus be present, it should be allowed to stand, and then be filtered. (However, the rennet semi-gelatinizes pus, so that it is not of very great consequence.) This plan should be adopted in all cases where the urine is turbid, from phosphates, pus, mucus, or extraneous matter.

The chief advantages of the employment of rennet are—

1st. Saving of much time.

2nd. Increase of the deposit.

3rd. The deposit is nearly free from phosphates.

4th. It is nearly free from smell.

The rennet I use is prepared in this way:—Take the fourth stomach of a calf as soon as killed, and scour it well inside and out with salt, so as to remove the curd. Let it drain a few hours. Place it in a wide-mouthed jar, and sprinkle a handful of salt upon it. In a short time the juice will exude, and dissolve the salt. Take this and filter through bibulous paper; place it in a bottle, and use as required. That left in the jar will continue to yield a fluid for some time; otherwise some salt and a small quantity of warm water should be poured over it, and allowed to stand a day. Then filter the juice. The stomach may also be filled with salt, and sewn up, or it can be stretched on a skin to dry. In the latter cases pour warm water upon it, allowing it to stand some hours, adding salt to help to preserve it. The more concentrated the juice is the better.

Rennet, already salted, may be obtained almost of any butcher.—*Lancet, Sept.* 17, 1855, *p.* 281.

112.—*Appearance of the Yearly Ripening of Ova in Women.* By Dr. MATTEI.—Dr. Mattei regards the theory of the monthly maturation of ova concurrently with menstruation as erroneous, and believes that for each ovary only one annual ripening takes place. The months of January, February, March, and April are especially favourable for this maturation. The appearances of this condition come on at times very gently, at others very painfully. The general appearances are, alterations of the voice, sleeplessness, at times neuralgias, prostration, vomiting, frequently palpitation, cough, hoarseness, without material change in the breasts. As local symptoms, there are sensation of weight or pain in the abdomen, from the sacrum to the thighs, and especially pains in that side of the pelvis on which the lymphatic glands are swollen and tender; there is also heat and excitement of the external genitals. The menstruation is disturbed; it is seldom rendered more profuse, most frequently more scanty, coming on easier, and attended with nauaea. At times leucorrhœa, diarrhœa, dysuria, sympathetic symptoms in the breasts; the excitation of the ovarian region causes pains, nausea, even hysterical cramps; hematocele, peritonitis, and phlegmon may occur. According to the individual, these symptoms may last for four, twelve, or twenty-eight days, and disappear altogether, or pass into symptoms of pregnancy or false conception. The interval between the ripening of the ova in the two ovaries is variable. The minimum observed by the author was four days, the maximum, five months. Dr. Mattei further says that this yearly ripening mostly ceases at the same epoch as the germination of plants and the rut of animals.—*Brit. and Foreign Medico Chirurgical Review, Oct.* 1859, *p.* 553.

113—SUGGESTIONS ON THE PATHOLOGY AND TREATMENT OF PLACENTA PRÆVIA.

By Dr. Arthur S. Donkin, Newcastle-on-Tyne.

[Professor Simpson has adopted the opinion of the late Professor Hamilton in believing that the hemorrhage in placenta prævia issues from the surface of that portion of the placenta which has become detached, and not from the orifices of the exposed uterine vessels. This hypothesis he attempts to explain on our anatomical knowledge of the vascular structure of the material portion of the placenta. But this is evidently erroneous, for, as observed by Dr. Barnes in cases where only a portion of the placenta is abnormally attached to the cervix,—on the separation of this portion from the cervix, the remainder continuing attached to the fundus, the flooding is arrested. It is in fact the contraction of the womb which secures immunity from flooding, and not the mere separation of the placenta. Dr. Barnes further asks, How is the flooding stopped from that part of the uterus bared of the placenta? and answers his own question thus: Dilatation of the mouth is owing to contraction of the longitudinal fibres of the cervix, and this contraction of the cervix arrests the hemorrhage, in the same manner as flooding after normal detachment of the placenta is stopped. Thus, the first effect is bleeding; the second to stop the bleeding. The severance of the placenta is accomplished in these partial presentations in successive arcs or zones from the os upwards, until the boundary line between normal and abnormal placental implantation is reached. This zone attained, the labour is a natural labour. Dr. Donkin says:]

With regard to these propositions, advanced by Dr. Barnes, the following queries may be put:

1st. Is dilatation of the os produced by contraction of the cervix, of whose cavity it is merely the external orifice?

2nd. Does the cervical region of the uterus contain longitudinal muscular fibres, by whose contraction it is shortened or retracted?

3rd. Is the spontaneous arrest of the hemorrhage, in *placenta prævia*, accomplished by the same process which checks that following detachment of the placenta from the fundus after natural labour, namely, contraction of the muscular coat immediately around the bleeding orifices in the early stage of labour?

4th. Does detachment of the placenta, from its prævial implantation commence at the os uteri externum, and extend gradually upwards, towards the uterine body;—or, in other words, does the severance, in central cases, proceed from the centre to the periphery of the placenta?

These queries I will endeavour to answer in the negative, in making the following observations on the anatomy and physiology of the cervix uteri.

When we attentively consider the gravid uterus *at or near the term* of utero-gestation, we are struck with the analogy it bears to the bladder, which is a hollow muscular organ, permitting its contents to accumulate until it is unable to sustain a further amount of distention, and then expelling them by a vigorous contraction of its muscular walls. The only difference being, that whereas the contents of the bladder are liquid, one single contractile effort is sufficient for their expulsion. The contents of the uterus are on the other hand, solid, and meet with considerable resistance on their passage; a series, therefore of successive efforts, between each of which there is a pause, are necessary to enable the organ to empty itself. But this analogy of the *gravid uterus, at the full term, to the distended bladder, rectum. or other hollow muscular organ,* may be carried still further; for not only are its muscular fibres those of involuntary or organic life, but, like these, it is provided with a sphincter, which passively dilates or relaxes prior to and during the act of expulsion or evacuation, and immediately contracts when this has been accomplished. *This sphincter is the cervix.* The gravid uterus, then, physiologically considered, consists of two parts; the first being the body and fundus, whose office is to contract; and the second being the cervix, which passively expands during the act of parturition.

"The neck of the uterus," says M. Cruveilhier, "is composed entirely of circular fibres, which intersect each other at very acute angles." This opinion is corroborated by the researches of M. Jobert, who observes that "the uterine neck is formed of fibres which constitute semicircles, and decussate without mingling; the semicircular arrangement is more evident in women who have had children than in others." According to the same author, a portion of the muscular fibres, forming the superficial longitudinal layer on the posterior surface of the body, pass into the posterior surface of the cervix. This latter asertion, however, is at variance with the opinion of Dr. Farre, whose observations show that these longitudinal fibres are entirely absent in this portion of the uterus. We are, therefore, I think, in a position to conclude, that in the *arrangement* of its muscular tissue, *the cervix differs from the body and fundus in the absence of a superficial layer of fibres having a longitudinal course or direction,* the fibres of which it is composed having a concentric arrangement in the manner of a sphincter. But this is not the only structural difference, which the scalpel and microscope have revealed, between these two portions of the uterus. For it has been ascertained that the cervix, or rather its middle coat, is composed not only of involuntary muscular fibre, but also of fibrous tissue; the two being intimately mixed together, but the quantity of the latter being so abundant that, in the opinion of Dr. Farre, "*this might, with almost as much propriety, be called the fibrous coat of the cervix.*"

M. Jobert has likewise pointed out the important fact, that while the peritoneum is firmly and closely united to the muscular substance of the body and fundus, in the cervix, on the other hand, a considerable quantity of loose cellular tissue intervenes between these two structures. "We may establish as a law," observes M. Jobert, "that the peritoneum is connected with the proper tissue of the uterus, in woman and in animals, by muscular fibres, never by cellular tissue or by yellow fibrous tissue, and that cellular tissue, in the entire animal series, is the means of union between the peritoneum and the neck of the uterus, the vagina, and large ligaments." This lax cellular tissue, according to Dr. Farre, also intervenes between the anterior surface of the cervix and the posterior wall of the bladder, where they are in apposition, and in which locality the former is uncovered by peritoneum.

These histological facts, then, justify the conclusion, that the *mechanism* of the cervix is such as to enable it to undergo, by means of its plentiful non-muscular element, a great amount of passive dilatation or expansion during the primary stage of labour, and by means of its circularly arranged muscular structure, to contract when the uterus has emptied itself, or, in short, to perform the office of a sphincter; while its connection with the peritoneum and bladder is so loose, that its dilatation and shortening can be effected without inflicting mechanical injury on these important organs. Against this suggestion the objection will probably be advanced, that there is no definite line of demarcation between the body of the uterus and the cervix—that they become gradually blended together; but the same argument will apply with equal force against the existence of a sphincter to the bladder.

To maintain, on the other hand, in the face of the anatomical evidence which I have adduced, that dilatation of the cavity and external orifice of the cervix is produced by the muscular contraction of its walls, or that the latter contract simultaneously with each successive contraction of the body and fundus during labour, would be quite as illogical and irrational as to assume that the neck of the bladder is opened by the contraction of its sphincter, or that the latter contracts during the act of micturition. For it is well known that a muscle contracts in the *direction* of its fibres; so that, were the cervix to contract during labour, it being a circular hollow muscle, its cavity would be prevented from dilating, and it would then constitute an insurmountable obstacle to the completion of labour, and lead to grave results. Besides, no one, it may be presumed, will deny, what common sense would teach, that the cavity of a hollow organ can only be enlarged by the expansion of its walls, and not by their contraction. Now, as the cavity and orifice of the cervix *do* become enormously dilated before the pregnant uterus can be emptied, it follows, that this amplification can only be produced by expansion of its walls, and that this expansion, *during labour*, from the rapidity of its development, can only be of the passive kind, and the result of the operation of physical forces.

[In summing up his observations on the anatomy and physiology of the cervix, the author puts the following proposition:]

While the body and fundus contracts during labour, the cervix is mechanically expanded, by which process the os externum is dilated. By this passive expansion the circularly arranged fibrous and muscular tissue of the cervix is put upon the stretch, and the whole cervix considerably shortened. I therefore hold, that it is anatomically and physiologically impossible that "dilatation of the os is contraction of the cervix," as asserted by Dr. Barnes.

Having offered these suggestions on the anatomy and physiology of the cervix, I will now endeavour to explain the part which it plays in the pathology of *placenta prævia*.

The expansion of the cervix, from the fifth to the middle of the eighth month of pregnancy, does not in the majority of cases produce any severance of the placenta from its attachment to it; at all events, it is seldom that hemorrhage, the never-failing attendant on such a result, makes its appearance during this period. The reason may be accounted for by the fact that, up to the last-mentioned date at least, the placenta is rapidly growing. This increase or growth will enable it to keep pace with the slowly expanding cervix, and to mould itself to the altered shape of the latter. In those cases, again, where the hemorrhage does not make its appearance until labour commences, it is possible that placental growth and cervical expansion progress, *pari passu*, up to the full term. In those cases, on the other hand, where the hemorrhage occurs earlier than six weeks before the term, it may be presumed that the cervix expands more rapidly than is usual, and thus gives rise to placental detachment.

In most cases, however, of placental presentation about six weeks before the term, or later, the equilibrium is abolished between placental growth and cervical expansion, the former process being at or near the period of its completion, while the latter is progressing with greater rapidity and force. An antagonism is thus engendered, between the placenta on the one hand, and the cervix on the other, at the point where the latter is, for the time being, undergoing expansion, and a severance between the two in this locality is the result. By this separation, the tension of the cervix on the placenta is for a time relieved; but, as expansion continues, the same process is repeated again and again at certain intervals, until the term of gestation arrives and labour commences. In this manner the sudden and repeated attacks of flooding, during the latter weeks of pregnancy, are accounted for.

When labour is established in a prævial case, in which uterine action is frequent and vigorous, and the cervix soft and elastic, each succeeding contraction of the body and fundus of the uterus expands a "zone or arc" of the cervix, and, consequently, detaches a corresponding amount of placenta, until, after the return of a sufficient number of pains, the whole of it is completely peeled off from the cervix. And if the cervix expands, as I have endeavoured to show, not only before but during labour, from above downwards, it follows that the detachment of the placenta must follow the same course. *It must, therefore, commence at the upper portion of the cervix, and, by the agency of each succeeding uterine contraction, travel downwards.* When the placenta, then, is centrally attached, its severance will proceed from its margin towards its centre; if, on the other hand, its attachment is partial and confined to one surface of the cervix, the course of its severance will be towards its periphery. This opinion, though in opposition to the authority of Dr. Barnes and others, is in accordance with that of Dr. Ramsbotham, who, in explaining the cause of unavoidable hemorrhage prior to labour, observes, that "when five months are completed, or about that period, expansion commences; and this unfolding or developing of the fibres must necessarily produce a separation of the placenta from its previous attachment to the *upper part* of the cervix."

It may with confidence be asserted, with regard to the hemorrhage which unavoidably results from the detachment of the placenta from the cervix, that the accumulated experience of the profession has demonstrated two very important pathological facts. The *first* is, that in general the hemorrhage entirely ceases, or very considerably diminishes, in the interval between each contraction of the uterus, and breaks out afresh with every returning pain. The *second* is, that as soon as the placenta is spontaneously and entirely detached from the cervix, whether its attachment there was partial or complete, the hemorrhage is permanently arrested, and if the mother survive up to this period, the labour progresses and terminates without a return of the flooding. The doctrines of Professor Simpson and Dr. Barnes on these peculiarities of the hemorrhage I have already analysed, and will now venture to advance my own opinions on the subject, which are based on the views I have already expressed on the anatomy and physiology of the cervix. These opinions, for the sake of brevity, I will place in the form of propositions, as follows:

Proposition I.—Each vigorous contraction of the uterine body and fundus mechanically expands a portion or "arc" of the cervix; the expansion of this portion severs the placenta from its surface; hemorrhage is the immediate result, *principally from the vascular orifices, both venous and arterial, opened on the surface of the cervix*, and partly also from the orifices on the placenta. The further progress, or *ulterior stage*, of this *passive expansion*, closes the bleeding orifices on the cervix by the *mechanical compression* exerted by its stretched fibres on the outer surfaces of the utero-placental vessels, while coagulation arrests the feeble flow of blood from the placental orifices. Thus, by the time the body and fundus have ceased to contract, the bleeding is entirely arrested or greatly mitigated. The same process is repeated again and again, by every succeeding pain, until the whole cervix is freed from the placenta, when its repetition can no longer take place. This is the mechanism by which nature arrests or mitigates the hemorrhage after each pain.

Proposition II.—Flooding, after spontaneous and complete separation of the placenta from the cervix, is permanently arrested, not merely because the process of detachment is complete, but also because, when this period arrives, the cervix is expanded and shortened to such an extent, that the *utero-placental vessels* passing through its substance are flattened and rendered impervious by the pressure of its stretched tissue.

Proposition III.—Immediately after the uterus has emptied itself of its contents, the cervix changes its condition of mechanical expansion for a state of rigid contraction. This latter condition prevents hemorrhage in prævial cases after delivery, in the same manner as contraction of the fundus prevents it after the separation of the placenta in ordinary labour.

The *modus operandi* of mechanical expansion of the cervix, in compressing the utero-placental vessels, as assumed in the first two propositions, may be illustrated in the following very simple manner. If we take a short tube, or a broad ring or "hoop," the walls of which are composed of threads or fibres of elastic india-rubber, intimately interwoven with each other in a somewhat circular direction; suppose this short tube has the meshes in its walls also permeated by numerous circular canals opening on its inner surface, and having flexible walls of their own; and suppose the diameter of the cavity of this tube to be an inch, if we forcibly expand it in a circular manner, until its cavity attains a diameter of four or five inches, we shall then find, that the stretching its walls have undergone will have *squeezed* and compressed these flexible canals and closed their orifices; in other words, their cavities will have become flattened and obliterated by the mechanical pressure to which they have been subjected. Now, I hold that by this species of mechanism, which is exceedingly simple and easily understood, nature arrests the hemorrhage in placenta prævia. For I contend, it cannot be shown that expansion and stretching of the cervix will not have the effect I have described on the large utero-placental vessels passing through its substance, *to such a degree as to completely arrest the passage of blood through them, or to such an extent as to enable coagulation to produce that effect.* I therefore contend that the hemorrhage is not arrested, as is maintained by Dr. Barnes and Dr. Tyler Smith, by the "*same process which arrests the flooding after normal detachment of the placenta from its normal seat at the fundus*," namely, by "*contraction of the uterine tissue at the seat of separation*:" *First*, because I deny that the cervix *contracts during the first stage of labour*; and, *secondly*, because, if this theory were correct, the hemorrhage would have an opposite character; it would, like ordinary uterine hemorrhage, occur in the intervals of muscular relaxation, and come with the return of every period of uterine contraction.

If the explanation I have now given be correct, we are rationally led to conclude that the more fully and freely the cervix expands during each contraction of the fundus and body, and the shorter the period required to enable it to throw off the placenta, the less will be the danger to life; for not only will the flooding cease more completely after each pain, but the sooner will the period arrive when it is permanently arrested. And as the conditions under which the cervix expands during the first stage of labour, are *first*, a soft, elastic, and fully developed condition of the cervix itself, and *secondly*, vigorous and frequent uterine action, we ought to find the *co-existence* of these conditions, in any given case, the most favourable to the preservation of life. This conclusion is fully verified by an appeal to the records of experience. If, for example, we refer to Table II, of the elaborate statistics of Dr. Trask, we shall find that in 29 cases of spontaneous expulsion of the placenta before the child (these being the whole number collected from all sources up to that period, in which the result to the mother is known), the attendant hemorrhage was not fatal in a single case. In these cases uterine action was vigorous, and from the result we may safely infer that the cervix was soft and elastic. On the other hand, experience verifies the supposition, that the most dangerous cases of placenta prævia are those in which, when labour commences, the cervix is undeveloped or rigid, and uterine contraction feeble and inefficient. Whether these conditions exist singly or combined, the first stage of labour will progress tardily, and the hemorrhage will be apt to prove fatal; because each succeeding contraction of the uterine fundus and body will be just sufficient to loosen or dislodge a portion of the placenta, without producing a sufficient amount of expansion in the cervix, at the seat of separation, to close mechanically the mouths of the utero-placental vessels which have been opened on its inner surface. The hemorrhage will, therefore, be more continuous in the interval between pains; and, extending over a lengthened period, fatal syncope will be most likely to supervene, before the cervix can be sufficiently expanded to withdraw itself from the placenta and to arrest the flooding.

In concluding these observations on the pathology of placental presentation, I will venture a few additional remarks as to their bearing on the question of *treatment*.

Either *pathologically* or therapeutically considered, almost all classes of placenta prævia might with propriety be grouped into two distinct classes, as follows:—

Class I. Cases in which, at the commencement of labour, the cervix is fully developed (*i. e.*, has completed its first stage of expansion, soft, and elastic, and in which uterine action is present and efficient.)

Class II. Cases in which, in the early stage of labour, the cervix is undeveloped, rigid, or unyielding, and in which uterine action is feeble and inefficient.

In the first class of cases, either *temporising measures* to assist nature, or the operation of *turning*, will be the necessary course of treatment. The *modus operandi* of turning in such cases has been philosophically described by Dr. Rigby. "When the os uteri," he observes, "is sufficiently dilated to admit the hand there will not be much fear of the patient losing much blood during the turning, for during the first part of the operation, the hand and arm act as a compress and plug; and afterwards, when the body of the child is advancing, this will act in a similar manner. There is little danger of the hemorrhage coming on after the child is delivered, for the contraction of that part of the uterus, to which the placenta has been attached, is much greater in these cases, than it is when the placenta is situated in the upper part of the uterus under ordinary circumstances." In short, the operation itself mechanically expands the elastic cervix, and compresses the surfaces of the utero-placental vessels as well as their mouths, while, by emptying the uterus, it permits *post-partum* contraction of the cervix to take the place of expansion.

The second class of cases are, on the other hand, exactly those in which the condition of the cervix is such as would, under *ordinary circumstances*, render the performance of *podalic version* exceedingly dangerous, and a violation of the important rule entirely precluding its employment as an obstetric operation, and in which *extraordinary or special conditions* render the operation still more hazardous. The fearfully fatal result of the operation in such cases, has been too clearly and convincingly demonstrated already, especially by Professor Simpson and Dr. Barnes, to require further illustration or argument to prove its impropriety.

But in this class of cases the flooding is generally so profuse, that some method of treatment is absolutely required to mitigate or arrest it, to prevent the supervention of fatal syncope. To meet the urgency of these cases, *forcible detachment of the placenta by the finger*, either *completely*, as practised by Professor Simpson, or *only from its attachment to the cervix*, as advocated by Dr. Barnes, is recommended for our adoption; the latter method being merely a modification of the former.

From the propositions I have already advanced on the pathology of the subject, it will be observed that those cases in which the placenta is detached and the hemorrhage arrested by nature's spontaneous effort, *the separation of the placenta and the arrest of the flooding do not stand in the relation of cause and effect, but as the concomitant result of cervical expansion; both progressing pari passu. Nature then does separate the placenta completely from the cervix, until the very period arrives when she has completed the mechanical closure of the mouths of the utero-placental vessels, which have been opened in the process.* The *forcible detachment* of the placenta by the finger, *to any extent*, is, therefore, *incorrect in principle*, inasmuch as it merely tears off the placenta, without making any provision for arresting the hemorrhage, which flows from the vascular orifices opened on the cervix by the operation.

The practical objections to which artificial detachment of the placenta is liable, as proved by experience, are the following: *First*, the gross mortality in those cases in which it has been employed is equivalent to 1 in 4 6-10ths; while the gross mortality in cases of spontaneous expulsion of the placenta is only 1 in 14¼. *Secondly*, that although introduced into practice for the ostensible purpose of preventing the necessity of turning in a certain class of cases, we find that exactly *one-half* of the cases treated by this method subsequently required the operation of turning in addition. Notwithstanding these objections, the fact must not be ignored, that in a considerable number of cases a cessation of the hemorrhage has followed this practice—a result which appears in many instances to have saved the life of the mother. But as the operation is attended and immediately followed by a profuse flow of blood, this, by suddenly and powerfully depressing the heart's action will permit the process of coagulation to take place in the bleeding mouths of the vessels. In this manner we may account for the subsequent arrest of the flooding; for we know that a sudden and copious gush of blood, by its salutary influence in producing early and temporary prostration or collapse, and thereby enabling coagula to form, is considerably less dangerous to life than a small and continuous stream of blood, which seldom acts on the circulation until it does so with a force which is at once irretrievably fatal. Thus, in 31 of the recorded cases, or about one-half of the entire number treated by this method, it is expressly stated that the detachment of the placenta was resorted to under the condition of extreme exhaustion. Now in these cases the already existing prostration of the circulation was unquestionably such as would enable coagulation to plug up the open mouths of the utero-placental vessels, and thereby prevent further bleeding. In other cases, again, it would seem that the operation was performed at the period when nature would have detached the organ; so that in these cases the arrest of the flooding can be accounted for by the change which the cervix had already undergone.

So far, then, as our statistical knowledge will enable us to judge of the value of this method of treating *placenta prævia*, it would appear not to have been attended with the success which was anticipated on its first introduction into practice; consequently, we are justified in attempting to devise some other means of arresting the flooding in the early stage of labor, in those cases whose characters rank them in the second class already defined. If we desire any method of treatment to be successful in these hazardous cases, we ought to endeavour to base it on a correct appreciation of the process which nature brings into operation to arrest the hemorrhage. If, therefore, it is correct, as I have endeavoured to prove, that the process in question is *expansion of the cervix*, we ought to assist her in effecting it, when her own powers are inadequate for the purpose.

It is from these considerations that, in the cases alluded to, in which something must always be done not only to check the flooding but to advance the labour, that I would venture to suggest *mechanical expansion of the cervix, by means of a sponge tent specially constructed for the purpose*, as a method of treatment. From the operation of this procedure the following results may reasonably be expected:—

1. It would gradually throw off the placenta, and by putting the fibrous structure of the cervix on the stretch, it would compress the utero-placental vessels. In other words, it would, by its action on the cervix, detach the placenta and arrest the hemorrhage *pari passu*.
2. It would act both as a plug and as a powerful compress applied to the opened mouths of the ultero-placental vessels.
3. It would excite uterine action.

In order to produce these important effects, the *sponge tent* employed would require to be rounded at its upper extremity to prevent its introduction injuring the placenta; it would require to be of large size, and so constructed as to expand rapidly under the influence of tepid injections.

To this method of treatment might be added the administration of ergot, or the application of galvanism, as recommended by Dr. Mackenzie of London, according to the peculiarities of each individual case. And although I have not as yet possessed an opportunity of testing its utility, I have ventured to suggest it as a legitimate conclusion, based on the anatomical, physiological, and pathological reasoning which I have already adduced, leaving further details for a future opportunity, when it shall have been put to the test.

[We pay the greatest attention to the opinions of such men as Dr. Simpson and Dr. Barnes, and this paper of Dr. Donkin is an excellent one, but none of these opinions hitherto have induced us to alter our old practice, which is exceedingly simple, easy and efficacious. When we are called to a case of placental presentation with flooding, we simply *plug* the os uteri and vagina completely, so that flooding cannot possibly occur, removing the plug occasionally to examine the state of things. As soon as the os is dilated, or dilatable, we turn the child and deliver. Little blood need be lost, and the operation is perfectly easy when done early. As soon as the child is turned hemorrhage ceases. Out of numerous cases we have very seldom met with any adverse event.—Ed. Retrospect.]—*Edinburgh Medical Journal, April,* 1859, *p.* 883.

114.—PLACENTA PRÆVIA.—AIR-PESSARY USED TO PLUG AND DILATE THE OS UTERI.

By J. Jardine Murray, Esq., lately Resident-Surgeon in the Royal Infirmary, and formerly House-Surgeon in the Royal-Maternity Hospital at Edinburgh.

[The patient, aged 29, was in the seventh month of the eighth pregnancy, and for nine weeks had suffered from hemorrhage more or less severe. Labour-pains had set in, accompanied by great loss of blood; and the vagina had been plugged by Mr. Pickard, of Brighton, the usual medical attendant, who requested the writer to see the case.]

When I first saw the patient on the evening of April 16, the skin was cold, the pulse weak and rapid, the lips extremely blanched. Labour pains were recurring at regular intervals of ten or fifteen minutes, each pain accompanied by hemorrhage. On examination, the os uteri was found to be little more than the size of a shilling, its margins thick and firm. The tip of the finger encountered the spongy placental substance overlying the patulous os. Sweeping the finger round the external surface of the placenta, I broke down its uterine adhesions within an inch and a-half of the margin of the os uteri; and, detecting the membranes toward the right side, I at the same time evacuated the liquor amnii. During an hour there was very little bleeding; but at the expiration of this period, another gush of blood accompanied a pain. On examination it was found that the os uteri was still insufficiently dilated to admit two fingers. Alarming syncope occurred; and though the patient was plied with diffusible stimulants, she did not rally for some minutes. It was evident that her only safety lay in speedy delivery. With the double object of controlling the hemorrhage and dilating the os uteri, I now introduced a flattened caoutchouc air-pessary between the wall of the uterus and the presenting surface of the placenta; and, retaining the pessary in its place by the tip of the finger, I cautiously inflated it by means of the attached syringe. A portion of the surface of the expanded pessary could be felt closely opposed to the margin of the us uteri. During thirty minutes no bleeding ensued. Then the pains became more powerful, and blood began to trickle over the thigh of the patient, who lay on her left side. More air was pumped into the pessary, and with its increased dilatation all hemorrhage ceased. From time to time we had demonstration of the value of the air-pessary as a plug; for whenever the trickling of blood recurred, it was effectually checked by further dilatation of the pessary by a few strokes of the syringe. About two hours after the first introduction of the pessary, the os uteri was ascertained to be nearly the size of the rim of a wineglass; and the air was permitted to escape from the now considerably inflated pessary, which then quickly collapsed, and was withdrawn. During a gush of hemorrhage which ensued, the hand was introduced, and the shoulder being found to present, the feet were seized, and the fœtus extracted. The placenta was immediately detached and removed, and the uterus became firmly contracted.

From its unusually large size, the placenta must have extended over the greater portion of the internal surface of the uterus. Over the edges of a portion of the uterine surface of the placenta there was a thin indurated crescentic layer of decolorised coagulum, four inches in length. This coagulum had probably been formed after the first onsets of hemorrhage, nine weeks previous to delivery.

The lochia were somewhat more copious than usual; but the patient recovered without a bad symptom.

Remarks.—From the exsanguine and occasionally pulseless condition of our patient, this case was one of much anxiety. The midwife in attendance did not solicit medical assistance till the patient had already lost much blood, which may be partly explained by the fact that she had been delivered prematurely on three previous occasions. That the hemorrhage did not cease after puncture of the membranes may have been due to the transverse presentation of the fœtus. It seems well established by the lamentable results of numerous cases on record that in placenta prævia forcible dilatation of the rigid os uteri by the hand is extremely dangerous,—probably from the risk of lacerating the dilated uterine vessels at the site of the placental attachment. The gentle and uniformally applied expansive force which the air-pessary is fitted to exert differs widely from forcible dilatation by the introduction of the fingers or hand. But I should doubt the propriety of applying even the equable dilating force of an air-pessary to a rigid, unyielding os uteri in a case of placenta prævia. In the case now under consideration the os uteri, though thick and firm, had not the rigid unyielding character sometimes met with even after profuse loss of blood; and the application of the air-pessary was so simple and effective that I venture to hope that in similar cases its employment may be followed by the most beneficial results. While the inflated caoutchouc bag acted admirably as a direct plug and dilator of the os uteri, and also as a powerful stimulus to reflex action, it may possibly have been of service in further separating the placenta from the uterine walls.

It is possible that the use of the vaginal plug might have been equally effectual in saving our patient's life.

Now, in cases of uterine hemorrhage in which direct plugging and dilatation of the os uteri may be deemed improper, I believe the inflatable caoutchouc bag used as a vaginal plug will be found to possess advantages over the tampon of sponge, cotton, or tow; for—

1. The material of the caoutchouc bag does not absorb; and it therefore acts more immediately and more efficiently in arresting hemorrhage than such substances as sponge, linen, cotton, or tow.
2. When uninflated the bag is small in bulk, and its introduction is therefore easy and painless.
3. When inflated to the requisite extent, it adapts itself to the surface with which it is brought into contact.
4. Whenever it is desirable to ascertain the condition of the os uteri, the air may be permitted to escape from the inflated bag, which then quickly collapses, and is readily withdrawn without pain to the patient.

The caoutchouc air-pessaries, which are made of various strength, shape, and size, were invented by M. Gariel, who, however, advocated their use only in the treatment of displacements of the uterus. I believe that to Dr. Keiller, of Edinburgh, we are indebted for extending their application to the dilatation of the passages in primiparæ, and in the induction of premature labour.

The case which forms the subject of this paper appears to be the first in which the air-pessary has been used in the double capacity of plug and dilator of the os uteri.—*Med. Times and Gazette, June* 11, 1859, *p.* 506.

115.—ON SOME RECENT CASES ILLUSTRATING THE PHYSIOLOGY AND TREATMENT OF PLACENTA PRÆVIA.

By Dr. Robert Barnes.

In a paper read before the Obstetrical Society of London, the author submitted fourteen cases which had come under his own care since the publication of his work on the subject; and appended two series of propositions—the one physiological, the other therapeutical—which appeared to be either proved or illustrated by these cases.

Amongst the physiological propositions were the following: That in many cases of placental presentation, there arrives a stage when the hemorrhage is spontaneously arrested. That this physiological arrest is not owing to pressure upon the bared surface of the uterus by the bag of liquor amnii, or the child; nor death to the child; nor to syncope; nor to total detachment of the placenta. That this physiological arrest of the hemorrhage is observed when that part of the placenta which had been implanted within the cervical or lower zone of the uterus has been all detached, contraction of the uterus attending. That, this stage reached, there is no physiological or pathological reason why further detachment of placenta seated within the middle and fundal zones should occur until after the expulsion of the child, when—and not till then—the remainder of the placenta is cast off as in normal labour. That the position of the greater portion of the placenta to the posterior wall of the uterus in these cases, where it forms, by resting on the projecting promontory of the sacrum, a solid inclined plain, directed forwards, is a frequent cause of the transverse presentations which are apt to complicate placenta prævia. That in a great majority of the cases where an edge of the placenta comes down to the os internum uteri, the umbilical cord springs from this edge, and thus is ready to fall through into the vagina, should the os not be occluded by the child's head.

Amongst the therapeutical propositions were the following: That owing to the high vascularity and development of the lower segment of the uterus, resulting from this part being the seat of the placenta, uterine inflammation and puerperal fever are exceedingly likely to ensue from the pressure and contusion attending the passage of the child. That this danger is much increased by the forcible introduction of the hand for the purpose of turning and extracting the child before the os uteri has expanded. That in some cases, where it is observed that the placenta has been separated spontaneously from the lower segment of the uterus, the os being expanded to the size of a crown-piece, and the hemorrhage having ceased. it is not necessary to interfere with a labour now become natural *quoad* placental attachment. That since the os internum uteri *must* expand to the diameter of the child's head, and since, during the dilatation, placenta adhering to the lower segment is liable to successive detachment, causing hemorrhages, it is an indication to expedite this stage of the labour as much as possible. That in some cases the ordinary means of inducing contraction—such as rupturing the membranes, plugging the cervix, ergot or galvanism—will suffice to cause the rapid and safe expansion of the os. That the adhesion of placenta to the lower zone of the uterus impedes the regular progress of labour, and delays the equable expansion of the os uteri. That in these critical cases where forced delivery or the artificial total detachment of the placenta are dangerous or impracticable operations, the introduction of the index finger through the os, and the separation of the part of the placenta adhering to the cervical zone, is a safe and feasible operation.—*Lancet, April* 16, 1859, *p.* 392.

116.—ON THE TREATMENT OF PHLEGMASIA DOLENS.

By Dr. J. Y. Simpson, Professor of Medicine and Midwifery in the University of Edinburgh.

[Before proceeding to the treatment, the author goes at some length into the pathology of the disease. His views may be stated to be as follows:—That the disease cannot justly be considered as merely and simply the result of the obstruction of the principal vein of the affected limb, by the occurrence of acute inflammatory exudation on the free surface of the lining membrane of that vein. If a portion of vein be isolated by ligatures, and the included portion emptied of blood, irritants entirely fail in causing inflammatory effusion from the serous membrane of the vessel. We have cases of phlegmasia dolens without obstruction of the pelvic or crural veins, and we have the latter without the former. The proximate cause of the suffering in cases of phlegmasia dolens seems to be the over-distension of the coats of the smaller vessels by the contained consolidated blood. The pathological origin of phlegmasia dolens is a vitiated or toxæmic state of the blood, and is always preceded by morbid states, more or less vitiating and altering the constitution of the circulating fluid. In the lower animals a diseased state very exactly resembling phlegmasia dolens, may be induced by throwing a solution of lactic acid into their blood. The disease occasionally, though rarely, occurs in the upper extremities, generally in the lower, owing to their circulation being more languid, the pelvic veins being much dilated during pregnancy. It generally occurs in the left leg, owing to its vein being longer, and more or less pressed upon by the right common iliac artery. Dr. Simpson then continues, that the indications for the treatment of phlegmasia dolens resolve themselves into the two categories of]

1st, General; and 2ndly, Local remedies; and in following out both classes of indications you will find that there are three principal rules to be attended to. Among the

A. General Indications.

1st. *Depuration of the Blood* holds the first rank. The importance of this indication must be at once admitted, if you have been convinced of the correctness of Dr. Mackenzie's observations and the truthfulness of his deductions. If there be abnormal and deleterious matters present in the blood, which have a peculiar action on the veins tending to the coagulation in them of the circulating fluid, then it is clearly a matter of prime importance to get rid of these morbid substances at once, so as to prevent the occurrence of coagulation altogether; or, at least, to arrest and check the process when once it is begun. From this point of view, this means of treatment must be regarded as being almost as much prophylactic as curative in its nature. Trye tells us, that he never lost a patient out of those whom he had seen with this disease; and this success he deemed to be due to the practice which he always adopted of administering an emetic as the first part of the treatment of every case. The shock produced by the emetic has a powerful effect in checking the progress of the malady; but probably the chief virtue of the remedy lies in its action as a most efficient depurant of the blood. At the outset I believe there is no better means of arresting the disease than by the administration of an efficient and gentle emetic of any kind, provided the state of the patient does not forbid its administration, and especially in cases where exposure to cold has acted as the exciting cause. But if that fails, or is contraindicated by the debilitated condition of the patient, then we must try to purge and purify the blood by exciting the various organs of elimination to increased activity by means of their appropriate stimulants. Hence the value of the various diuretics, diaphoretics, and purgatives in the early stage of the disease. Many practitioners are in the habit of giving for the first day or two of the attack a mercurial purgative combined with ipecacuanha or antimony, using in this way both the intestinal and cutaneous surfaces as eliminatory excreting organs. But these, or any analogous therapeutic means, must not be pushed so far as to produce any very debilitating effects. Small and repeated doses of alkaline salts given in such quantities as to act upon the kidney, and, if necessary, also on the skin and bowels, are perhaps, as the general rule, both more safe and more efficient—as bicarbonate, acetate, nitrate or tartrate of potass; conjoined, when required, with wine of ipecacuanha or antimony, or with some aperient tincture. Besides these, salts of potass, the analogous diuretic and aperient salts of soda, are generally held to have the effect of impeding, preventing, or arresting any morbid tendency in the blood to coagulate spontaneously, and of tending powerfully to the dissolution of these coagula when once they have taken place. In one patient chlorate of potass was used, and in addition tincture of the muriate of iron was employed, both as a renal depurant and as a tonic.

2nd. *The Use of Antiphlogistics and Febrifuges.*—General antiphlogistic measures are by many practitioners held to be indicated in the first stage of phlegmasia dolens when the pulse is high and strong, and the general symptoms of fever prevail. But if employed at all, they should, I believe, be rarely or never employed to any heroic degree. Formerly phlegmasia dolens was regarded as an affection which very seldom indeed proved fatal; and the strongest antiphlogistic measures employed by the authors who have reported it as such were little or nothing more than local blisters at the groin; or these preceded by moderate local leeching. Perhaps when the history of phlegmasia dolens is fully written. it will be found that the danger and mortality attendant upon the disease have been increased since venesection and other heroic antiphlogistic measures were resorted to in its management, under the idea that it was purely and radically an intense local inflammation of the lining membrane of the large veins of the affected limbs. In almost all cases of phlegmasia dolens, you will find the pulse much quicker, and more or less marked symptoms of fever present. But the best febrifuges which you can use are the depurants of which I have already spoken; and usually the time soon comes when you must, in preference, have recourse to,

3rd. *The Administration of Tonics and Stimulants.*—As the disease frequently runs a tedious course, and is often asthenic in character from its first beginning, the use of this class of remedies is indicated in most cases from an early period, and their use generally requires to be assiduously persevered in. It takes a long time to undo the effects of the changes in the nutrition of the limb consequent on the occlusion of so many of its blood-vessels, and while the process of absorption is going on and the tissues are gradually becoming restored to their normal condition, it is necessary to keep up the patient's strength by the judicious administration of tonics and stimulants, in addition to proper food. It matters little, in most cases, what spécial remedies of this class you employ; but wine and the preparations of iron and quinine are the constitutional remedies which you will find to be most frequently had recourse to in the second stage of phlegmasia dolens.

B. The Local Indications

may likewise be classified under three divisions, viz.:

1st. *The Use of Local Antiphlogistics.*—Means must be adopted to subdue the inflammation which is set up in the veins, whether this arise primarily from injury or disease of their walls, or secondarily from changes resulting from the primary distension and subsequent softening process that occurs in the occluding thrombus. The patient usually complains of much pain and tenderness on pressure along the course of the filled and cord-like iliac and femoral vein, and under the idea that the disease was fundamentally and primarily inflammation of the femoral veins, most practitioners have been in the habit of ordering one or two dozen leeches along the inside of the affected thigh; and after the bleeding consequent on the application of the leeches had ceased, the use of a series of blisters was strongly recommended by some of the older authors. What formerly I have usually done was to apply leeches at the outset, and afterwards to paint the tract of the femoral vessels all over with tincture of iodine. Latterly, I have seen several cases where little or indeed no active local antiphlogistic measures of this kind were adopted; and I believe these cases have progressed as well, or indeed better, than where I followed the usual plan of local leeching and blistering. Perhaps these antiphlogistic measures ought to be reserved for cases where secondary inflammation comes on in the walls of the veins, as a result of the distension and irritation of these walls by the contained and disintegrating plugs or columns of consolidated blood. Then, to relieve the pain, you will require often in this disease to employ opiates internally and freely; and you must have recourse, further, to the use of fomentations. One of the best and most soothing measures that you can adopt for this purpose is to wrap the limb in a wet towel or in a sheet of dry cotton-wadding, carefully and completely covered with oil-cloth to prevent the escape of the insensible perspiration. I believe you will find an application of this kind to be the best of all poultices; an ordinary poultice—say, of bread and milk—

although at first several degrees warmer than the part to which it is applied, falls to the same temperature in a very short space of time, and then its continued soothing effect seems to depend entirely on its power of keeping in the insensible perspiration. But such also, as I have said, is precisely the *modus operandi* of the wadding enveloped in oil-cloth; and this application has one advantage over the poultice, that while the latter requires to be very frequently renewed, the former may be left unchanged for an almost unlimited period. I am seeing, just now, with Dr. Moir, a lady suffering from a very smart attack of phlegmasia dolens, coming on after delivery, who has derived the greatest relief and comfort from the use of cotton-wadding rolled all round the limbs, and completely surrounded by a covering of oil-skin cloth. In this case the wadding has been taken off every twenty-four hours, and a fresh piece applied. If the pain of the limb is very severe, apply sedative liniments and applications, with or without the fomentations, or in a permeable covering. As local sedative liniments, you may use equal parts of olive-oil and laudanum; or two parts of olive-oil and one of chloroform; or the common opiate liniment of the pharmacopœia. With all this the limb must be kept at perfect rest. The limb itself is usually very much paralysed, and motion in it is almost impossible; but the patient must be enjoined to keep perfectly quiet, and to abstain, as much as possible, from moving any part of the body. There is another local measure of the highest moment, and one you must not overlook—viz., position. The proper position is elevation of the limb, which frequently gives great relief from pain, and always favours at the same time the process of resorption. In truth, position is one of the most powerful means in the treatment of many diseases. Attention to this point is frequently of more importance, and affords more satisfactory results, than the use of any kind or quantity of drugs. It is usually attempted to secure elevation of the limb by placing it upon a pillow, but this means is not always very satisfactory, as the limb readily rolls off whenever the bed comes by any chance to be moved or shaken. A better way is to raise all the lower half of the mattress, by placing something underneath, so as to make the limb lie on a sort of inclined plane, and in a position from which it cannot too readily be moved. Elevation of the limb in this manner not only tends to afford the patient the most effectual relief from her suffering—it is also of most essential importance in aiding us to carry out another local indication for treatment, that, viz., according to which we endeavour,

2nd. *To Promote Absorption.*—After the inflammatory stage has fairly passed over, it behoves us to try to bring about a reduction in the size of the limb by stimulating and promoting the process of resorption, so as to bring back the limb to its normal state. By careful and constant elevation of the limb, as we have just seen, much may be done to favour the absorption of the effused matters; but its effect may be greatly aided and increased by the firm support afforded by a flannel bandage applied pretty tightly from the toes upwards. Frequent frictions, the use of stimulating liniments, and the occasional application of a small fly blister to the groin, are also important adjuvants in the fulfilment of this indication. Dr. Dewees, Dr. Meigs, and other American practitioners, strongly recommend, indeed, the free and repeated use of friction of the limb, with sedative liniments even in the earlier and most acute stage of the malady, and totally envelope the extremity, in the intervals between the frictions, in flannels wrung out of a hot mixture of equal parts of vinegar and water. When the swelling has been reduced you require,

3rd. *To Endeavor to Restore the Power of the Limb.*—The limb may remain weak and almost powerless for some months, or even for a year or two, or for life. To restore it to its proper power, you will be obliged to have recourse to such local tonics, if I may so call them, as frictions, bandages, and the use of warm douches, which must all be persevered with for a lengthened period. Sea-bathing, and all means calculated to restore and invigorate the general health, will also have a beneficial action on the weak extremity. Local stimulants, such as galvanism, may be applied to the limb itself; but its power will never be completely restored until the patient has begun to use it, and accustom it to frequent exercise.—*Med. Times and Gazette, June 18, 1859, p. 620.*

117.—ON THE PATHOLOGICAL ANATOMY OF PELVIC CELLULITIS.

By Dr. J. Y. Simpson, Professor of Medicine and Midwifery in the University of Edinburgh.

The disease consists essentially and primarily of inflammation of the cellular tissue of the pelvis; and to understand aright its nature and its course, and to be able to construct a proper plan of diagnosis and treatment, we must study,

1. *The Seat of the Inflammation.*—The pelvis, as you know, is lined and closed in inferiorly by means of a fascia, which gives off sheaths to the different pelvic muscles, and furnishes processes for the protection, support, and separation of the various pelvic organs. Wherever two layers of this fascia approach each other after covering the opposite surfaces of any organ or muscle, and wherever two layers covering contiguous organs come to be in opposition, you will find that there is always a greater or less quantity of loose cellular tissue interposed between them. Thus between the layers of the broad ligaments, between the vagina and the rectum, between the iliac muscle and the bone, and in short, in almost every part of the pelvis, there is an abundance—a great abundance—of cellular tissue; and, I repeat, the disease of which I have now to speak consists of inflammation, acute or subacute, of this abounding cellular tissue.

2. *The Products and Terminations of this Inflammation* vary according to the stage of the disease, and may be considered under the four following divisions:—

1st. Serum;
2nd. Pus;
3rd. Coagulable Lymph;
4th. Sloughing of the Cellular Tissue.

1st. *Effusion of Serum* is the first phenomenon that occurs when inflammation has become established in the cellular tissue of the pelvis. The necessary result of the effusion of this fluid is a swelling in the part, which may be felt on examination through the vagina or rectum, or even through the abdominal parietes, according to its special seat. This swelling is of greater or less extent, according to the intensity of the inflammation, and according to the freedom with which the fluid is allowed to escape along the cellular tissue, and to pass from one loculament or division of pelvic fascia to another. The swelling or tumour produced by the effused serum is from the first firm, dense, and resistant to the feel, and sometimes becomes very hard, particularly when the effusion takes place between two layers of the fascia which are not loose, and mobile, and yielding, but are bound at their margins to some osseous ridge or strong ligamentous band; as when it occurs, for example, in the wall of the pelvis, externally to the margin of the broad ligament of the uterus. In such a situation the swelling comes sometimes to feel hard as a cricket-ball, or a "deal board," as Dr. Doherty has expressed it, and has been, and might easily be, as far as mere hardness is concerned, mistaken for an exostosis of the ilium; and when in the roof of the vagina it sometimes feels as firm to the touch as a scirrhous growth. One other common and very characteristic feature of the tumour is this—it feels as if it were *affixed* to and grew, as it were, from the side of the pelvic bones, whenever the effusion has extended to a sufficient extent laterally, so as to reach the walls of the pelvis. Florid tumours of the uterus, ovarian tumours, and others with which the "inflammatory tumour, of pelvic cellulitis are sometimes confounded, do not present this symptom. After the effusion has invaded loculament after loculament of the pelvic fascia it soon loses its primary round or oblong form. In examining the swelling day after day you can sometimes find it daily altering its figure, and occasionally you can thus trace its gradual process, as it creeps from one side of the pelvis to the other, and passes before or behind the neck of the uterus, and at last involves and *fixes* the uterus in its mass. The inflammatory effusion is by no means always limited to the cellular tissue of the lower or true pelvis. Often from the first a large swelling or effusion can be felt in one or other iliac region; and I have seen one or two cases where the tumour was in a great degree central, and as large as the uterus at the fourth month. Occasionally the disease invades the cellular tissue in the right iliac fossa, and around the head of the cæcum, without even stretching downwards into the true pelvis. Again, we see occasionally in practice smaller inflammatory swellings and effusions limited to single loculaments of the pelvic cellular tissue situated in the anterior or on the posterior walls of the vagina, and not even passing upwards to the cellular tissue contained between the broad ligaments and lying around the neck of the uterus.

I should wish particularly to have it impressed upon your minds, in regard to this disease, that there is no pus in the effusion or tumour at first, and that it does not begin in any case by being an abscess, but that first of all an effusion of serum takes place into the inflamed cellular tissue, and no formation of pus generally occurs till about ten or fourteen days afterwards. In one of the first cases of the disease I ever saw, and when I was only beginning to learn something of its nature and course, I got a lesson in regard to this matter which is very strongly impressed upon my mind. Dr. Andrews, who was then a lecturer on Midwifery in London, was on a visit to Edinburgh at the time, and I offered to show him a case of pelvic cellulitis as an object of interest; for the subject was then new, and not many cases of the disease had been observed. The patient was a young girl, of twelve or thirteen years of age, in whom I had made sure that inflammation had been set up in the broad ligament of the uterus, and caused great swelling and induration of it. This I supposed to be due to an effusion of pus, for we then spoke of and thought of the disease as always "pelvic abscess,"—and about the tenth day from the commencement of the disease, I introduced, in presence of Dr. Andrews, an exploring needle with the view of bringing away the pus and reducing the swelling. But to our astonishment there flowed into the dish held to receive it, not yellow pus, but a transparent fluid so clear and limpid that Dr. Andrews at once whispered to me, "Have you not punctured the bladder?" I knew from the direction of the needle backwards that it was not near the bladder, and after a considerable quantity of this clear fluid had escaped, we soon had evidence that it was not urine, by its rapid coagulation in the cup which contained it. It was serum, such as you see in the stages of many inflammations, and such as you may obtain from the skin after the application of a vesicant; and in the earlier stages of pelvic cellulitis it is this inflammatory serum whose effusion gives rise to the swelling and hardness, and which is the only fluid you will then procure on using an exploring needle. If the inflammation be not now subdued, and the effused serum be not absorbed, it will betimes lead to

2nd. *The Formation of Pus.*—You may meet with a case of pelvic cellulitis where there is an effusion of serum attended with much pain and distress; yet if the disease proceed no further and resolution of the process be effected, the disease might run its course, and its real nature might be altogether unsuspected, unless you had previously been made aware of the probability of its occurrence. But if the inflammatory process remains unchecked, and goes on to its higher stage of suppuration, the disease is not so likely to be overlooked; and hence most of the descriptions that we have of the disease refer to it only in this more advanced stage. The pus may become developed in any part of the pelvis where inflammation has been set up, and in some cases it is confined to one fascial loculament, while in others it occurs in two or more simultaneously. It does not always remain in the part where first it is formed, but forces its way from one loculament to another, until it reaches a cutaneous or mucous surface through which it may be evacuated. Its progress is often very difficult to trace, and to understand it aright requires an intimate knowledge of the course and connection of the fascial sheaths of the pelvic organs. Perhaps the most common seat for the development of an abscess is in the cellular tissue of one or other of the broad ligaments of the uterus, and when matter has been formed there, it may make its way towards the roof of the vagina either by passing in front of the neck of the uterus between it and the bladder, or, as is far more frequently the case, by sinking backwards between the cervix uteri and the rectum. In such a case the abscess may open either into the rectum or into the vagina, which are the two most common canals for the evacuation of pelvic abscesses, and fortunately also the two most favorable. But there are two other cavities into which the matter sometimes, but less frequently, finds its way; and as they are not by any means so accessible, the treatment of the case becomes more complicated and difficult. These are the cavities of the bladder, and of the body or cervix of the uterus; and when one and the same abscess opens into both of these organs, as sometimes happens, a form of vesico-uterine fistula results which is not always very amenable to treatment. On the other hand, you will sometimes find the matter burrowing onwards, and finally making an opening for itself in some part of the cutaneous surface of the body. The pus may pass, for example, underneath the pelvic fascia, and escaping from the pelvis along with the femoral vessels, it may come to point somewhere in the groin; or it may pass backwards through the great sacro-sciatic notch along with the sciatic nerve, and lead to the formation of an abscess in the hip, as I lately saw in a case which I visited along with Dr. Moir. Again, the matter in some cases sinks downwards, and escapes at some point in the pelvic outlet.

One interesting point which it is of importance to observe and to remember in connexion with the evacuation of pelvic abscesses is the extreme rarity with which they become discharged into the cavity of the peritoneum. When we know that inflammation has been going on around some of the pelvic organs, and has led to the formation of pus beneath the peritoneum, we might be apt to form a very unfavorable prognosis, and to look upon a fatal peritonitis as almost certain to ensue. But experience of such cases assuredly does not warrant us in entertaining such a gloomy view, for this reason, that abscesses forming under the peritoneum very rarely perforate it, and open into its cavity. Why this is so, is not very easy of explanation. Cruveilhier avers that it is because the peritoneum is lined, and protected by a layer of fascia, the existence of which is denied by others. In some cases adhesive peritonitis is set up, and the abscess is prevented from bursting into the peritoneal cavity by the resistance offered to it by the two adherent layers of the membrane. But whatever be the explanation of it, the fact is not the less true, and it is always a hopeful one to be borne in mind—that the peritoneum has a very remarkable power of resistance to the passage of matter, and that, in consequence, pelvic abscesses very rarely terminate by evacuation into its cavity. I shall have to revert again to this point when I come to speak of the artificial evacuation of the pus; and I pass on now to notice another product of inflammation in the cellular tissue of the pelvis, viz.,

3rd. *Coagulable Lymph.*—When an effusion of firm, solid, coagulable lymph, or fibrine, takes place into one or more of the fascial loculaments in the course of the disease, the swelling which results is extremely dense—really as hard as the "deal board" of Dr. Doherty—and many long months usually elapse before it softens, breaks down, and is discharged. I have a patient under my care just now in whom suppuration has set in after an attack of pelvic cellulitis, which came on when she was in the country, so that there is now formed an abscess in the right iliac fossa. But fourteen years ago she had an attack of the disease, which came on after a confinement, and took on the form I now refer to. On that occasion there was an effusion of coagulable lymph into the cellular tissue around the cervix uteri, forming an extremely dense solid tumour, like a hard cancerous growth, or deposit in the roof of the vagina. Nearly two years elapsed before this effusion was fully absorbed. When pelvic cellulitis terminates in effusion of coagulable lymph, the swelling is always very long in disappearing.

4th. *Sloughing of the Cellular Tissue* of the pelvis sometimes occurs as a result of inflammation in it. It is produced by the compression of the vessels caused by the effusion of lymph or serum into the surrounding tissue. The parts, deprived of their usual supply of nutritive matter, die, and being separated by a suppurative process from the surrounding textures, they come to lie in the midst of a fetid abscess, and a cure in such a case cannot be accomplished till a free opening is made, and these necrosed masses are evacuated, along with the pus in which they are imbedded. I have seen cases where very large sloughs thus escaped, or were removed rather by the finger passed through the artificial opening. These cases are always most severe and exhausting in their character, and usually long also in their duration.—*Med. Times and Gazette, July* 9, 1859, *p.* 26.

118.—ON THE INTRODUCTION OF AN EXPLORING NEEDLE IN THE DIAGNOSIS OF PELVIC ABSCESSES.

By Dr. J. Y. Simpson, Professor of Medicine and Midwifery in the University of Edinburgh.

[It is necessary first to state that the spot at which the feeling of fluctuation is generally for the first time perceived, in cases of pelvic abscess, is in the roof of the vagina, immediately behind the cervix uteri, or to one side, as if where the broad ligament would open below if its layers were separated by accumulated fluid.]

The exploring needle, which is of invaluable service in the examination of diseases, is never used to more advantage than when employed for the exploration of pelvic abscesses, when they happen to be unusually difficult or doubtful in their diagnosis. For in the common run of cases you will usually be perfectly able to make out the diagnosis without this assistance. In any case, however, of pelvic cellulitis where you are in doubt as to the formation of pus, and have reasons for being certain of its presence, you may make sure of it at once by pushing an exploring needle into the centre of the tumour. The instrument has, perhaps, been neglected too much as a means of diagnosis in surgery, when we consider with what freedom from danger its employment is attended, and how frequently abscesses, aneurisms, and other tumours have been confounded together, when by its use such mistakes could easily have been avoided. I know of a case where a distinguished surgeon introduced an exploring needle into a tumour in the groin, under the belief that it was a bubo, and to prove to others present that it was so; but to his astonishment, no pus escaped, and instead, a quantity of air. It was a crural hernia, the sac of which had become inflamed, and into it he thrust the exploring needle, and thus saved himself from the fatal mistake of laying open a hernial tumour with a bistoury, when he intended only to open an inflamed and suppurating gland. The use of the exploring needle saves, I know, from many mistakes in obstetric surgery; and since the safety with which it may be introduced into the most important organs and the most malignant tumours has been abundantly demonstrated, I think its employment might be advantageously extended, The best exploring needle is a long, slender, thread-like trocar, with a wire stilet passing through it. Of course there will be no escape of pus through it, when it has been thrust into a solid tumour or into an inflammatory swelling before pus has been fully formed, and even when pus is there, it is usually only a drop or two that escapes through the narrow tube. You will not find the pus in some cases traverse the trocar, particularly if the pus is thick, but on withdrawing the trocar and blowing through it, a drop or two will escape from the end of the trocar. But when none flows out, the negative sign is itself of importance. While, on the contrary, if a drop escapes, it may be a sufficient warrant for you to proceed to the more free evacuation of the purulent collection. A medical practitioner, of great ingenuity, who had been many years in India, when his health began to fail him, came home several years ago, and while spending a short time in Edinburgh, I had an opportunity of showing him some cases of pelvic cellulitis, where I succeeded in demonstrating the presence of pus by the use of the exploring needle. Soon after going to London he met some medical men there in consultation upon an old Indian patient of his. The case, as he afterwards told me, seemed to him to present the chief characteristics of the example of pelvic abscess which he had seen here. He expressed to the medical attendants of the lady his opinion of the case, and proposed as a means of settling the difficulty to introduce an exploring needle into the seat of the disease. The other doctors rather scoffed at the idea; but, as they were altogether at sea as to the nature of the disease, they agreed to allow him to introduce the exploring needle, which he accordingly did. To his great surprise and vexation, however, no escape of pus followed the withdrawal of the stilet; but, being still unconvinced that his opinion was erroneous, as a last resource he applied his mouth to the end of the tube, and succeeded, by sucking it, in extracting a few drops of pus sufficient to convince his sceptical brethren of the true nature of the case, and of the value of the exploring needle as a means of diagnosis. He was then allowed to open the abscess, and the patient got speedily well. In consequence of his treatment of this case, my friend got rapidly into a large practice in London, but after a few years his disease unfortunately returned, and death struck him down.

When, then, you have a patient attacked with rigors, followed by a high degree of fever, and attended with pain in the interior of the pelvis, and when, after a time, the fever changes in character, and instead of being inflammatory presents more of a hectic type, you may be pretty certain that she has been suffering from an attack of pelvic cellulitis which has passed on to suppuration. And in every case it will be possible for you to correct or confirm your diagnosis by means of a careful local examination. One or two marked symptoms may enable you sometimes to make a good guess as to the existence of the disease. Several years ago, I was attending with my friend Dr. Wood, an anxious case of labour, where the lady had manifested symptoms of insanity in the last periods of pregnancy. While thus engaged, a gentlemen came, bearing with him a note to me from the North of England. The note was written by the medical attendant upon the gentleman's wife, and anxiously desired me to visit the lady as soon as possible. In relation to her disease it stated only two bare facts, viz., that the lady who had been confined six weeks before, was hectic, and was suffering from great pain in the pelvis and down one of the limbs. I read the note to Dr. Wood, saying it was, I believed, a case of pelvic abscess, against the accuracy of which diagnosis he was inclined to wager. On visiting the patient in England next day I found a large pelvic abscess which I freely opened, and the patient made an excellent recovery.

Formerly the changes produced by the disease caused it frequently to be confounded with cancerous, fibroid, or cystic tumours of the uterus and ovaries, or other organs of the pelvis; but now we may be almost always sure of the true nature of the case when we find the tumour associated with constitutional phenomena, running a regular and rapid course, and adhering to the bone or periosteum in the remarkable manner to which I have referred. For, let me repeat, inflammatory tumours feel fixed and immoveable to a degree seen in the case of no other morbid growth, and more particularly when occurring in the broad ligament,—their most common seat—and lying close to the ilium, they feel so hard and adherent that they might almost be mistaken for an osseous tumour. The old stories of large ovarian and uterine tumours of a supposed nature yielding under mercury, &c., were in all probability merely tumours formed by inflammatory effusions of the kind I have been speaking of.—*Med. Times and Gazette, July* 16, 1859, *p.* 52.

119.—ON VESICO-VAGINAL FISTULA.

By Dr George Buchanan, A.M., Glasgow.

[Dr. Buchanan gives this case chiefly to show the operation as it was performed by Dr. Bozeman, who was then in Glasgow. The patient, after a severe labour, in which instruments were used, had a fistula between the bladder and vagina, causing the urine to dribble away constantly.]

On admission to the Infirmary, examination of the parts disclosed a fistula the size of a sixpence, communicating between the bladder and vagina, the situation of which was about an inch internal to the orifice of the urethra, and in the mesial line. The perineum was found to be ruptured, and the fissure between the rectum and vagina to extend for about three inches, laying these two canals into one for that extent.

Dr. Bozeman, of America, being in Glasgow on a short visit at this time, he was requested to examine the patient, and he pronounced it a case in which he could nearly insure success by his new plan of operation.

A consultation of the physicians and surgeons of the Infirmary was called, and they unanimously agreed to request Dr. Bozeman to perform the operation. Accordingly, that gentleman, having shown and explained the exceedingly perfect and ingenious instruments which he had brought along with him, proceeded to operate in the following manner, in presence of the hospital staff, and several medical gentlemen who had heard of the case and were interested in the result.

The patient was placed on a table on her knees, leaning forward on her elbows, and the parts were brought clearly into view with the aid of a single-bladed silver speculum, in shape somewhat like a shoe-horn, bent at a right angle about three inches from its broad extremity. This bent part was easily introduced, and being pulled up against the rectum, disclosed the cavity of the vagina throughout its whole extent, the fistula in view now on its inferior wall. The operator carefully made raw the edges of the opening, by seizing first the anterior lip with a small hook, and cutting off a slice about an eighth of an inch in breadth, the edge being bevelled at the expense of the vaginal mucous membrane, so that the raw surface was nearly a quarter of an inch broad. The same kind of edge was formed on the posterior lip of the fistula, but this was accomplished with scissors ingeniously contrived to be used with the right hand, and to cut either from right to left, or the reverse, as might be desired. During this proceeding a good deal of oozing of blood took place, which somewhat retarded the operator; but by careful sponging, the parts were kept free sufficiently to allow all the steps of the operation to be clearly seen. When the fistula was converted into an oval opening with perfectly smooth edges, the patient was allowed to rest a little till the bleeding should cease. The edges were then brought together with four silver-wire sutures. The needles were introduced at the distance of about three-sixteenths of an inch from the cut edge, not pushed through into the bladder, but turned across the wound, beneath its mucous membrane and so brought out at a corresponding point on the opposite side. In this way four silk threads were passed, and to their extremities the silver wires were attached and drawn through. When the opposite wires were pressed together, it was found that the edges of the opening fitted perfectly to each other. The wires were then fixed *in situ* by the button shield, which is the peculiarity of Dr. Bozeman's plan of procedure. An oval disk of thin lead rather larger than the opening, bent into a convexo-concave shape, was pressed against the wound, and found to fit the contour of the vaginal wall. In this were four small holes, at equal distances along its long diameter, through which the four pairs of wire were passed, and the shield was then slid down, its concave surface being pressed against the incision. The wires were then pulled tight, and fixed by leaden bullets. A catheter with a double curve was effected. The patient was then removed to bed, and had immediately a grain of solid opium, which was ordered to be repeated every sixth hour. A nourishing diet was prescribed, with as little fluid as possible, and a pint of London porter daily. Previous to the operation the bowels had been cleared out by a large injection.

August 19. Patient has felt comfortable since the operation. Urine comes freely by catheter, and is collected in a small bed-pan. None dribbles away by the vagina. The bed is kept quite dry.

21. Still going on well. No uneasiness in the wound, and no appearance of ulceration going on. No motion of bowels, or tendency to it, since operation. The opium was diminished to a grain, morning and evening.

26. The catheter has daily been removed, cleansed, and replaced. It remains *in situ* without apparatus, and the patient has remained steadily in one position. The bowels have not been moved, and there is apparently no ulceration around the shield. This being the ninth day from the operation, and the period which Dr. Bozeman recommends, I proceeded to remove the apparatus. The patient was placed on her knees, as at the operation; and the vagina being exposed with the aid of a bent speculum, I removed the bullets by twisting them a little to one side with a long forceps, and snipping across the wires between them and the leaden shield. When the bullets were cut off, the shield fell off, and the wound was seen perfectly cicatrized throughout its whole extent. Three of the wires I easily got hold of with a pair of forceps, and pulled out; the fourth had got embedded in the soft tissue, and I could not find it. However, knowing that a metallic wire would produce no irritation, I left it in, rather than disturb the parts by a prolonged search. The patient was again removed to bed, the catheter retained as before, and she was requested to move about as little as possible for a day or two. The opium was discontinued.

27. Urine passes entirely by the catheter. Complains of headache and uneasiness in bowels. To have two drachms of sulphur and bitartrate of potash.

Sept. 1. Bowels moved freely yesterday by a large dose of black draught, after which the use of the catheter was discontinued. She can now retain the urine for some hours, and pass it voluntarily. Dismissed to-day.

On the 7th September I visited the patient at her own house, and found that the cicatrix had become quite firm. She can retain urine for two or three hours in the recumbent position, but not so long in the erect. Still complains of headache and indigestion, with irregularity of the bowels. I ordered some Gregory's mixture and a small dose of quinine, twice daily.

On the 16th September the patient was stronger, but still had some uneasiness in stomach and bowels. She was ordered to take a little exercise in the open air, having confined herself to the house since she returned home. In examining the cicatrix, which I found perfectly strong, I felt the sharp point of the wire which was left in the tissues when the shield was removed. It had produced no inflammation or ulceration, and I easily seized it with the forceps and withdrew it. The bladder has not yet entirely regained its retaining power when the patient stands or walks, but she has perfect côntrol over the urine for several hours when in bed. The bed is never wet, as it used constantly to be, and both the patient and her husband express themselves exceedingly happy at the result, and thankful for the success of the operation.

I have published this case at Dr. Bozeman's request, and because the merits of this operation are still doubted in some quarters. True, it has not succeeded in every case, and in some instances death has been the issue; but the same can be said of every operation in surgery, however simple. There can be no doubt, however, that a measure of success has followed this proceeding which cannot be affirmed of any other mode of treatment. I do not intend to add any statistical or critical account of its success, but it may be proper to allude to the plan of Dr. Sims of New York, who claims a large amount of success for his operation. To him belongs the credit of having introduced all the preliminary steps of the operation as performed by Dr. Bozeman. The use of the single broad-bladed speculum, and the position of the patient on her knees, does away with a great part of the difficulty of operating on a part so concealed and inaccessible as the wall of the vagina.

The formation of a broad, bevelled raw edge round the fistula affords an extensive surface for union. The introduction of the sutures at a distance from the edge of the wound allows an amount of traction and support which cannot be attained by stitches placed close to the incision. Above all, the employment of thin silver wire as a substitute for silk thread prevents the ulceration or cutting out, which is inevitably caused by organic substances imbedded in the living tissues. It has recently been found by various experimenters that it is of little consequence what metal is used, and iron, copper, and platinum wires have been found to act as well as silver; and the material which may ultimately be chosen as the best suture will depend on the flexibility of the metal.

Dr. Bozeman, in the accounts which he has published of his operation, freely acknowledges that, in the proceedings above mentioned, he has closely followed the directions of Dr. Sims, and only claims for his method of fixing the wires by a metallic shield an amount of success which has not followed that of Dr. Sims, who employs two bars of lead, after the manner of the quill suture. This method of drawing the edges of the wound together, called by Dr. Sims the "clamp suture," is a decided improvement on the common interrupted suture; but the use of a metallic shield, as recommended by Dr. Bozeman, seems to make the operation perfect. By its use the wires can be pulled perfectly tight, and the vaginal aspect of the incision is drawn up into the concavity of the shield; so that towards the interior of the bladder is presented, not the line of the wound, but smooth lips of mucous membrane. The shield also prevents the vaginal and uterine secretions from coming in contact with the wound, which is thus protected, both externally and internally, from the presence of irritating fluids. In the case detailed, the menses flowed during the progress of the cure; and had it not been for the shield, there is little doubt that a considerable irritation would have been set up.

It is much to the credit of American surgery that this distressing affection can now be treated, with a probability of success even greater than any other surgical diseases; and the surgeons of this country cannot fail to recognize the claims both of Dr. Sims and his follower, Dr. Bozeman. Dr. Sims has done much by perfecting the operative part of the proceedings, but we cannot deny to Dr. Bozeman the merit of adding the last, a very essential part, of the treatment. It only remains to add, that the whole of the hospital staff, as well as the other medical men who were present at the operation here detailed, besides being satisfied with the perfect adaptation of the operation to effect the end proposed, were unanimous in according to the skill of the operator a large share of its successful result. There was but one opinion, that while he was entitled to share in the credit of devising the means, his mode of employing them exhibit the master-hand of an accomplished surgeon.—*Glasgow Med. Journal, Oct.*, 1858, *p.* 318.

120.—ON THE TREATMENT OF URETHRAL CARUNCLES.

By Dr. J. Y. Simpson, Professor of Medicine and Midwifery in the University of Edinburgh.

[The occurrence of florid red caruncles at the orifice of the urethra is not infrequent, and when removed, they are unfortunately very liable to be reproduced. Provided these growths do not cause pain or inconvenience, it is better to leave them alone; but if a source of annoyance, surgical interference generally becomes necessary. In some cases their removal cures the patient permanently, but in many, as above stated, the relief is only temporary. This operation, however, simple as it may appear, is not always so easy and successful as the descriptions of it found in books might lead you to suppose.]

You will read, that if you apply a ligature to the polypoid forms of urethral caruncle, cut off the sessile ones, and apply in some cases alum, nitrate of silver, or some astringent or caustic to the parts, you will be able in most cases to cure your patient. But those who speak thus cannot have had many patients for a lengthened period under their observation; otherwise they would have found the disease recurring far more frequently after such operations than their published accounts indicate. For after simple removal with a ligature or scissors, they almost invariably grow again; and there is one further point about their pathology to which I have forgotten to refer, but which renders it a matter of the greatest difficulty to effect a perfect and permanent cure by removal of the urethral caruncles.

There is often in such cases not merely a prominent red caruncle, or a number of them, lying at the orifice of the urethra; but there are in addition to these, a number of small very red specks scattered all about the mucous membrane, around the orifice, and upon the neighbouring mucous surfaces, which red specks and patches are not very striking in appearance and may very easily be overlooked, but which are found to be as exquisitely tender as the urethral caruncles themselves when touched with the point of a probe. These insignificant looking flat red spots are all so many seats and centres of the painful sensation; and if in removing the larger bodies you leave them unheeded, you will never succeed in curing your patient or in affording her permanent relief. From the study of the homological anatomy of the organs of generation in the two sexes, physiologists have come to recognize in the female organs, parts or members corresponding to each of the different constituents of the generative organs of the male with only one very marked exception. Thus we know that the large and well-developed uterus of the female is only the representative or analogue of the small pouch or sinus pocularis in front of the crista galli of the male urethra into which the seminal ducts themselves—the analogues of the Fallopian tubes—enter. So that when in passing a bougie into the male bladder your instrument becomes arrested at this point, as is very frequently the case, you have in reality got entangled in the male os uteri. But there is one member of the generative apparatus of the male for which anatomists have not yet been able to determine the existence of a certain analogue in a female; I mean the prostate gland. All anatomy, both human and comparative, leads us, however, to suppose that the female urethra corresponds to that portion of the male which lies in or behind the prostate at the neck of the bladder, and that the prostate gland belongs rather to the urinary than to the generative organs. In this case analogue in the female of the prostate in the male should be sought for in connection with the urethra or urinary canal, and not in connection with the uterus, as has most commonly been attempted to be done. Now it may be that in the numerous glandular structures which are scattered about so profusely in the neighbourhood of the orifice of the urethra in the female, we have the representatives of the mass of acini which combine to form the conglomerate prostatic gland of the male; and, if so, it would be a curious subject to determine how far the disease which we are now considering depends upon an enlargement and change in these small scattered follicles, and how far it bears a resemblance to some of the diseases occurring in the prostate. But whatever may be said as to the pathological anatomy of the disease, one thing is certain in regard to it, and that is, that, as a very general rule, no hope of a permanent cure can be entertained except by the radical removal of all the small red spots, as well as of the more prominent and projecting tumours. Different authors have recommended different

METHODS TO BE FOLLOWED FOR THE REMOVAL OF URETHRAL CARUNCLES.

1. *Application of a Ligature.*—As in the case of so many other morbid growths, and encouraged, more especially, by the success which attends the use of the ligature in the removal of internal hemorrhoids, some have proposed to get rid of these urethral caruncles by tying a ligature tightly round their base, and so strangulating and causing them to slough off. But here it could only be applicable to those cases where the growth was stalked or poly-

poid; and even then it would be a much more tedious and painful process to tie a thread round the neck of the tumour and leave it there to die and separate, than to have recourse at once to the simpler

2. *Excision with a Knife or Scissors.*—If the ligature may seem to possess some advantage theoretically over cutting instruments in such cases—inasmuch as there can be no fear in using the former, of the hemorrhage which you might suppose likely to attend the latter, when applied to such vascular growths—yet in practice you will find that, while the ligature is in most cases very difficult, or indeed impossible of application, the excision of the diseased structures is not attended with any formidable degree of hemorrhage. I never saw a case where it was not very easily controlled. You may use either a knife or a pair of scissors for the excision. It is usually advantageous to draw out first the caruncles with a hook or small vulsellum, in order to make their removal complete. In operating, you must be careful to remove not only the projecting tumours, but also the pieces of mucous membrane on which they are seated, as well as any pieces of membrane that may be studded with the red spots of which I have spoken to you. Removal of urethral caruncles in this way, I must again warn you, however, though it does sometimes effect a permanent cure, usually affords only a temporary relief. For a month or two, or even longer, the patient may remain free from pain; but, unless some further more potent remedial means be had recourse to, her sufferings are almost certain to return. Along with Dr. Fowler, I saw a few days ago a patient with a painful tumour at the orifice of the urethra, which I removed along with a portion of sound mucous membrane; and this has had the good effect of releasing her in the meantime from the local pain and irritation which have been distressing her. But I greatly fear that you will find as a general rule, that the reproduction of the caruncle and of all its painful symptoms will take place, unless you make use of some powerful means for destroying the morbific tendency of the part; and I believe that the most hopeful and the most effectual means that you can in such a case employ is the

3. *Application of the Actual Cautery.*—I have tried caustics of many different kinds, for the cure of this intractable disease. But seldom have I found permanent success from the employment of any of them. I had a patient under my care several years ago, with a very painful growth at the orifice of the urethra, which I had frequently attempted to burn down with different kinds of caustics, but it was always reproduced; and when excision was had recourse to, the result was no better. I was beginning to despair of ever being able to effect a cure, when at the suggestion of my friend Professor Retzius, of Stockholm, who saw the case with me, I destroyed it with the actual cautery; and ever since the patient has remained perfectly well. Since that time I have used it frequently, and, in many cases, with the happiest result. Yet I must confess to you, that this means, too, has in some instances failed to effect a permanent cure. In order to apply the actual cautery to destroy a caruncle or caruncles of the urethra, 1, you may use either an iron of proper size and shape adequately heated; or 2, you may apply the requisite degree of heat through the galvano-caustic wire. The latter method is specially useful when the caruncles extend up the urethra, higher than the orifice,—because you can introduce and apply the wire, before heating it up by the transmission of the galvanic current. Apply immediately afterwards cold water and cloths soaked in it; and subsequently treat the ulcerated surface, after the slough separates, with very frequent applications of black wash, zinc lotion, or other surgical applications. The position, therefore, in which we at present stand with regard to the treatment of these urethral caruncles is such, that their excision with the knife or scissors, and their removal by means of caustics, are to be looked upon and employed as measures for affording usually a temporary relief only; while their destruction by means of the red-hot iron, or the galvano-caustic wire, affords a reasonable prospect of a permanent cure, although even by this more heroic treatment we cannot be absolutely certain in every case that we free the patient from her disease for ever.

Palliative Treatment.—I have one word more to add with respect to the treatment of this painful affection before I have done with it, and it is this. In those cases where all radical measures have proved ineffectual for relieving your patient, or where the caruncles are in such a situation as not to admit of their removal, you will require to administer sedatives of various kinds internally, or, what is better still, to apply some local anodyne. The best local remedy that I know of for this purpose is prussic acid, applied in the form of an ointment made up of two drachms of the dilute hydrocyanic acid of the Pharmacopœia to the ounce of lard. A bit of this ointment about the size of a pea applied to the part three or four times a-day, often relieves the pain more effectually than any quantity of opium administered internally, or than any other form of local anodyne which I have used. Aconite and chloroform ointments sometimes also form good palliatives. So does tepid water. You will add, sometimes very greatly, to the comfort of your patient, by advising her to sit in a warm hip-bath during micturition.

[We quite agree with Dr. Simpson in most that he says on this subject. In many of these cases we have been successful without the actual cautery, and simply by the application of the strong nitric acid. Both in nævi of the rectum and at the mouth of the urethra in the female nitric acid will often succeed; but it must be kept applied to the part for a much longer period than is generally done. Surgeons generally rub or apply it well over the part. whereas it should be kept in contact for a minute or two. Taper a lead pencil or bit of stick, and cover the end over with folds of cotton thread for about an inch. Then apply a little oil round the nævus, so as to stay the action of the caustic. Next dip the stick and cotton thread into nitric acid, and keep it well applied for a minute or so to the nævus, pushing the stick a little way up the urethra. Now withdraw it, and oil the adjacent parts again, so as to prevent the acid acting too deeply on them. We have thought, since reading Dr. Simpson's paper on different caustics, that his mixtures (equal parts) of chloride of zinc and oxide of zinc, well dusted over the nævus, would be a good application.—Ed. Retrospect.]—*Med. Times and Gazette, April* 2, 1859, *p.* 333.

121.—DIAGNOSIS OF CARCINOMA UTERI.

As to the differential *diagnosis* of carcinoma uteri, I have little to say regarding it at present; for you have not yet had an opportunity of being made acquainted with the other diseases with which it is liable to be confounded. But there is one observation which I should wish to urge upon this point: it is this, that you are not entitled to diagnose the existence of cancer in the womb from any degree or kind of pain, bleeding, and offensive discharge, from which the patient may be suffering, not even when these are combined with the pale cachectic look which is usually pathognomonic of the disease. All these local and constitutional symptoms may be present in other uterine diseases besides cancer; and, in some latent cases of cancer, they may be found absent. You can detect it with perfect certainty only by physical diagnosis. You must make a vaginal examination with the finger—for the speculum is here of little service—and only when the sense of touch has assured you of the condition of the cervix uteri, and not till then, can you honestly and conscientiously pronounce upon the nature of the case.

One morbid condition of the cervix uteri with which cancer of the organ may be, and often is confounded, is that which results from chronic inflammation in it. This causes enlargement of the cervix and expansion of the os, attended with great induration, and when ulceration sets in, and pain and menorrhagia are developed, the case may very readily be mistaken for a case of cancer in its first stage. But here you have always the distinguishing characteristic, to which I have already alluded, that in chronic inflammatory induration of the cervix the deposit is confined to the organ itself, which remains loose and mobile; whereas in the case of cancer the surrounding tissues usually become early involved in the disease, and being infiltrated with the morbid deposit, make the cervix uteri feel firm and fixed. And when ulceration has set in, you will find that the ulcer which results from inflammatory change—though it may look very irregular, rough and ugly—is always on a level with the unbroken surface, or even projects above it, whereas the cancerous ulcer is always depressed, excavating, as it were, and eating out the part in which it has its seat. One often sees mistakes made with regard to the diagnosis of cancer of the uterus, which are almost unconceivable, and which you may in almost every case avoid if you will just keep your wits about you when making an examination, and use a little common sense—a most invaluable and indispensable aid in all kinds of diagnosis. When you feel a rough, irregular, excavated or anfractuous ulcer seated on a hardened base, and surrounded by hardened tissue in any other part of the body, as the exterior of the chest, face, or extremities, you set it down at once as cancer; and when the finger applied to the os uteri recognizes the same condition, you need not doubt that here, too, you have to deal with the same disease. Cancer of the uterus used often formerly to be confounded with simple polypus uteri; and Dupuytren and other Parisian surgeons have recorded instances of patients being sent to them from the country, who had been told by their own doctors that they must inevitably die of their complaint, and who were completely cured by the simple operation of the removal of the polypus, and were thus recovered, as it were, from the very brink of the grave. I have seen the same mistake frequently committed in our own times, and yet it is a mistake which care will always enable you to avoid.—*Med. Times and Gazette, Jan.* 15, 1859, *p.* 54.

122—ON INTRA-UTERINE POLYPI.

By Dr. A. K. Gardner, New York.

[As to the manner of separating tumours from the abdominal wall, the author observes that there are but three modes; viz., 1, by cutting instruments, as the scissors or bistoury; 2, by ligature; 3, by the ecraseur. The second is open to objection from the danger of pyæmia. The third is a compromise between the other two methods, but is open to the objection that it leaves a ragged and crushed surface, which will heal only by granulation.]

If the polypus be fibrous, and so large as to render such operations impracticable, the body is to be seized by hooks, hooked forceps, or even with an ordinary obstetric forceps, and forcibly drawn externally, and there subjected to such operation for its removal as may be deemed advisable. The force required for thus extracting an intra-uterine polypus is so great, as not only to displace the uterus from its situation, so as to bring it down upon the perineum, but it also necessarily inverts it, in order to expose the point where the pedicle unites with the uterus. Upon this pedicle being cut, the uterus often returns to its natural position with an audible snap, like that made by the hollow India-rubber ball of a breast-pump or syringe, when the pressure upon its sides is removed, and it suddenly resumes its natural position.

Sometimes, however, we find that the enlargement of the uterus is not due to a pediculated tumour which hangs from the uterus, but we have a fibrous tumour developed in the uterine stroma. Sometimes this is found to be immediately under the mucous membrane; but even if it is not so superficial, the best treatment—if any treatment is necessary—is to cut down upon it, making a deep incision through the mucous membrane of the uterine cavity, through any intervening tissue, down upon the tumour. It is better that the incision should penetrate the tumour than not to attain to it, for the fibrous tumour being bloodless, no ill result can ensue. We expect a double result from this incision. We take off the pressure—the cause of the hemorrhage—by cutting through and thus destroying the congeries of the enlarged and congested vessels enveloping the tumour; and secondly, we allow the tumour to enucleate itself, and thus to transform itself into a fibrous polypus, permitting, at some after period, an easy removal by any of the means already mentioned. It is to be hoped, that some subacute inflammation may take place in the tumour, resulting in a suppurative disorganization, for we know the marked tendency that fibrous tumours of the uterus have to degenerate when disturbed. This is especially noticeable in those tumours which accompany pregnancy, and which, when injured by the pressure of parturition, and perhaps deprived of some portion of their accustomed nutriment, by the arrest of the uterine circulation consequent upon the return of the uterus to its natural size, and the closure of the uterine sinuses, degenerate and slough away.

The secondary effect of this incision, even if not successful in the main purpose of effecting the enucleation of the tumour, is to diminish the hemorrhage, at least temporarily. That, attendant upon the operation, is easily controlled by the tampon or the colpeurynter.

It should be especially borne in mind that, in all intra-uterine operations, the season for their employment should be selected with particular reference to the usual period of menstruation. Even if the menstruation has been absent for several months, being arrested by debility, or perhaps confounded with the hemorrhages which have occurred irregularly, yet the *molimen menstruale* is still present, and might seriously affect the success of the operation.—*American Med. Monthly, July*, 1859, *p*. 48.

123.—*A Convenient Mode of Treating Vaginitis and Superficial Inflammation of the Cervix Uteri.*—M. **Foucher** mentions in the *Bulletin de Thérapeutique* of May 15, that, in the above affections he prefers ointments to injections. In simple vaginitis, he introduces every morning with the assistance of the speculum, a good-sized pledget of cotton wool, well smeared over with tannin ointment, into the vagina, bringing the pledget in contact with the cervix. By means of a thread tied to it, the wool can be removed by the patient, either in the evening or on the next morning. Every time the pledget is taken off, an injection of cold water, or of a solution of alum, should be used to wash the mucous membrane of the vagina. By a little practice, patients soon learn to introduce the pledget themselves, the surgeon then cauterizing the inflamed surfaces to hasten cauterization. M. Foucher uses the same treatment for fluor albus with much success ; but he tries, at the same time, to modify the morbidly disposed organism with the following pills: Extract of rhubarb, quinine, or extract of bark, steel reduced by hydrogen, of each half a drachm—for forty pills. To counteract constipation, the author has found half a grain of powdered belladonna, given every night in the form of pill, extremely useful.—*Lancet, June* 11, 1859, *p*. 591.

124.—ON PRURIGENOUS VULVITUS.

By Dr. J. Y. Simpson.

Not infrequently a prurigenous eruption appears on the mucous membrane of the vulva, and extends up along the vagina as far as the cervix uteri. It extends also often, and is sometimes, indeed, originally situated on the cutaneous border of the vulva, and appears on the outer cutaneous surface of the labium spreading backwards along the perineum, to the circle of the anus. Occasionally it is a flitting and transient affection, recurring with menstruation, pregnancy, or delivery. But patients will apply to you from time to time, in whom the disease has become more chronic and fixed, having lasted for weeks, or months, or even years ; producing almost constant irritation and distress ; frequently interfering with rest and sleep, and rendering the victims of it miserable and almost deranged. When the disease has become somewhat chronic and necessitates the patient to attempt to alleviate it by constant and sometimes rough friction, you will find the mucous and even the cutaneous surface at the most irritated parts white and thickened with red fissures, and scratches appearing on the affected part. I have spoken of the disease as fundamentally prurigenous; and we can often see on the affected surface a small papular, and sometimes a vesicular, or even aphthous eruption; but cases ever and anon occur, of severe pruritus in these parts, without your being able to trace in them any distinctive eruptive appearance.

Treatment.—Prurigenous disease of the vulva or neighbourhood can be relieved, and generally cured, by the assiduous and persevering application of a solution of biborate of soda (five or ten grains to the ounce of water), infusion of tobacco, either alone or containing a similar quantity of borax dissolved in it, or an ointment of iodide of lead (one drachm to the ounce), or an ointment of bismuth and morphia. Chloroform also applied locally, in the form of vapour, liniment, or ointment, forms one of the most certain means which you can use. The simplest way of employing it is by adding a drachm of chloroform to an ounce of any common or sedative ointment or liniment. You will find great advantage in the management of the disease in alternating some of these local applications with each other, for most of them begin to lose their good effects when persevered in above a few days consecutively. In the more obstinate and severe cases, strong astringents are sometimes of the greatest use, employed either alone or along with sedatives, as a very strong solution or ointment of alum or aluminated iron or tannin; or the powder of these substances mixed up with some powdered morphia, and applied continuously, for a few days, to the irritated part. Several times, in very obstinate cases, and where the disease was limited to a portion or circle of the cutaneous tissue, I have temporarily separated the affected portion of skin by a free subcutaneous incision with a tenotomy knife. Of course it leaves no wound except the small wound left by the entrance of the knife itself. I have found this little operation perfectly and entirely successful in some cases, and only of temporary benefit in others.

Perhaps it is unnecessary to add that the general health of the patient must be fully attended to, and that sometimes arsenic, aqua potassæ, and other alterative medicines of that description are required.—*Med. Times and Gazette, April* 16, 1859, *p.* 385.

125.—ON PRURITUS OF THE LABIA IN FEMALES.

By Dr. Edward Rigby.

[The following case illustrates what we frequently meet with in practice, and is difficult to relieve. Dr. Rigby makes no mention of the nitrate of silver in these tiresome complaints. We have found no remedy to equal it, but do not use it as is generally recommended in large, but *small* proportions. If you meet with one of these cases of violent itching, get a pretty large bushy camel's-hair pencil, and dip it in a solution of nitrate of silver (from two to five grains to the ounce), and push it well up to the os uteri: repeat this three or four times a day, keeping the patient as much horizontal as you can, and taking off all pressure from the hemorrhoidal veins by very gentle laxatives. A stronger solution, done seldom, does not answer so well as a weak one, done often. The patient must be taught to do it properly herself. Dr. Rigby's case is as follows:]

Mrs. A., aged 34, married eight years, never pregnant.—Feb. 23. Tall—brunette—pale—sallow. Constant heat, with sense of rawness, smarting, and intense itching of the vulva; much worse after a catamenial period: constant purulent discharge. Bowels confined. Tongue deeply sulcated, rough on the dorsum, with clean edges. Pulse very feeble. Skin clammy and moist; urine thick from lithate of ammonia. Headache both in the forehead and vertex; is of a rheumatic habit. Was suddenly seized four months ago, while standing at church, with great swelling, heat, redness of vulva, the labia becoming hard, and attended with intense itching.

Examination.—Nymphæ red and much excoriated; os uteri is directed forwards; the fundus is felt directed backwards, it is hard and swollen: introduced the sound, and replaced it easily.

Pil. hydr. extr. coloc. ext. hyosc. aa ℈i. M. ft. pil. xij. sumat ijh. s. pro. r. n.

Haust. ferri et magnesiæ sulph. primo mane.

℞. Acidi hydrochlor. dil. acidi nitrici dil. aa ʒi.; liquoris Taraxaci ℥j.; infusi cinchonæ oblongifol. ℥vii. M. ft. mistura sumat cochl. magna. ij. bis die ante cibum. ℞. Potassæ bicarb. ℈iv.; potassæ nitratis ℈ij.; sp. ammon. arom. ℥ss.; aquæ ℥vijss. M. ft. mistura sumat cochl. magna ij. post prandium.

16th. Better in every respect—the morning laxative does not appear to act comfortably. Rep. mist. cinchonæ ante, et mist. potassæ, post prandium. Let her take 2 of the pills just before the next catamenial period. ℞. Pil. rhœi comp. gr. v. h.s.

April 6. Catamenia appeared about ten days ago, rather suddenly, four days before the time, with much pain. The irritation was very severe for a time, and has continued slightly since. Bowels very confined even with the aid of medicine. Appetite better; general health fairly good; tongue still sulcated, though in other respects natural.

℞. Acidi nitrici dil., acidi hydrochlor. dil. aa ʒi.; liq. Taraxaci ℥i.; infusi sennæ comp. ʒiv.; infusi gentianæ co. ad ℥viii. M. ft. mistura sumat cochl. magna ij. bis die ante cibum. Rep. pil. p.r.n. Let her take a large enema of warm water occasionally.

℞. Sodæ biboratis, vini colchici, aa ℥i.; decocti papav. Oi. M. ft. lotio.

10th. Is in great suffering—says that the lotion appeared to excoriate her, and that the mixture had no effect upon the bowels. The enema, however, brought away a large accumulation of hardened fæces. Rep. enema p. r. n. Rep. mist. cinchonæ.

℞. Extr. aloes aquosi ℈ij.; extr. hyosc. ℈iss.; mastiches gr. xij. M. ft. pil. xx. sumat ij. h.s.

Liq. plumbi diacet ℥ss. decoc. papav. Oj. M. ft. lotio.

May 7. Better in every respect; her constipated bowels are the great difficulty.

The constipated and unhealthy bowels and other symptoms of general derangement of the digestive organs occurring in a rheumatic habit with considerable evidences of debility, as shown by the pale, sallow face, feeble pulse, and vertex headache, are the characters which point out the real features of this case. The circulation had probably been growing more and more unhealthy for some time; and if the cold which she caught by getting the lower extremities severely chilled, had not localised the mischief on the external generative organs in the form of pruritus, it is more than likely she would have suffered an attack of rheumatic or gastro-bilious fever.

The fiery heat and redness of the affected parts fully characterised the inflammatory nature of the attack. The congestion and irritation produced by the retroverted condition of the uterus probably still further increased the disposition of the complaint to localise itself in this form. The retroversion presented nothing peculiar or unusual; it was simply a feeble atonic uterus, which its fundus forced downwards and backwards under the weight of habitually constipated bowels. The alterative, laxative, and tonic plan of treatment quickly produced a great improvement in all her symptoms, and she continued nearly free from annoyance for some weeks, when the evil appeared to be woke up again into activity by the occurrence of a catamenial period. I cannot help thinking that she had unconsciously lapsed again into some degree of her former habits of constipation and gastro-biliary derangement; it was evidently a matter of no slight difficulty to overcome her long-standing disposition to constipation; and the alteration which I made in the mixture appeared to fail entirely; nevertheless, she continued to improve steadily in general health; and the occasional use of a large warm-water enema effectually prevented any intestinal accumulations. The capriciousness of local remedies in this affection is well known; but I was much surprised at the entire want of success of the lotion of borax and colchicum wine in poppy decoction, which, in a somewhat similar case, had afforded the greatest relief.—*Med. Times and Gazette, Jan.* 1, 1859, *p.* 4.

126.—*Arsenic in Pruritus Vulvæ.*—M. Imbert Goubeyre relates a case of obstinate pruritus occurring early in pregnancy, and which had resisted the most varied means, yielding rapidly to minute doses of Fowler's solution of arsenic.—*Gazette Hebd., Jan.* 28.—*Med. Times and Gazette, Feb.* 19, 1859, *p.* 188.

127.—A CASE OF PROLAPSUS UTERI, CURED WITHOUT OPERATION, OR THE NECESSITY OF WEARING A PESSARY.

By Wm. E. C. Nourse, Esq., Brighton.

A single woman, aged about sixty-five, applied to me, in February, 1858, on account of prolapsus uteri, from which she had suffered for three years. She looked haggard and worn, had been losing flesh, strength, and appetite for many months, and complained much of depressed spirits, dragging pain in the loins, and inability for exertion. The womb was generally down, though she could reduce it. I found it hanging between the thighs, about the size of a melon, the mucous membrane dry and glazed, of a reddish-brown colour, and superficially ulcerated. The orifice of the urethra occupied the usual position in front of the tumour, and the os uteri was seen at lowest part of it. The womb being replaced, was found very movable, and the vagina much relaxed and enlarged.

Considering the success which had attended operations for the cure of prolapsus by partial occlusion of the vagina, I determined to see if I could not apply the principle of these operations in a simpler way. I therefore directed that, now that the womb was replaced, it should never again be allowed to come down, even for a single moment; that a sort of thick pad, or cushion. of a length and breadth sufficient to cover completely the external parts, should be applied, and be kept in its place by a broad and firm T-bandage before she again rose from the recumbent posture; that she should prepare a sufficient number of these pads and T-bandages, should always put one on before she rose from her bed in the morning. just as a ruptured person puts on a truss, and should never go about without one; and, lastly, that she should, introduce, every night into the vagina a few grains of tannic acid, made up into a sort of soft pill.

She steadily followed the plan proposed, and at the end of two months had gained strength and flesh, and got rid of all her discomfort. The tannic acid was now used twice a-week only, but the pad was directed to be worn constantly. She still continues its use, and reports that the womb has never once come down, and that she has quite recovered her health and spirits, can walk out, and attend to her household duties, quite as well as before she became subject to the affection.—*Lancet, Jan.* 22, 1859, *p.* 81.

128.—EXTIRPATION OF AN INVERTED UTERUS BY ECCRASEMENT.

By Dr. A. H. M'Clintock, one of the Presidents of the Dublin Obstetrical Society, Master of the Lying-in Hospital, &c.

[The case was that of a woman of two-and-twenty, tall, thin, and of anæmic aspect: for upwards of twelve months subject to profuse metrorrhagia. A pedunculated tumour was found low down in the vagina, its neck being embraced but not constricted by the thin os uteri. It was of a dark pink colour, and when scratched or abraded, blood was discharged. So far the case possessed all the characters of a polypus, but the history excited strong suspicion that the case was one of chronic inversion of the womb, as her symptoms dated back to her last confinement fourteen months ago, when under the care of a rude country midwife, and who probably used much violence in removing the placenta, as she says the cord was broken in the attempt, and the after-birth "twice slipped away from the nurse."]

Intending to remove the tumour with the écraseur should it prove to be a polypus, I drew it down beyond the labia by means of a vulsellum. This was effected without force, and with little annoyance to the patient. On passing the finger along the neck of the now extruded tumour, I found that the os was entirely effaced—not a trace of it could be detected; and that the vagina was perfectly continuous with the neck of the tumour at every point of its circumference, without any intervening ridge or projection, such as the os uteri would occasion. On replacing the tumour within the vagina, the os uteri again became developed, and could be distinctly felt in its former position. A bougie passed within it only a short distance—not quite half an inch—and was arrested at its height all round. The result of these explorations led to the conclusion that there existed a chronic inversion of the womb. This opinion was subsequently concurred in by Dr. Johnson and Dr. Churchill, who were good enough, at my request, to see the case. On examining high up within the rectum (as suggested by Dr. Arnott), the uterus could be surmounted by the finger—at least, so thought Dr. Churchill and myself. Had this manœuvre been practiced when the uterus was external, the indication would, doubtless, have been much more clear and satisfactory.

The case seemed in every way, as appeared to us, a favourable one for attempting manual reposition. The os uteri was relaxed, the inversion incomplete, the uterus itself small, and its substance admitted of being easily indented with the finger. Accordingly, on three different occasions—viz., on the 4th, 8th, and 13th October—the taxis was tried. In each of these attempts I endeavored to effect the reduction of the misplaced organ in the following manner: The patient, having been chloroformed, was placed on her back at the edge of a bed, with her legs drawn up and separated. The hand was introduced into the vagina, and firm compression and upward were simultaneously made upon the body of the uterus. This was a very fatiguing piece of manipulation, the hand very soon getting benumbed and powerless. During the brief intervals of relaxation, to allow the hand to recover its power, a steady pressure was maintained against the lowest part of the uterus, by means of a round piece of wood about eight inches long, with a shallow, cup-shaped extremity. The uterus could be very distinctly felt through the abdominal parietes, and my other hand was thus able to co-operate, by making regulated pressure above the pubes. The only effect upon the uterus of each of these operations was, for the time being, to reduce the size of the fundus and body; and this diminution seemed attributable rather to the simple effect of squeezing and compressing the organ, than to any replacement of its inverted portion.

On all these occasions I adopted the course of proceeding described by Professor White, of Buffalo, U. S. the report of whose surprisingly successful cases had just then reached me. On the evening previous to the first operation I applied extract of belladonna very freely to the os uteri, and some hours later she got an enema of starch and laudanum. This (first, operation lasted fifteen minutes. The patient exhibited such marked symptoms of prostration—pallor, coldness of the surface, failure of the pulse, vomiting, involuntary discharge of fæces, &c.—that it was deemed imprudent any longer to persevere. Drs. Churchill and Kidd were present during the operation, and Dr. Byrne, one of the assistants, who administered the chloroform on this and the two other occasions. The second and third attempts at reduction respectively occupied twenty-five minutes, and each time I had to desist in consequence of the supervention of a state of prostration bordering on collapse. This did not quite pass away for some hours, but probably would not have lasted so long, if the patient had not obstinately refused to take any wine or brandy. Whether this alarming depression was in any way ascribable to the chloroform, I cannot take upon me to say. She certainly took a strong aversion to its inhalation, and it was with very great difficulty she could be induced to breathe it on the last occasion. In the intervals between the operations, the instrument already described was used night and morning for some minutes in pressing up the uterus. Beyond the temporary weakness just mentioned, and the occurrence of some hemorrhage, no ill effects, strange to say, followed these manipulations.

Only one alternative now remained, and that was the removal of the misplaced portion of the womb. To effect this, there was a choice between the ligature, the knife or scissors, and the écraseur. I resolved to employ the first and the last; but was not quite decided how long to leave on the ligature before performing écrasement, whether for twenty-four or forty-eight hours. In conferring with Dr. Johnson upon this point, his opinion was, not to remove the ligature under forty-eight hours at least. This sound advice was strictly followed.

At morning visit of October 20, I passed a ligature of silk fishing-line, prepared in oil, round the neck of the uterus, by means of a Gooch's canula; and having drawn it moderately tight, fastened it. The top of the canula was just within the os uteri. Very considerable pain was complained of when this was done, and in the course of two hours it much increased, and she presented symptoms of general depression, with vomiting, By the aid of warmth and stimulants, reaction was brought about. A grain of morphia was given in pill, soon after which the pain began to abate. At 7 p. m. the pulse was 80, and she was quite free from pain or tenderness. The ligature was tightened, and she got half a grain of morphia and two of dried carbonate of soda. The next day she was going on satisfactorily, the pulse being quiet and no pain present; but she had at times some sickness of stomach, and at midday there was a slight return of the faintness. The ligature was drawn a little tighter. The uterus, as seen through the speculum, presents a dead white colour, having quite lost the deep pink which it presented before the operation. At forenoon visit of the 22nd the only change in her condition worth mentioning was, that the discharge from the vagina, though very small in quantity, was decidedly, and for the first time, fetid. This shewed that strangulation had taken place, which might have been doubted from the colour of the uterus. The ligature had now been on for forty-eight hours; and, preparatory to using the écraseur, we urged her to inhale chloroform, but this she positively refused to do, declaring she would in preference suffer any amount of pain. Her endurance, however, was not put to a severe test. Drs. Churchill, Denham, and Byrne being present and assisting me. I very gently drew down the uterus with a vulsellum, until it appeared between the labia—just enough, in fact, to permit of our carrying the chain of the écraseur around it, and no more. She lay on the left side, with the thighs well flexed, and the handle of the instrument was directed backwards. The chain having been securely lodged in the sulcus made by the ligature, this and the canula were withdrawn. The écraseur (one of Charrière's) was worked very slowly, and eight minutes passed over before the uterus was severed. Pain of a severe, but not of a sharp or an acute kind, attended, but no hemorrhage of any amount. It was thought prudent to abstain from making any examination, and even from syringing the vagina. She was enjoined to observe the strictest bodily quietude, and one grain of acetate of morphia was given to her. To prevent even the slight exertion attendant upon voiding the urine, this was drawn off with the catheter for the next few days. At 2 o'clock she was easy; pulse 96: no hemorrhage. At 7 p. m. the pulse was 120; she complained of pain, and there was considerable tenderness in the hypogastric region; there had been no rigor, however. A turpentine epithem was applied to the belly, and she was ordered half a grain of calomel and the same of opium every third hour. She passed the night tranquilly, and in the morning (23rd) the pulse had fallen to 104, the pain had nearly ceased, and there only remained some tenderness and some fulness above the pubes. There was a little discharge of a serous nature from the vagina. Her pills were continued for to-day, and she was allowed some cold chicken broth, as the stomach was irritable. Her pulse at visit next morning (24th) was down to 88, and, except for the presence of some sickness of the stomach, she was in every respect improved. This gradually subsided. In the course of a few days all hypogastric tenderness and fulness were entirely removed. She was not permitted to get up for a fortnight, and she returned home the 2nd December, having greatly improved in health and strength since the operation.

This case very strikingly exhibits the necessity for caution and strict investigation in the diagnosis of uterine polypi. In *nine* of the cases collected by Dr. Forbes, the inverted uterus was mistaken for polypus. I do not believe that the ordinary digital or ocular examination would, in the instance before us, have suggested, even to the most skilful and experienced, the remotest suspicion of its true nature. Had the patient, from any motive, misstated the history of her case, or suppressed the fact of her having had a child, a grave error of diagnosis, and perhaps of practice, might have been committed. It is manifest also, from this and other cases which might be cited, that the characters of the tumour, as to size, colour, sensibility, and density, can very rarely be depended on to furnish positive information of its nature.

It has been stated in the foregoing history that when the uterus was drawn external, every trace of the os tincæ was obliterated, but reappeared when the organ was put back. That this actually took place was not only plain to myself, but to others who examined the patient, and among their number Dr. Johnson and Dr. Churchill. This circumstance is one worthy of notice, as bearing on diagnosis, although I am not aware that it has attracted attention before. I could not venture to say whether this sign is present in every case or not; but when it is present, we may safely regard it, I think, as a very unequivocal indication of the uterus being inverted.—*Dublin Quarterly Journal, Feb.* 1859, *p.* 137.

129.—ON OVARIOTOMY: WITH REMARKS ON THE MEANS OF DIMINISHING THE MORTALITY AFTER THE OPERATION.

By T. Spencer Wells, Esq., Lecturer on Surgery at the Grosvenor-place School of Medicine, Surgeon to the Samaritan Hospital, &c.

[The writer has long regarded the ligature on the peduncle, and the sloughing of the stump within the abdominal cavity, as one of the most frequent causes of death after ovariotomy.]

In cases where the peduncle is long, this danger can be avoided by fixing the stump outside the wound; but where the peduncle is short, the écraseur offers evident advantages.

[Eight cases are detailed, which, though interesting, are too long for insertion here. Mr. Wells then proceeds to observe that we shall most effectually diminish the mortality after ovariotomy]

1. By the selection of proper cases only for operation.
2. By the determination of the stage of the disease in which the operation is most likely to prove successful.
3. By careful preparations to avoid all unnecessary sources of danger.
4. By the use of anæsthetics to lessen the risk of *shock*.
5. By certain precautions in the performance of the operation. And
6. By careful after-treatment.

1. As to the selection of cases. Without entering into the wide question of the diagnosis of ovarian tumours, it may not be quite unnecessary to say that the surgeon should be quite sure the tumour is ovarian before he determines to perform ovariotomy, when we remember that this operation has been attempted in cases of pregnancy, of hydatid tumours of the liver, of colloid disease of the peritoneum, of other malignant growths within the abdomen, of fibrous tumours of the uterus, of excessive fatty deposit in the integuments, of ascites, and in cases where there was no tumour whatever. The more glaring of these errors are not likely to be committed again; but

with all our care there are cases in which doubts can only be entirely removed by an exploratory incision.

Then as to the characters of the tumour, I have no doubt that the surgeon who would only operate on cysts containing fluid which could easily be emptied, and if non-adherent, withdrawn through a very small incision, would be more successful than another surgeon who removed solid tumours requiring the long incision. But the cases I have related, and very many others on record, show that the presence of extensive adhesions and large solid masses are quite consistent with a successful result.

The cases in which the operation certainly ought not to be performed are those in which there is coexisting disease of some important organ. If all the patients who had died after ovariotomy from old standing diseases of the lungs or kidneys were eliminated from the catalogue, the results of the operation would appear in a far more favourable light. The operation is discredited because it has been performed in ill-selected cases.

There are those who believe that the existence of adhesions, so far from contra-indicating the operation, are rather favourable than otherwise, inasmuch as the peritoneum, in its altered character, is not so likely to take on an inflammatory action as when it is in a tolerably healthy condition. Many facts might be cited in favour of this view, and more against it; and it is one deserving of further inquiry.

2. As to the period for operation. There are surgeons who would only operate as a forlorn hope, when it is quite clear that the patient's days are numbered, and that if her life be shortened by the operation she cannot lose very much. Some, on the contrary, argue that this plan of operating *in extremis* has brought, and does bring, the operation into undeserved discredit; and that the less the general health of the patient has been shattered by the disease the more likely is she to survive a severe operation. To this it is replied that ovariotomy is an exception to the general rule, and that the most successful cases are those in which the patient has been most reduced by the disease, or the peritoneum most altered by extension. It is remarkable that in all the successful cases I have related, the disease was in a very advanced stage, while in the first fatal case it was in a much earlier period of development, and the general health comparatively little injured; but this point again requires more extended inquiry.

3. Patients have been exposed to unnecessary danger by being submitted to operation in a theatre, or in cold rooms, or in dry overheated rooms; or their limbs have been imperfectly covered and not protected from the fluid emptied from the cyst even after it has ceased. I have seen a patient's legs, thighs, and buttocks perfectly cold and wet from having been exposed for nearly an hour, uncovered, and lying on a sheet soaked with fluid. All this has been sufficiently insisted upon by Dr. Clay, Dr. F. Bird, and others, to render more than an allusion unnecessary.

4. *The use of Anæsthetics.*—The materials do not exist for a trustworthy comparison of the results of ovariotomy in cases where ether or chloroform were and were not administered. This is another question which can only be settled by future and more extended observation; but I firmly believe that th mortality *must be* diminished by the use of anæsthetics. The sickness which sometimes follows the use of chloroform is, of course, a great disadvantage, and the depressing effects might, in some cases, be too great; but, on the whole, I think that by greatly lessening the anxious apprehension of suffering which so often brings patients into an unfavourable condition for undergoing any operation, and by entirely preventing the shock at the time of the operation caused by the pain, by the knowledge that the abdomen was laid open, and by the manipulations of the operator and assistants, the good effects of anæsthetics must far outweigh any occasional ill-consequences.

5. Supposing that all the necessary preparatory measures have been taken—a quiet, well-ventilated room secured, a good nurse provided, a couch and mattress placed in a good light, the room warmed to about 70°, and the air kept from being too dry by a kettle of water boiling on the fire; the

patient dressed in a warm flannel gown, and the legs kept warm by woollen stockings and hot-water cans, if needful; a good supply of soft, new, well-scalded sponge at hand; plenty of water at about 96°; soft flannel moistened in this water to protect any exposed intestines; all the necessary instruments for incision, tapping, securing vessels and the peduncle, and uniting the wound; an abdominal bandage; and a clean warm bed ready to receive the patient; with chloroform, ether, wine, and brandy;—there are still some points of practical importance which deserve notice.

[It is extremely important that the patient should not be exposed to any possible risk of putrid infection from students or surgeons engaged in the dissecting rooms.]

The *incision* should be exactly in the median line, so that integument and aponeurosis only need be divided before arriving at the peritoneum; it should not reach sufficiently near the pubes to endanger the gravitation of serum into the areolar tissue around the bladder, and that of the pelvis generally, still less to endanger the bladder. Without running this risk, sufficient space can be often obtained below the umbilicus. If more be required, the incision can be carried upwards towards the ensiform cartilage, still in the median line, but with a curve round the umbilicus.

In case of any doubt as to the exact boundary of the cyst, it is far better to open it than to run any risk of unnecessarily detaching the peritoneum from the abdominal walls.

In cases where the cyst contains fluid it is well to use a large trocar, in order to save time; and to have an elastic tube attached to the canula, so that the fluid may be removed without wetting the patient, and while the abdomen is still covered.

This modification of the trocar is of some importance. Tapping the abdomen with an ordinary trocar is a very clumsy proceeding. It is necessary to have basins held up to the canula, and these, as they are filled, are emptied into pails, amid much splashing, and unnecessary exposure and wetting of the patient. In all cases of tapping the chest or abdomen, or opening large abscesses, I have, for several years past, been in the habit of using the canula of Schuh, of Vienna, and attaching an elastic tube to it, thus conveying the fluid away quietly and neatly without alarming the patient, or wetting her linen, while the entrance of air into the space occupied by the fluid evacuated is rendered impossible. More recently I have used a still simpler and equally convenient instrument, first described by Mr. Charles Thompson, of Westerham, in the Medical Times and Gazette of March, 27, 1858.

The trocar is made to fit the canula closely, like the piston of a syringe; there is an opening from the lower part of the canula, on to which an elastic tube can be fastened; I have added a stop-cock to the elastic tube, a few inches from the canula, in order that the rate at which the fluid flows off may be regulated, should the patient suffer from its too rapid evacuation.

In ovariotomy the entrance of air into the sac is of no importance, and the mode of using this instrument is apparent; but in simple tapping of ovarian cysts, as in evacuating fluid from the pleural or peritoneal cavities, or from the sac of large abscesses, it is of vital importance to prevent the entrance of air, and this may be done by drawing the piston back, placing the end of the elastic tube in a basin of water, and withdrawing the air from the tube and canula by suction, or by a syringe fitting the end of the canula. If the end of the tube be kept under water, and the piston be pushed forward, ready for use, the elastic tube is kept filled with water, which meets the fluid from the cyst when the operation has been commenced, and a syphon is formed. I think few surgeons who have once used this instrument, will ever use an ordinary trocar again for tapping either chest or abdomen.

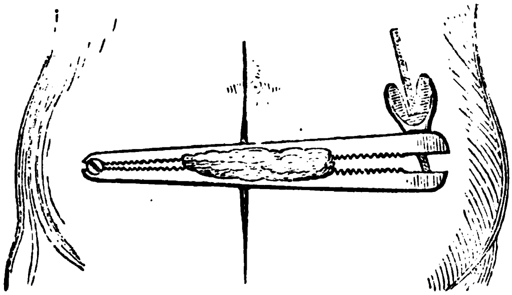
In breaking down adhesions it is far safer to use the hand than the knife. Should any portion of adherent omentum or mesentery contain large vessels, these should be tied or twisted at once, to prevent bleeding into the peritoneal cavity, and as it might be very difficult to find the bleeding point if once separated. In case of firm, tough adhesions, the écraseur might prove very useful. Should there be adhesion to the intestines, or any viscus so firm that separation is very difficult, it might be better to cut away that portion of the cyst, leaving it adherent, than to run any risk of injuring an important organ; but in this case it would be desirable to remove the inner secreting coat of the cyst, if possible.

In drawing out the cyst caution is required that the cyst only is drawn out; and, in removing a solid tumour, care is necessary that it does not fall suddenly and tear through any portion of the peduncle, thus giving rise to hemorrhage.

In securing the peduncle ligatures were used in the first and second cases, clamps in all the others; but in the third and fourth these ligatures also became necessary, as in each case a portion of peduncle slipped from the grasp of the clamp. This danger, however, was avoided in the other cases, by taking care not to cut the cyst or tumour too close to the clamp, but leaving at least an inch projecting beyond the line of constriction. If ligatures be used, it should be remembered that a single one can very rarely be sufficient; and that cases are on record where patients have died on the table from a single ligature slipping, and it being afterwards impossible to find the bleeding vessels. The peduncle should be carefully transfixed, and tied in portions not broader than a finger. Perhaps it might be well to divide the round ligament with a knife, only tying the remainder of the peduncle; and I think it extremely probable that the écraseur may completely do away both with the ligature and the clamp; as it might be used very slowly, and the peduncle not entirely separated for some hours after the wound was closed.

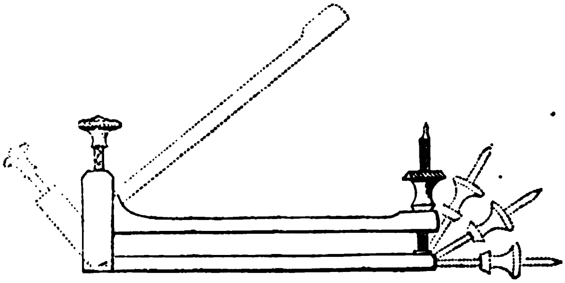
The annexed wood-cut shows the manner in which the peduncle is embraced by the clamp, which lies across the wound, and prevents the sloughing stump from slipping into the abdominal cavity. In the drawing this instrument is half the real size, so that the body, to be in proportion, should be half the life size. I used this clamp in Case 2, having it made like Ricord's fenestrated forceps for circumcision, with a screw for fixing. But it was not easy to apply, owing to the fixed stem; and as it was made of steel the rust which formed on it caused some irritation of the skin around the wound, although it was protected by lint. I therefore had an instrument made with a movable stem, and with two screws instead of one, in order to insure a more equable pressure on all parts of the peduncle; and it was strongly gilded to prevent rusting. This clamp is represented in the wood-cut (Fig. 2), the dotted lines showing the manner in which the blade and stem are moved. I used this instrument in Case 3, and I have since had the screws made flat instead of round, as, when the surgeon's hands are bloody, it is not easy to turn the round screw. In Cases 6 and 7, I used the calliper clamp, as originally devised by Mr. Hutchinson; but in Case 8 I returned to my own, and believe it to be that which is most readily applied, which makes the most equable pressure on the peduncle, and lies the most easily, of any I have seen, upon the wound afterwards. The secret of safety from hemorr-

Fig. 1.



hage, in using any clamp, is to leave a sufficient amount of tissue projecting to give security against retraction of divided tissue, or slipping of the clamp,—in other words, not to cut away the tumour too close to the clamp.

Fig. 2.



By carefully cleansing the peritoneum from all blood, clotted or otherwise, or any of the contents of the cysts which may have escaped, I think far more good is done by removing matter which would subsequently decompose, than harm by any irritation of the peritoneum with soft sponges. In this and previous stages of the operation the intestines should be kept as much as possible from protruding, by an assistant holding the edges of the wound together; and when intestine is exposed it should be protected by the warm moist flannel. Artificial serum has been used for this purpose instead of plain water, but I should certainly prefer the latter.

In cases where it can be done without undue traction upon the uterus, the fixing the stump of the peduncle outside the peritoneal cavity appears to be a most important safeguard against peritonitis, and the low fever caused by absorption of the putrid matter flowing from the decomposing stump. Any one who sees the quantity of filthy sanies surrounding the stump as it lies on the abdomen of a patient day after day before it separates, must be struck with the danger attendant on such a process going on within the abdomen, even though this diffusion were checked by protecting effusion of lymph. It remains for a statistical inquiry to show how far mortality has been diminished in cases where the new plan (first recommended, I believe, by Mr. Duffin) was adopted.

For bringing the edges of the wound together I think gilded hare-lip pins—or pins of the *passive* iron-wire introduced by Dr. Simpson—with the twisted suture, are the most trustworthy. The whole depth of the wound is brought together, and the pins act as splints, preventing any escape of intestine, an accident which has happened by the giving way of ordinary sutures. A few superficial sutures of silver, or iron-wire, may also be advisable to secure perfect apposition of the divided edges of skin. I have found cotton-wool the most comfortable covering for the wound; and as an abdominal belt, nothing answers better than a double fold of flannel, kept from slipping upwards by a strip of linen passing round each thigh, and pinned to the lower edges of the flannel. Experiments on dogs, rabbits, and guinea-pigs, have convinced me that it is better to perforate the divided edges of the peritoneum by the hare-lip pins, so as to press two folds of the peritoneum against each other, than to run any risk of contact of the peritoneum with the divided tissues of the abdominal parietes. But as I intend to make these experiments the subject of a special communication to one of our learned societies, I merely state the result here for the information of other operators.

6. On careful nursing and judicious after-treatment a great share of success must depend. In many of the cases of ovariotomy reported, it seems perfectly wonderful that the patient has survived the after-treatment. Bleeding, leeches, blisters, calomel and purgatives, and starvation, have often been

prescribed. Who can wonder at the death of the patient? The wonder is that so many have survived all this. The great objects appear to me to be to keep the patient perfectly quiet, free from pain, and thoroughly clean; the surgeon should not be led by the fact of the pulse being very rapid to fear peritonitis, and adopt active measures. It has seemed to me that the rapidity of the pulse is owing, in a great measure, to the return of the heart to its normal situation; but whether this be so or not it is certainly no ground for active measures. With the exception of opium, given with the precautions I have already indicated, I am disposed to attach far more importance to hygienic measures than to medicinal treatment. A well-ventilated room, warm, but not unpleasantly so to the patient, the air comfortably moist, and perfect quiet, seem to me to be the chief requisites. With the object of maintaining rest, the patient should not get up to pass water, but the catheter should be used every six hours. One good effect of the opium is to keep the bowels quiet, but if there be much flatulence I do not think it is well to keep up the state of constipation too long. I have never seen any good done by O'Beirne's tube in relieving flatulence. A little soda in peppermint water, or chloric ether with aromatic confection, seems to give as much comfort as anything; but this flatulence is always a troublesome symptom for some days. The intestines seem to miss their accustomed support or pressure. The application of warmth and moisture to the abdomen, by means of a warm linseed-meal poultice, is particularly grateful to the patient, and I feel disposed to regard it as of considerable importance in the after-treatment. As to food, I have not been disposed to urge it until there was a decided appetite, but to leave beef-tea, arrow-root, with or without wine or brandy rice-water, and barley-water, or a little tea and dry toast, with the nurse, to be given very much as the patient wished for them, and, after a few days, some solid food. The fœtid stump I have kept covered by a muslin bag of peat-charcoal, changed once or twice a-day, in order that the patient might not be nauseated by the fœtid odour which would otherwise proceed from it. I have not disturbed the wound until union was perfect: and if gilded pins or metallic sutures be used they may be left with safety for many days; though I believe the best plan is to remove the pins on the third or fourth day, leaving the superficial sutures three or four days longer. So soon as the patient is able to get about pretty well, country air is the best restorative.

It is said that by palliative treatment, by tapping, and by the injection of iodine, we have other means of relieving patients of ovarian disease, whereas the removal of the stone is the only means of relieving the calculous patient. I reply, that palliative treatment is of little avail—that tapping is in itself dangerous, even in the few cases to which it is applicable—and that, although injection of iodine is proved to be occasionally successful in those rare cases when the cyst is unilocular, yet its effects, even in these cases, are uncertain, and sometimes deadly.

An objection of far greater weight is the oft-repeated fact, that patients may live many years with ovarian disease, and die at last at a good old age. I answer, that it is no comfort to one woman dying of ovarian disease, to tell her that another woman has lived for fifty years with a disease somewhat similar. We do not propose to perform ovariotomy in a case where a woman, if left alone, would probably live in tolerable comfort for several years. We let *her* alone, and do the little we can to increase her comfort. The cases in which we incur the heavy responsibility of performing a dangerous operation, are those in which the patient *must* take her choice on the one hand, between the risk attendant on such an operation, with a hope of perfect cure if all go well; and. on the other, a life of suffering, to be terminated, at no distant date, by a miserable death. For in most cases it is a life of suffering, and great and constant suffering, mental and bodily; and death from the natural termination of the disease is one of the most trying we ever witness. All must acknowledge the truthfulness of the following picture, drawn by one of the ablest and most logical of the general opponents of ovariotomy. In the last edition of his lectures on the diseases of women, Dr. West, after describing a variety of painful symptoms under which women suffer in the earlier stages of ovarian disease, says (page 527), that as the end approaches we have "symptoms of the same kind as we see towards the close of every lingering disease, betokening the gradual failure, first of one power, then of another: the flickering of the taper, which as all can see, must soon go out. The appetite becomes more and more capricious and at last no ingenuity of culinary skill can tempt it, while digestion fails even more rapidly, and the wasting body tells but too plainly how the little food nourishes still less and less. The pulse grows feebler, and the strength diminishes every day, and one by one each customary exertion is abandoned. At first the efforts made for the sake of the change which the sick so crave for are given up; then those for cleanliness; and lastly, those for comfort—till at length one position is maintained all day long in spite of the cracking of the tender skin, it sufficing for the patient if in that, respiration can go on quietly, and she can suffer undisturbed. Weariness drives away sleep, or sleep brings no refreshing. The mind alone, amid the general decay, remains undisturbed; but it is not cheered by those illusory hopes which gild, though with a false brightness, the decline of the consumptive; for step by step death is felt to be advancing; the patient watches his approach as keenly as we, often with acuter perception of his nearness. We come to the sick chamber day by day to be idle spectators of a sad ceremony, and leave it humbled by the consciousness of the narrow limits which circumscribe the resources of our art."

This picture drawn by Dr. West is true. We have all seen the poor creatures he so eloquently describes fading hopelessly away. But the resources of our art are not so limited as he would imply. We may be something more than idle spectators of a death-bed. We have a resource to offer—hazardous it is true—but one which has in many cases been crowned by a complete and brilliant success.

Some months ago I stood at one of these death-beds, in consultation with one of our highest authorities on ovarian disease. He was half convinced by my arguments in favour of an operation, but he felt his responsibility deeply, and said, "How dare I advise an operation we both know to be so dangerous?" I replied, "How dare you leave the poor woman to die without an effort to save her?"

These are the anxious doubts and grave responsibilities ever recurring in the life of practitioners of medicine and surgery. My earnest hope is, that by bringing this subject prominently forward for discussion, something may be done to solve the doubts and lighten the responsibility of those who seek for the "influence of authority in matters of opinion."—*Dublin Quarterly Journal, Nov.* 1859, *p.* 257.

130.—UNILOCULAR OVARIAN CYST—INJECTION WITH IODINE —SUCCESSFUL RESULT.

(Under the care of Dr. SAVAGE and SPENCER WELLS, Esq., at the Samaritan Hospital.)

[M. A. G., aged 56, widow, was at first an out-patient under the care of Dr. Priestley, who diagnosed a unilocular ovarian cyst, and reported the case as a very favourable one for injection of iodine. The tumour had apparently existed about two years.]

State on admission.—The whole abdomen from the pubes to about half way between umbilicus and ensiform cartilage was occupied by an elastic fluctuating smooth tumour, dull on percussion, the patient being about the size of a woman in the eighth month of pregnancy. Percussion clear in both lumbar and lateral lumbar regions. Uterus healthy. No depression of anterior wall of vagina. The pulse is small and weak, and the patient suffers a good deal from pain, and from the weight of the tumour and its interference with respiration.

A consultation was held, and it was agreed that Mr. Wells should tap the cyst, and if it proved to be unilocular inject it with iodine; while if it were multilocular he should perform ovariotomy.

April 12. As fluctuation was more distinct above than below the umbilicus, Mr. Wells inserted the trocar in the medium line, about an inch above the umbilicus, using Dr. Graily Hewitt's canula fitted with an exploring sound passing through a diaphragm of india-rubber, so that the interior of the cyst could be explored without permitting the escape of fluid. The extremity of the sound was passed round all over the interior of the cyst; and as it appeared quite smooth and no smaller ingrowing cysts could be detected, the sound was withdrawn, and an elastic catheter introduced through the catheter, by which thirteen pints and a-half of clear straw-coloured fluid were evacuated. When the cyst was quite empty, an ounce of a caustic solution of iodine was injected through the catheter by means of a glass syringe with platinum nozzle, and the catheter and canula withdrawn, leaving the iodine in the cyst. One or two drops of iodine solution followed the canula.

Mr. Wells stated that in most of the cases recorded in which iodine had been injected into ovarian cysts, some 4, 6, or eight ounces of the strong tincture had been used; and he attributed many of the subsequent ill-effects to the rapid absorption of so large a dose of alcohol. In two cases he had seen himself, he believed the patients suffered from the alcohol, and not from iodine, and he determined to follow the example set by Mr. Hutchinson recorded in the Medical Times and Gazette (vol. xvi. p. 602), and use an aqueous solution sufficiently strong to cauterise the secreting lining-membrane of the cyst, allowing for some unavoidable dilution by the fluid necessarily remaining in the cyst after it has been emptied as completely as possible. The solution used was made by dissolving a scruple of iodine and half a drachm of iodide of potassium in an ounce of water.

Not much pain was felt at the time, but it became very severe in about an hour all over the umbilical region. Two grains of solid opium were given at once, and a grain was ordered every four hours if the pain continued. She had a distinct rigor at night, followed by heat and perspiration.

[The report states that improvement took place, till at the end of three weeks she begged to sit up. Wine and quinine were daily administered. The cyst could be felt about the size of a cricket ball, lying centrally between the umbilicus and pubic.]

Remarks.—Since M. Boinet first practised the injection of ovarian cysts with iodine, the operation has been performed very frequently, both in the country and on the continent, and very considerable success has resulted. But there have been many deaths and many failures. The result of all this experience is leading us to the conclusion that failure *must* follow the injection of compound cysts, and that death *may be*, and often *is*, the result of a practice which is certainly forbidden by our knowledge of the structure and growth of these cysts. On the other hand, it appears that in all the successful cases the cyst was simple or unilocular, and that the success has been very considerable. Thus, of 45 cases reported by M. Boinet, in 34 the cysts were simple. Of these 31 were cured and three died; while of 11 compound cysts so injected there were five failures and 6 deaths. Nothing can show more strongly the importance of a correct diagnosis; and Dr. Graily Hewitt's exploring trocar thus becomes of great use as a guide to successful treatment.

The apparatus necessarily consists of an ordinary canula and trocar, and a probe fourteen inches or more in length, graduated in inches, rounded at its extremity, and composed of a flexible metal. In the shoulder of the canula is transfixed a diaphragm of india-rubber, in the centre of which is a perforation, large enough to allow the trocar or the sound to pass, but grasping the sound so tightly as to prevent escape of fluid from the cavity into which it is introduced.

The trocar and canula are thrust into the tumour, as in the operation of paracentesis; the trocar is withdrawn, and the sound introduced in its place. The observer is now in a position leisurely, carefully, and conveniently to probe, by means of the sound, the whole of the abdominal cavity or cyst, and no fluid escapes during this examination.

Latterly a branch pipe has been added to the canula, fitted to an elastic tube or stopcock, so that the fluid may flow, if desired, while the sound is being used to explore the sac at different degrees of distension.

It may be well to add a few words as to the success of iodine injection to the practice of those who have followed M. Boinet. Dr. Simpson was the first to adopt it, and he is said to have done so in many cases with great success, but he has not published any numerical statement of the results. Dr. West has tried it in eight cases. Not one of these was fatal. In two there was no benefit derived. In one there was only slight improvement. In four the disease was clearly retarded, and in one it was cured, as at the end of two years fluid had not re-collected. In our fourteenth volume will be found a full abstract of a discussion on this subject at the French Academy of Medicine, where M. Velpeau stated that out of 139 instances in which iodine had been injected, 30 deaths had occurred; 64 were permanent cures; and in 36 the fluid re-accumulated. But it should be remembered that in 20 of these fatal cases, the opening into the cyst had been maintained by a tube kept through the wound; a detestable practice, leading almost certainly to cyst inflammation.—*Med. Times and Gazette, May* 28, 1859, *p.* 5498.

131.—ON OVARIOTOMY.

By J. Baker Brown, Esq.

[The following remarks were made by Mr. Brown after a paper read at the Medico-Chirurgical Society by Mr. Spencer Wells. Mr. Brown's remarks are so good and practical that we here append them, separate from Mr. Wells' paper.]

Mr. Baker Brown said that as he had devoted much time and attention, for nearly thirty years, to the study of ovarian disease, he trusted that he might be allowed to make some observations on the paper which had just been read. He regretted its length, because so little time was left for discussing many points of great practical interest; he should, therefore, be only able to notice a few parts of the paper. He thought the author had not sufficiently appreciated the advantages of the clamp as suggested by Mr. Hutchinson, as in his opinion this was one of the greatest improvements yet made for perfecting the operation. He could not understand how any vessels in the pedicle could have required tying, if a proper clamp had been carefully applied. He thought that the author had not felt the importance of preventing the admission of air within the cavity of the peritoneum during the operation; this should be done by two assistants carefully covering the wound with warm wet flannels, so as to prevent both the extrusion of the intestines and the admission of the air. The air in the room should be warm, not less than 66° to 70° Fahr.; and the same degree of temperature should be maintained for some days after the operation, so as to allow the skin to perspire freely. The author had made one strong recommendation which he (Mr. Brown) considered seriously objectionable,—namely, that the cut edges of the peritoneum should be caught by the sutures or pins employed in bringing the edges of the wound together. Now he as strongly advised all operators to avoid this recommendation; for he had found, in experiments upon the lower animals, that peritonitis immediately followed the use of metallic sutures when passed through the peritoneum; and as experience proved that no necessity existed for this novel mode of bringing the edges together, he (Mr. Brown) trusted that it would be found necessary only to apply sutures through the aponeurosis of the muscles, carefully avoiding any portion of the peritoneum, just as the operator for hare-lip carefully avoided the mucous membrane in applying the pins. He had found that deep sutures alone were necessary in this operation, and he recommended that the first suture should be applied close to the pedicle, secured by the clamp, and then at every half inch upwards. By this means the chance of air getting into the peritoneal cavity during the process of healing was prevented—a point of great practical importance, as he believed that as much danger arose from the irritation of the atmospheric air within the peritoneum as from the sloughing pedicle, when allowed to remain in the pelvic cavity. He (Mr. Brown) observed that the author had not yet felt the importance of giving the patient opium and wine an hour before the operation, yet this was a point of great value, especially when preceded and followed by the free sucking of ice. He differed from the author in recommending the use of opiate suppositories in preference to the administering of opium through the mouth. He (Mr. Brown) believed that if the pure opium of commerce, made into pills direct from the mass, be used, most patients will be able to take opium who would not otherwise bear it; that suppositories were only required in exceptional cases, and then they were of real use. The author had mentioned one case where a band of adhesion extended from the cyst to the liver; these bands were peculiar, and required great care in dividing. Some years since, he (Mr. Brown) operated on a case in St. Mary's Hospital, where such a band existed, and after consultation with his colleagues, he cut through this band, which appeared to possess no bloodvessels, and yet this patient died two or three days afterwards, with all the symptoms of internal hemorrhage; and on a post mortem examination it was found that this band possessed two large bloodvessels, and from them the blood had flowed which destroyed the patient. In the very next case which he operated on in private practice, the same kind of band was found, and being torn through, no bleeding followed. This led to the conclusion that all adhesions should be torn through, and never cut. The question of adhesions in this disease was one which had led many to consider their existence as opposed to the completion of the operation. He (Mr. Brown) believed, and he was borne out by the great experience of Dr. Clay, that they offered no objection to the operation; indeed, it was doubtful if the peritoneum had not been so thoroughly altered from its normal character as to be less prone to inflammmation on that very account. In the second case described by the author, the adhesions were unusually many and strong, and offered such difficulties that most young operators would have been deterred from attempting to break them down; yet this very case did well. On the contrary, had the operation not been completed, there is little doubt she would have died from suppuration and formation of pus within the abdomen. The author alluded to the assistance which he (Mr. Brown) had given in this very operation, and had spoken of his zealous services. He (Mr. Brown) felt very strongly that the words efficient services would have been more appropriately used. Upon the general question of ovariotomy, Mr. Brown felt that a correct diagnosis was of the first and most vital importance. When the nature and character of the case was carefully made out, then the surgeon could decide on the different modes of treatment to be followed, but not till then. Supposing that extirpation was decided on, then the sooner an operation was performed the greater would be the chances of success: in fact, the history of the operation for hernia well illustrated this part of the subject. Formerly that operation was never performed till the patient was in extreme danger and unable to bear the shock; and if the patient did live for twelve hours, the usual black or aperient dose the next morning so irritated the bowel that death rapidly followed. Whereas now, by earlier operation and the use of opium afterwards, these cases were successful as they were formerly the contrary. In ovariotomy, a careful after-treatment is of as much importance as a correct diagnosis.—*Lancet, Feb.* 19, 1859, *p.* 187.

132.—ON PUERPERAL CONVULSIONS.

By Dr. Francis H. Ramsbotham, Obstetric Physician to the London Hospital, &c.

[On Monday, February 7, Dr. Ramsbotham was sent for by Mr. Pryce, of Walworth, to a patient pregnant for the first time, between six and seven months. She was stout and plethoric, and for six weeks previously had complained of drowsiness, puffy hands, and other similar symptoms. On the day previously, convulsions came on, preceded by a violent attack of vomiting and purging. The fits recurred very frequently through the night, and when seen by Dr. Ramsbotham she was quite unconscious, with widely dilated pupils, acting sluggishly to the stimulus of light.]

The uterus occasionally became hard, and there seemed to be a disposition for the commencement of premature labour. She had been bled twice during the night, losing about thirty ounces of blood at the two operations; and in the day had been cupped on the back of the neck, and twelve leeches had been applied to her temples. The hair had been cut off, ice was applied to the head, and a turpentine enema had been administered, which had brought away a large quantity of fœtid stools. I had some difficulty in reaching the os uteri with my finger, for it was very high, and inclined more than usually backwards. It was dilated to the size of a sixpence, just admitting the end of the finger, and the membranes were felt tense. Before withdrawing my hand I ruptured the membranes, and gave exit to a considerable discharge of liquor amnii; I then felt a limb, but could not make out which. Her size was gradually diminished. She had no more fits while I stayed in the house. I recommended that as long as she remained unconscious three grains of calomel should be placed on her tongue every two hours, in the hope of its passing into the stomach; and that another turpentine enema should be injected. There were only two more fits after I left; labour pains supervened, and she was delivered at 2 a.m. (Tuesday); the knees presenting. Her consciousness gradually returned soon after delivery; though headache continued. On Saturday, the 12th, Mr. Pryce wrote to me, that he had seen her that morning sitting up, after a refreshing sleep of five hours; that she still complained of her head, and felt relief from the cold application. The lochia after delivery were very scanty; she took ten grains of calomel, and for two days the sphincters performed their duty imperfectly. It is remarkable that she has no recollection whatever of anything that occurred for six days before the attack appeared, although she was following her ordinary occupation all the time; and except for the headache and drowsiness seemed as usual. No albumen could be detected in the urine. She is now convalescent.

Holding the opinion, as I do, that an attack of puerperal convulsions is merely a modification of cerebral apoplexy, I consider that this young woman's life was saved by the prompt and decisive bleeding. But I should not have detailed the case, were it not for one extraordinary feature that it displayed, and which I have before observed on no few similar occasions, I mean, the entire loss of memory in regard to the occurrences of some days antecedent to the seizure; while the patient appeared in health, and was occupied in her ordinary duties. I have before known this *obliviscence* extend to six days preceding the first fit, all which time, in the language of my father, who first, I believe, pointed out this peculiarity, seemed a blank in the patient's existence. Injuries to the brain occasioned by other causes have been followed also by the same loss of consciousness of events that happened before the accident occurred. M. Koëmpfen, in vol. iii. of the Royale Académie de Médicine, gives the case of a cavalry officer, who fell from his horsè on his head. A total want of recollection came over him of everything that occurred on the day previous to the accident, as well as for some hours after it. Though he recovered perfectly, he never regained his memory of these periods. A medical friend of mine, since dead, was thrown from his horse one morning at ten o'clock, while returning from a patient's house. On being brought home, he ordered appropriate medicine for his patient; and though he remained in his house, he appeared to his family to be quite conscious of everything that was going on around him. When he was well he told me that he had forgotten how the accident took place; and that he had no recollection of going to see his patient, nor of anything that had happened since seven o'clock that morning; the last thing he remembered was a child dying in his arms at that time, to which he had been called up two or three hours previously. He was equally forgetful of every occurrence that happened for twenty-four hours after his fall; although during the whole of that time he was conversing free and rationally with different members of his family, and also with his medical friend who had been called in on the emergency. Sir C. Brodie (Psychology, Eng., 1855, p. 58,) mentions the case of a young gentleman who was thrown from his horse while hunting. He was stunned, but only for a few minutes, and rode home in company with his friends twelve or thirteen miles, talking with them as usual. On the following day he had forgotten not only the accident, but all that happened afterwards. He gives another, in the person of the royal groom, who, while cleaning a horse, was kicked by him on the head. He did not fall, nor was he stunned or insensible; but he quite forgot what he was doing at the moment the blow was inflicted. "There was an interval of time as it were blotted out of his recollection."

In the Penzance Gazette, February 14, 1844, there is the instance of a woman who hanged herself, but was cut down before life was extinct. After her recovery she had no recollection of having attempted to destroy herself; nor did she remember anything that occurred on the previous day. And in the Times for July 4, 1845, there is an account of a youth who was charged with attempting to destroy himself by hanging, and who denied having any recollection of having made such an attempt. I have no doubt that his forgetfulness was not feigned. I attended a woman through a very severe attack of puerperal convulsions, who, when I saw her a month after, had completely forgotten how to write. though she wrote very well before. She could not make a letter without having a copy before her; and when she tried she looked exactly like a child beginning to learn. Brodie also, *ut supra*, gives an instance of a gentleman who, after a fever, had forgotten how to read; of another who, having recovered from an attack of apoplexy, was unable to understand oral language or to converse, though he could read as well as ever.

These instances, in my opinion, would go far to establish the truth of the doctrine, that different parts of the brain are subservient to different passions, emotions, and other attributes; for it seems clear that in all of them, whatever other portions of the organ might have received injury, that particular locality, wherein is situated the seat of memory, was more than any other disturbed in the performance of its functions.—*Medical Times and Gazette, March* 5. 1859, *p.* 233.

133.—REMARKS ON THE INTERNAL EXHIBITION OF TURPENTINE IN NEURALGIA AND PUERPERAL DISEASES.

By Dr. Gondouin.

Hitherto essential oil of turpentine has seldom been used in France, except for the purpose of counter-irritation, and chiefly ranked among the resources of veterinary medicine. Internally the syrup was prescribed, or the essential oil solidified by some process, the object being to modify, by the substituting method, the condition of mucous membranes in a state of chronic inflammation, attended with excessive secretion. Thus turpentine was exhibited for chronic bronchitis with abundant bronchorrhœa, and in old cases of gonorrhœa, in which the disease had extended to the mucous lining of the bladder, ureters. or kidneys. In the early stages of consumption, M, Trousseau had found turpentine particularly beneficial; not that he expected by its use to modify the morbid deposit in the lung, but with a view to the removal of the phlogosis, which almost invariably, at least in a latent manner, accompanies its formation. The professor is still in the habit of prescribing to consumptive patients the syrup of turpentine, in doses of 1 to 2 ounces daily.

Our readers are also aware that M. Trousseau, for some years, has recommended in obstinate cases of sciatica, the following enema:—

℞. Ol ess. terebinth,, 2½ dr.; vitelli ovi., N. 1; aquæ. 3½ oz.; laudan, liq. syd., 26 m.

Make the oil into an emulsion with the yolk of the egg, add the water gradually, and finally the laudanum. This mixture should be divided into two parts, one of which is injected in the morning, the other at night. A simple enema of warm water should first be taken, and when it has been returned, the emulsion should be injected in as little water as possible. The treatment must be continued for several days, or a relapse is to be feared.

In England, however, turpentine has been applied to more important uses, and M. Trousseau now prescribes it, according to the precepts laid down by Dr. Graves, of Dublin, with much benefit to women suffering from puerperal symptoms. The extreme gravity of the phlegmasiæ consequent upon parturition, due precisely to their tendency to suppuration, is a well-known fact; whether the inflammation is limited to the peritoneum, extends to the subjacent cellular texture, or spreads to that in which the iliacus internus and psoas muscles are imbedded, the anxiety these symptoms occasion is the same. In these various circumstances, unconnected, of course, with epidemic influence, against which the resources of science are generally unavailing. M. Trousseau has derived advantage from the exhibition of large doses of turpentine, in combination with opium. Under the influence of this treatment feverishness yields, the pulse loses its frequency and becomes fuller, the abdominal pains and tenderness decrease, and finally, secondary suppuration is less common. If at the period of the admission of the patients into hospital, the feverishness has already abated, and abscesses have formed, the Professor still perseveres in the use of the same medicine, under the impression that it will conquer the inflammation which still lingers around the abscesses. With regard to purulent collections, they disappear, it is true, very seldom indeed; but after a time, and therefore with little peril to the patient, they open either in the intestine, or the bladder, if they are situated beneath the peritoneum; or generally in the inguinal region, if they occupied the sheath of the psoas, or iliacus muscles.

We are unable to point out the precise mode of action of turpentine in these various instances. Its administration, in such cases, is altogether empirical, and Dr. Graves and M. Trousseau recommend it much in the same manner, and without any more distinct motives, in those erratic forms of neuralgia specially observable in women, and which cannot be satisfactorily traced to any particular cause or to any anatomical change.

Last year, Dr. Bonfils noted in M. Trousseau's wards, and recorded in the Bulletin de Thérapeutique two interesting cases of puerperal peritonitis, which were speedily checked by this treatment. The first of these patients was, concomitantly with peritonitis, affected with pleuro-pneumonia on both sides of the chest; and in spite of this dangerous complication, she recovered in the space of seven weeks. The second case had a less fortunate issue, but the puerperal symptoms had completely yielded, when morbid manifestations of a different nature made their appearance, and she was carried off, in all probability, by a putrid disease. In both instances the medication was instituted as follows:—

In the first case, opium was prescribed in pills, and turpentine in enemas. Five pills, of one-sixth of a grain each, were at first taken during the day; the dose was then increased to eight, and finally to ten pills daily, opium being continued during thirteen days. The dose of the turpentine was at first 2½ dr. for two enemas. one in the morning, the other at night. The quantity of turpentine was then gradually augmented to 5 dr., 6½ dr., and 8 dr., and the latter dose was persevered in for a fortnight. The formula for two enemas was the following:—

℞. Ol. ess. terebinth 2½, 5, 6½, 8 dr.; vitelli ovi N. 1; aquæ 3½ oz.

Add to each injection five or six tablespoonfuls of decoct. radicis althæ, or of the infusum lini. The enema to be retained as long as possible.

In the second case, five-sixths of a grain of opium were also exhibited in pills during three days. The turpentine was swallowed in lichen capsules, each containing 15 gr. of the essential oil. The patient took six every day —two in the morning, two at noon, and two at night.

Quite recently, in a case of erratic pains in the appendages of the womb, in a young woman whose catamenial periods were irregular, M. Trousseau prescribed the following emulsion, to be taken in four doses in the course of the day:

℞. Ol. essent. terebinth 1¼ dr.; vitelli ovi ½; syr. cort. aurant. 10 dr.; aq. melissæ 2½ oz. F. S. A.

Thus administered, turpentine preserves a most unpleasant taste, and in private practice it is more conveniently exhibited in small gelatinous capsules of the size of an olive, which open and close like a needle-case. They are not expensive, and have the further advantage of being easily filled by the patient immediately before being swallowed, one by one, in a spoonful of water, in the course of the day.

It has been remarked, that these gelatinous capsules are open to the objection of placing the essence in immediate contact with the coats of the stomach, and of causing thereby a certain amount of discomfort to that viscus when the protecting sheath breaks. Some practitioners prefer, therefore, to dilute the essence in some appropriate excipient—a method which is supposed to render the action of the medicine more general, and milder. For this purpose M. Perrens, Secretary-General of the Society of Pharmacy of Bordeaux, has proposed, in the Union Médicale de la Gironde, to disguise the nauseous flavour of the turpentine by the addition of oil of peppermint. 15 m. of this oil, mixed with ½ oz. of turpentine, render the latter less unpalatable. M. Perrens recommends the following formula;—

℞. Vitelli ovi No. 1; ol. ess. terebinth. ½ oz.; ol. ess. menth. pip. 15 gr.; syrupi 1 oz.; aq. destill. minth. 3 oz. M. F. S. A.

In the case of very impressible women, who might not be able to overcome their reluctance to the above mixture, the turpentine may be prescribed in an electuary, and the following M. Perrens considers a satisfactory combination:—

℞. Ol. ess. terebinth. 2 gr.; tragacanthæ 10 dr.; sacchari pulv. 5 dr.; syrupi q. s.

Prepare a semi-fluid electuary, two teaspoonfuls of which may be taken in a wafer impregnated with strong peppermint water.

We should, however, remark that, when turpentine is exhibited, as it is by Professor Trousseau, in capsules, followed, if necessary, by one or two drops of laudanum, it seldom causes any serious disturbance of the digestive functions. Appetite, it is true, sometimes diminishes, and some unavoidable nausea may be experienced, but the stomach soon, in most instances, becomes familiarized with the medicine, and the epigastric heat and cerebral excitement produced by the first doses of turpentine soon disappear altogether.—*Dublin Hospital Gazette, May* 1, 1859, *p.* 139.

134.—ON A CASE OF INFANTILE SYPHILIS, WITH REMARKS.

By Dr. T. H. Tanner,

(Being an abstract of a paper read before the Obstetrical Society, London.)

The author commenced by observing that amongst the diseases which may be propagated from parent to offspring, few are more disastrous in their results than constitutional syphilis. It is probable that the syphilitic poison is the direct cause of the greatest number of abortions and premature labours which occur in the present day; and that even when it fails to destroy fœtal life at an early period of gestation, it induces other severe disorders, having a fatal tendency at a more or less remote period. The chief points of interest in the case then related are the following:—

In August, 1851, a married lady was delivered of her first child, which was strong and healthy, and has since continued to be so. Soon after her labour the husband contracted a syphilitic sore from a prostitute, for which he put himself under the care of an eminent surgeon. He took mercury, and was salivated; but two months after an apparent cure he became affected with secondary symptoms, for which he again took mercury. Being nervous as to the consequences, he did not have intercourse with his wife until after the lapse of nine months from the date of his being primarily affected. At the commencement of 1853, the wife's health began to suffer, though not very materially; but on the 12th June of the same year she was delivered at the seventh month of gestation of a still-born child. Some months afterwards her health began more decidedly to fail; spots appeared on her skin; she had a sore throat, and her hair came off. In May, 1854, she gave birth to her third child; it was dead, and she fancied she had not gone more than six months and a half with it. In the November of the same year she aborted at the third month. In August, 1855, she had a child born dead at the sixth month; and in October, 1856, she was delivered at the eighth month of pregnancy of another dead child.

In March, 1858, she first became a patient of Dr. Tanner, and was then put upon a course of bichloride of mercury for three months. On the 4th of last September she was delivered of a seemingly healthy live child; the labour took place some three weeks before its proper time. The infant only remained well about a fortnight, when it manifested all the symptoms due to constitutional syphilis. The treatment consisted in the inunction of mercurial ointment, no medicine of any kind being given by the mouth. In a month the child was apparently well in every respect, so that all medical treatment was discontinued; but a fortnight afterwards it died suddenly. At the post-mortem examination, every organ was found healthy, the brain, lungs, thymus gland, heart, &c., all presenting a perfectly natural appearance. The only change was in the blood, which seemed to be more watery than it ought to be.—*Lancet, June* 11, 1859, *p*. 586.

135.—ON THE TREATMENT OF CARCINOMA UTERI.

By Dr. J. Y. Simpson, Professor of Medicine and Midwifery in the University of Edinburgh.

In the way of the constitutional treatment of uterine as of other forms of cancer, we can do nothing or almost nothing, except, perhaps, retard and alleviate the course and the effects of the malady. Nearly every form of mineral and nearly every form of vegetable remedy has been exhibited and tried which seemed at all likely to stay the progress of the malady, but as yet with little, or indeed with absolutely no success whatever. All that we can do constitutionally is to keep the patient as near the standard of health as possible by generous diet, by invigorating regimen, and by tonic medicines, and thus enable her to bear up against the debilitating and destructive march of the disease.

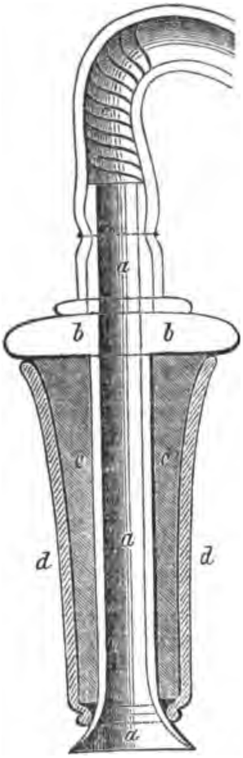
But we can employ various measures which are of more or less avail to stay and stem, at least, the local symptoms and sufferings, and smooth the patient's progress to the grave.

With this view, we have three leading indications to attend to, viz., 1. To use means to alleviate the pains and sufferings attendant upon the disease; 2. To use means to prevent and arrest the attendant menorrhagia; and 3. To use means to counteract the offensiveness and acridity of the attendant discharges. Let us first consider the

1. *Measures calculated to palliate the attendant pains.*—To relieve the pain you must learn to administer opium by the stomach or rectum, or to apply it locally to the uterus, accordingly as you find the patient can best bear it. Begin it always in small doses. Most women suffering from this or from other painful forms of malignant disease, become, as it were, opium-eaters;

and I think it is our duty to teach them how to keep themselves as easy and comfortable as possible, whether the object be best attained by the use of opium or in any other way. It is certainly our duty to alleviate, when it is beyond our power to cure. Where opium disagrees with a patient, you must have recourse to some other anodyne drug. You will find that conium has been much employed and recommended by some authorities, who have supposed it not only to be of use in relieving the pain, but to have some specific power of checking the progress of cancer. I have never been able to satisfy myself of the existence of this power; but I believe it to be a very good sedative. So also are belladonna, lupuline, stramonium, and Indian hemp, in some exceptional instances. Besides being used by the mouth, most of these anodynes may, if it be deemed better, be administered by the rectum in the form of a suppository, or applied to the cervix uteri in the form of a medicated pessary. By this term of "medicated pessary," I mean, as most of you know, small balls of a round or ovoid form, and of the size of a walnut, made of some medicinal substance mixed up with ointment, and brought to a proper degree of consistence by the addition of yellow wax. Our Edinburgh druggists keep medicated pessaries of various kinds, containing morphia, belladonna, tannin, &c. The patient can usually introduce them herself, and they have the advantage over injections and lotions of applying the medicinal agent in a more continuous form. They are made of such a consistence as to dissolve gradually in the vagina at the temperature of the body. Generally they are coated with a layer of ointment made firmer from containing a larger proportion of wax, with the view of facilitating their introduction.

We have other local sedatives in the vapour of chloroform, and in carbonic acid gas, which may be used to supplement the action of these anodyne remedies, or to supplant them entirely where some idiosyncrasy prevents their use. The carbonic acid has been chiefly employed here within the last few years, and I imagined for a time that this application of it was something new and modern. But I may as well warn you, that should any one of you imagine that he has made a discovery of something practical in medicine, if he will take the trouble carefully to look over the works of Hippocrates, or Galen, or Paul of Ægina, or of some other ancient medical writer, he will very probably get all the glory taken out of him. When the ancient Greek and Roman physicians burnt various herbs, the fumes from which were conducted by a tube to the os uteri in cases of uterine pains, ulcers, &c., they in reality applied carbonic acid gas. In Germany the waters of some of the baths, such as those of Marienbad and Nauheim, have long been used as local sedatives to the uterus, and these waters contain always a proportion of free carbonic acid. Dr. Dewees, a distinguished American author, also speaks of having used the pure gas in this way as a sedative for the pains of carcinoma uteri. My attention was first called to the subject by a paragraph in one edition of Dr. Pereira's "Materia Medica;" but Dr. Pereira himself considered this paragraph of so little importance that he expunged it from the last edition of his work. The paragraph was to the effect that his friend, Dr. Clutterbuck, had been requested by a lady suffering from uterine pain and irritation, to be supplied with some means of applying carbonic acid gas to the womb, as she had formerly experienced great benefit from such an application at the hand of an Italian physician. Latterly, I have used it extensively in many cases of uterine pain, and in some with excellent effect; for it is, in fact, a good and powerful local anæsthetic. The application is very easily accomplished. A tablespoonful of crystallised tartaric acid is mixed with a tablespoonful of crystallised bicarbonate of soda in an ordinary wine-bottle, three or four wine-glassfuls of water are added, and the gas which is evolved is carried off through a caoutchouc tube, and applied to the womb by means of a gum-elastic nozzle attached to the extremity of the tube. Our chemists have got into the way of supplying patients with boxes such as that I have beside me, containing twelve powders or packets, with six drachms of tartaric acid in each, and other twelve with an ounce of bicarbonate of soda. They furnish them also with the appropriate tubes, which, let me add, are sometimes provided with a sort of brass box immediately above the cock or stopper to hold pieces of sponge, and by pouring on these sponges some chloroform, you may have the combined sedative action of the two anæsthetics. Usually, however, the hollow cork and tube used are simple, (see Fig.) and without any box; and if you wish to apply chloroform vapour along with carbonic acid, you require when employing the simple tube, to add merely a teaspoonful of chloroform to the contents of the bottle before introducing the cork. When the tube is introduced into the vagina, after the evolution of carbonic acid gas has commenced, there is perceived first of all a rush, and a slight feeling of heat; by-and-by a soothing effect is produced. Besides its anæsthetic properties, carbonic acid is one of the best of local curative applications that can be made to an ulcer. In the last century Dr. Ewart, of Bath, made the experiment of applying this gas to two open cancers of the breast constantly and for a considerable time, and with this good result, that one healed up completely, though of course only temporarily; and in the other the pain was relieved and the ulcer partially healed. When carbonic acid fails to relieve the pain, the vapour of chloroform may be superadded to it in the manner I have indicated: or chloroform vapour by itself may be applied by means of an ordinary Higginson's (barrel) syringe, which, let me add, is the cheapest, best, and most convenient of all syringes for all purposes. The long or free extremity of the syringe is introduced into the vagina, and the other end of the instrument is inserted into the mouth of a four or six-ounce bottle, about one-third or one-half filled with chloroform, and then the application of the fingers to the middle part or barrel being in the way of alternate compression and relaxation of the barrel, sends speedily a current through the apparatus. If the bottle were full, or nearly so, there would be a risk of the liquid chloroform getting into the instrument, and being pumped into the vagina, which it would blister and scald, and thus produce an effect quite the opposite of that which is desired. After the tube has been introduced into the vagina, by working the barrel in the ordinary manner, the vapour of the chloroform rises into the instrument, and may be projected for any length of time against the uterus: for you can send through the instrument a current of air or vapour as easily as a current of liquid. Applied in this manner to mucous surfaces generally, chloroform vapour has a very soothing and sedative effect. Ten minutes usually suffice for the application at one time either of carbonic acid gas, or the vapour of chloroform; but patients sometimes desire it to be continued longer. Either of them, or both, may be repeated, if necessary, many times a-day.



Section of the perforated cork used in the local application of carbonic acid gas to the uterus. *a a* Metallic tube passing through the centre of the cork, and receiving the gas at its wide extremity *a*. *b b* Metallic ring, in which the metallic tube is fixed. *c c* The perforated cork, sheathed by a layer of caoutchouc *d*. *e* A coil of wire in the commencement of the india-rubber tubing, to prevent it from collapsing at the point of flexion: the metallic tube may be prolonged and bent, and in this case the coil of wire can be dispensed with.

I would just add one other remark in regard to chloroform, as supplement to my observations on the constitutional, as constrasted with the local sedative treatment, of the pain which attends a cancer of the womb, and it is this, that where opium disagrees with the patient, or where she requires such large doses that she is deterred from using it, you may relieve the pain by bringing her occasionally under the general anæsthetic influence of chloroform, by inhaling small quantities from time to time, and thus you may occasionally procure for her a rest and a respite from her suffering, more easily and completely than by any of the usual forms of anodynes. I have seen chloroform, when swallowed, also answer well for a time as a general sedative in cases of cancer. One of the best modes of exhibiting it, is by making a mixture of chloroform with compound tincture of cardamoms, in the proportion of five or ten drops of chloroform to each drachm of the tincture. A teaspoonful of the solution or mixture may be given in a wineglassful of water several times a-day, or whenever the pain becomes severe. Laudanum or solution of morphia may be added to this mixture in appropriate doses, if you wish to combine it with an opiate. Remember to order the dose of the solution of chloroform in the tincture of cardamoms to be added to and stirred up with the wineglassful of water; for, if you reverse this, and add the water to the mixture, part of the chloroform is generally separated and precipitated.

There is another means by which you may sometimes succeed in alleviating the pain of cancer. I allude to the application to the part of some freezing mixture. Dr. J. Arnott, who has directed particular attention to the anæsthetic effects of a low temperature to various parts of the body, has suggested that the freezing of a cancerous part may be of use not only for soothing pain, but also as a means of curing the disease. And when the first experiments began to be made with it, some cases of temporary cure were reported, just as in the last century, Ewart and others met with partial and temporary healing of open cancer of the mamma from the continued application of carbonic acid gas. We had a patient with ulcerated carcinoma of the cervix uteri in the hospital a few years ago, whom we treated by the occasional application of freezing mixtures to the os. These generally relieved her pain, and seemed to check the discharge, and make the ulcer partially contract and heal up for a time. But ulceration soon set in again, and the condition of the patient speedily got as bad as before. Yet possibly the application] of a refrigerating mixture might prove a useful adjunct to our means of treatment, and might be much more frequently and regularly employed if we could get over the one great objection to its use which lies in the difficulty of its application. At present the method employed is to introduce a speculum into the vagina, and through this to apply to the os uteri, in a muslin bag, a freezing mixture. The mixture most commonly employed consists of two parts of ice to one of common salt. The ice must be pounded or bruised, and when thoroughly mixed up with salt in the proportion I have just stated, and applied in a muslin bag to a part for a certain length of time, the circulation there is stopped, and the part becomes cold, pale and insensible. This condition remains for a short while after the removal of the bag, and then the parts gradually return to their previous condition. Perhaps some simpler and more manageable method of applying great cold may yet be devised, and if this do happen, it may prove a valuable boon. For we know that the application of ice, or, to speak more correctly, of a temperature at or below the freezing point, to other parts of the body, has the effect of temporarily alleviating pain, or temporarily inducing an insensibility during which some of the slightest surgical operations can be performed without causing any pain to the patient. Thus I have lately seen two gentlemen have each several teeth extracted without pain, from having their gums previously frozen. One of these gentlemen, my friend Dr. Small, from whom seven teeth were extracted after a stream of water of a temperature of from 10° to 20 Fahrenheit had been for some time allowed to pass through a thin metallic box accurately fitted to his gums, certainly made wry faces during the operation; but he explained afterwards, that the contortions of his countenance were not excited by pain, but by the mortal terror he was in at every application of the forceps, lest the "grunch" which he distinctly felt would be accompanied by pain. If, I repeat, we had some simpler means of freezing the os uteri and other parts affected with cancer more easily, we should more frequently perhaps have recourse to this expedient. As yet our only means are only a source of irksomeness to the patient and of difficulty to the practitioner. It has always seemed to me that solid carbonic acid applied in a caoutchouc bag, or otherwise, and mixed perhaps with ether, ought to be the most convenient and best freezing agent for producing local anæsthesia in the practice al ke of the dentist, the surgeon, and the accoucheur, and that we may find some easy means of applying this to the os uteri; but I have not yet been able to obtain the acid in the solid state, so as to have an opportunity of reducing the theory to practice. Patients with cancer of the uterus will often require your medical care for other forms of distress and suffering than the mere local uterine pain connected with the disease. They have sometimes their sufferings aggravated by a tendency to constipation, particularly when they first begin to use opium, which is kept up in the later stages by mechanical obstruction of the bowel from the spread of the carcinomatous deposit. Gentle laperients, or what often serves better still, mild enemata, are required under these circumstances. In the same way the bladder is apt to be irritated during the progress of the deposit, and you will find it occasionally necessary to relieve the symptoms of dysuria with infusions of uva ursi, buchu, &c., and with the addition of the alkalies or acids, as indicated by the condition of the urine. Besides the secondary or sympathetic pains which may spring up in different parts, and of which I have already told you, occasionally demand for their alleviation local anodyne liniments or plasters, or even the injection of a few drops of the watery solution of morphia into the subcutaneous cellular tissue of the affected part. Occasionally you will find that they will disappear after the application to the ulcerated surface of the cancerous uterus of slight caustics or sedatives, such as a solution of nitrate of silver, acid nitrate of mercury, and the like.

2. *Measures calculated to arrest the attendant Hemorrhages.*—But there are other indications which require to be fulfilled besides that of the alleviation of pain. Thus you will sometimes be called upon to check and counteract the hemorrhage which occasionally occurs to an excessive and exhausting extent in cauliflower excrescences, and other malignant diseases of the cervix uteri. In these cases you have large, tortuous, thin-walled vessels coming up in loops towards the surface of the papillæ, and merely surrounded by radiating layers of epithelial looking cells, as shown in Virchow's memoir on "Cancroids of the Os Uteri." From these vessels, which are very liable to injury, profuse floodings readily occur, debilitating and destroying the patient, who would not be so rapidly worn out by the disease if we could always manage to arrest them. Such a flooding, let me again repeat, is often the first symptom that attracts the patient's notice. It is apt frequently to recur in some cases, rapidly ruining and running down the patient's health and strength, and not unfrequently proving the more immediate cause of her death. This bleeding, therefore, you will often be called upon professionally and practically to abate and arrest; and as one means of attaining your object you may occasionally require to have recourse to plugging of the vagina. But a mere plug of lint or sponge will not always suffice; you will sometimes have to medicate them with some agent which has the property of coagulating the blood. Some have recommended the use of a concentrated tincture of iodine as possessed of this property in a high degree. Others have spoken in favour of a strong solution of nitrate of silver. In either case it has usually been proposed to apply the medicaments through a speculum introduced into the vagina, and pushed up to the cervix. But when I stated to you in reference to the diagnosis of uterine cancer, that the speculum was a useless instrument, I should have added also that it was a dangerous one, for coming in contact with the vascular mass it almost necessarily excites more or less hemorrhage, or aggravates it when it has already been established; and we gain a very important point, if, in the application of our styptics, we can dispense with its employment. We have two remedies, the application of which is easy, and in almost every case is perfectly sufficient. One of these is tannin, which, when applied in the form of a fine powder through a small tube, or mixed up in the form of a medicated pessary, serves as a valuable means of coagulating the effused blood, and thus preventing the further flow. Matico and other vegetable

astringent washes are useful merely in proportion to the quantity of tannin which they contain. But we possess a still simpler and surer styptic in the perchloride of iron, which I generally use as made at my suggestion by Messrs. Duncan and Flockhart, druggists, dissolved in glycerine. A saturated solution of it in glycerine is more adhesive to the surfaces with which it is placed in contact, than the solution of it in water. Perchloride of iron was proposed, as you know, by some physicians and surgeons on the Continent not very long ago, as a preparation which—when injected into the sac of an aneurism, or the cavity of a varicose vein—would produce coagulation of the blood in them, and so lead to the permanent cure of the morbid states. For stopping the hemorrhage from leech-bites, it is the best and readiest agent we possess; and for arresting the bleeding from small orifices over an extended surface, such as exists in piles, I know no better remedy. A gentleman in our profession, for whom I have a very high regard, told me lately, of the great success which had attended its use in his own wife, who had long suffered from internal hemorrhoids, which frequently came down and bled to a great degree. Six months ago, she had a very bad attack, at a time when her husband happened to be from home. There had been much hemorrhage, and she was reduced to great weakness and faintness. Though the wife of a doctor, she was one of those ladies who have a great and salutary aversion to seeing doctors professionally; and so when she sent one of the members of her family to ask me for something to relieve her, I sent simply some perchloride of iron in glycerine, with instructions to apply it on a piece of lint. The bleeding was checked by this means at once, and her symptoms relieved; and, what is better still, the hemorrhage has never since returned. In connexion, I say, with hemorrhages from the uterus, it is likewise a most valuable agent, and may be applied in various ways. It deliquesces very readily, so that it cannot be kept and applied in the form of powder; but it may be made up for use into a medicated pessary. I apply it most frequently, however, on some lint or on a piece of sponge, to which a string is attached for its easy removal afterwards. Introduce a piece of sponge or lint partially dipped, or rather wetted in its centre, in glycerine, saturated with perchloride of iron, into the vagina; push it up to the os uteri, and leave it there for twenty-four hours; and usually you will find this result, that the bleeding is completely arrested, and there is no recurrence of it for a time. I sometimes see a patient at present in whom the hemorrhage from ulcerating cancroid of the cervix has been allowed to go on for three months, under the idea that it was only an ordinary menstrual discharge of unusually long duration. Early in my attendance upon her there occurred a sudden and profuse drain, which rendered her almost pulseless. A sponge was applied, steeped in the solution in the manner I have told, which at once put an end to the bleeding. There has been no recurrence of hemorrhage from that time to the present—a period of three months; but the disease is progressing in its fatal course, and lately has perforated the bladder, so as to allow all the urine to escape per vaginam.

3. *Measures calculated to counteract the Offensiveness, &c., of the attendant Leucorrheal Discharges.*—Another symptom which you will sometimes find it necessary to do something to relieve, is the fœtid and acrimonious discharge. The odour it exhales is occasionally sickening to the last degree; the very breathing of it fevers the patient; and, therefore, the discharge must be checked or its effects counteracted as far as possible. This may be effected in some instances by the frequent injection of a weak solution of the chloride of zinc, in the proportion of a grain to the ounce of water; or one or two grains may be applied occasionally in the form of a medicated pessary. Or you may endeavour to fulfil the indication, as we have attempted it in the two cases in the hospital, by using pessaries, containing from three to ten grains of M'Dougall's disinfecting powder. This deodo-rising powder was discovered, not by Mr. M'Dougall, but by Dr. Andrew Smith, the late director of the Army Medical Departments. It contains, I believe, as its active ingredients, carbolic sulphite of magnesia and lime, with the addition of five per cent. of carbolate of lime, and has the property of pre-

cipitating all fœtid and decomposing animal matters in foul waters, &c. When applied here in the form of a pessary or lotion it is of great use in relieving the odour, and serves at the same time as a cleansing and non-acrid application to the ulcerating surface.

Sometimes the vaginal discharge in cases of carcinoma uteri is, for a long period, watery and serous, without offensiveness or acridity, but weakening and detrimental from its great abundance and copiousness. This occurs particularly in instances in which the disease has a warty or cauliflower character. Such patients occasionally require to use many napkins each day in order to absorb the excessive serous discharge as it escapes from the vagina. In these cases the abatement and arrest of this profuse and debilitating drain becomes occasionally an important indication of treatment; but it is an indication which it is by no means easy to fulfil. Sometimes the assiduous and repeated use of astringent vegetable or mineral injections has the desired effect. More frequently their effect is only either very partial or very temporary. In order that they may be successful you generally require to change every few days from one astringent to another—as from a solution of tannin, or from a strong injection of oak-bark or green tea, to a solution of sulphate of zinc or sulphate of alum, one of the least irritating and astringent of all—being a solution of the so-called aluminated iron, or sulphate of alum and iron in the proportion of three or four grains of the salt to the ounce of water. Sometimes these astringents answer more effectually if the patient apply them in the form of a small plug of sponge dipped in the fluids or solutions.

In all cases of carcinoma uteri, accompanied with discharge, due cleanliness and frequent ablution of the external parts and lower portion at least of the vagina, is a matter of moment, with the view of obviating the disagreeable and irritating effects of the acrid fluid which is passing over them. and adds greatly to the comfort of the patients. Sometimes the mucous membrane of these parts and the surrounding skin can be protected much from the irritation of profuse and irritating discharges, by the free application of liniments or ointments after each use of the bidet, or after each washing. Equal parts of glycerine and simple ointment, or of glycerine and olive-oil, or of lime-water and olive-oil, often answer this purpose well.

Before I conclude these observations on the palliative treatment of carcinoma uteri, I must add one observation, lest my remarks may have misled you, so as to make you too officious in your management of your cases. Do not unnecessarily oppress your patients with the *nimia cura medicinæ.* In instances where the pain of the disease is not very great, nor the bleeding or discharges at all great, you will perhaps only aggravate the one and the other by unnecessary and meddlesome local treatment. You will frequently find cases of uterine cancer, particularly those of a slow type, go on better without any local treatment at all than with it. Such patients are often happier and more comfortable when cleanliness merely is duly attended to, and nothing special in the way of treatment is attempted or done. Reserve the palliative and local measures I have spoken of for these cases—and they form a large class—where such measures are really called for by the severity of the attendant symptoms.—*Med. Times and Gazette, Jan.* 22, 1859, *p.* 77.

136.—MEDICO-CHIRURGICAL USES OF VULCANISED INDIA-RUBBER.

The Medico-Chirurgical applications of caoutchouc air-bags are being again brought under the notice of the profession in this country; but the merit of originality belongs to M. Gariel, whose proposals appear to be ingenious and practically useful. We believe that our readers will find the following extracts from M. Gariel's Memoire, translated and sent to us by Mr. Jardine Murray, of Brighton, to be interesting and instructive. The wood-cuts are selected from among M. Gariel's illustrations.

FIG. 1.

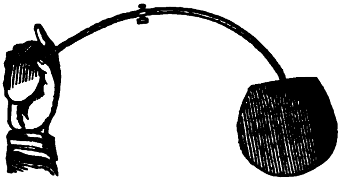


FIG. 1.—Caoutchouc air-bag, which may be used either as a pessary in displacement of the uterus, as a plug in uterine hemorrhage, or as a dilator of the vagina.

FIG. 2.

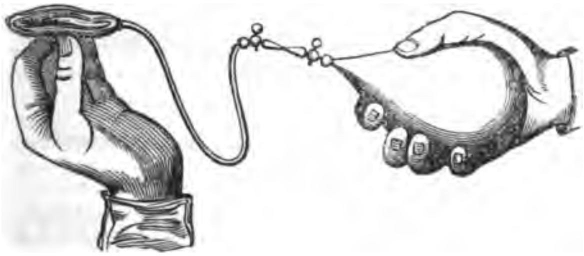


FIG. 2.—A similar air-bag of large size is here represented in the collapsed condition in which it ought to be introduced. The caoutchouc bag is distended to the requisite extent by compressing the insufflator, which is represented as held in the right hand. A ball-valve syringe may possibly be found preferable to the bag insufflator here represented.

FIG. 3.

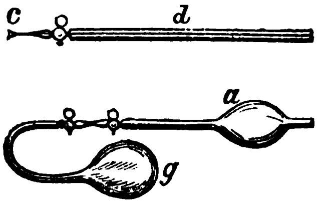


FIG. 3.—This figure represents M. Gariel's application for dilating strictures of the urethra, œsophagus, cervix uteri, &c. In its collapsed state the bulb is hardly to be detected. To facilitate introduction, the tube is fitted with a wire stylet *cd*, which is withdrawn when the end of the apparatus has been carried past the stricture. An insufflator, *g*, is then attached in the usual way, and the bulb, *a*, may be distended to the necessary degree. M. Gariel's ingenious chapter on this subject will amply repay perusal.

"APPLICATIONS FOR DILATATION.

P. 35. B. AIR SOUNDS FOR THE DILATATION OF THE VAGINA AND CERVIX UTERI.

"186. The sounds with bulbs applicable to the dilatation of the vagina and cervix uteri, must (like the bulbous sounds already described—œsophageal, urethral, &c., p. 33) be introduced empty of air.

"187. The continuous and progessive dilatation which may be obtained by insufflation, is so powerful, that all congenital or acquired strictures of the vagina must give way before it, unless these latter be complicated with extremely hard cicatricial products (bridles).

"188. Their application is easy, however great the degree of stricture, since a sound 3 millimètres in diameter, may readily be made to produce a dilatation of 2 or 3 centimètres. It may be necessary, however, to use sounds of various sizes; but it is only after having employed the smallest sounds that the larger sizes can be used.

"189. Might not this property of the bulbous air-sound be turned to advantageous use in inducing premature labour in cases of deformity of the pelvis? (Ne pourrait-on également tirer parti de cette disposition des sondes à renflement pour provoquer prématurément l'accouchement dans les cas de vices de conformation du bassin?)"

"HÆMOSTATIC APPLICATIONS.

P. 41. A. AIR-PLUGS.

"219. These plugs consist of a caoutchouc tube, terminated by a bulb which is scarcely noticeable when empty, but is susceptible of considerable enlargement when distended by insufflation. (See fig.)

FIG. 4.

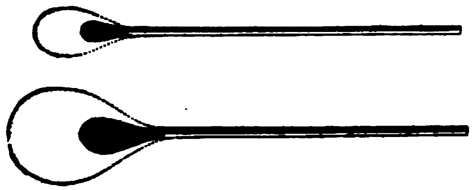


FIG. 4—Air-bags, of various sizes, for plugging or dilating the vagina, cervix uteri, nasal fossæ, &c. The dotted lines indicate the periphery of the bags when moderately distended by air.

"220. They are applicable to all cases and to all cavities in which it is usual to resort to plugging.

"221. They are unalterable, and may remain in position during several days without undergoing the slightest decomposition.

"222. They apply themselves exactly over the parts which are the seat of hemorrhage.

"223. Their volume may be diminished at once, or by degrees, without producing any alteration in the shape of the apparatus, or any folds in its walls; the blood cannot, therefore, escape through one of those longitudinal folds which always exist when any amount of air is withdrawn from a non-elastic bladder.

"224. Should the hemorrhage reäppear from their volume being too soon or too rapidly diminished, they may be reïnflated with the greatest ease.

"225. These plugs may be made of vulcanised caoutchouc, or of caoutchouc imperfectly vulcanised, which latter I prefer in this special instance. When made of vulcanised caoutchouc, their walls are too resisting to allow of their being dilated otherwise than by an insufflator, and I would rather avoid the necessity of using an instrument which may have been forgotten, and which at all events renders the apparatus less portable. Besides, in cases of hemorrhage there is often no time to lose, and I think pulmonary insufflation may be more quickly accomplished than artificial insufflation can be.

"226. These plugs are so small that they may easily be carried in a surgical pocket-case.

"227. The air-plugs suitable for the vagina, are of larger size than those for the rectum or nasal fossæ.

"228. The uterine plugs are always used successfully, except in flooding after delivery. Bleeding may always be immediately checked when it is dependent on polypus, cancer, &c.; or when it occurs during the early months of pregnancy. There is only one exception to this rule—when the os uteri is exceedingly dilatable, as after parturition at the full time.

"229. In such a case, if, after the application of a plug to the vagina or even to the uterine cavity, the flow of blood have been arrested, it were unsafe to infer that the hemorrhage is necessarily checked; for internal hemorrhage might be going on, and if unattended to, might cause the most serious accidents.

"230. But it is not certain that this would occur. It is questionable whether, the passages being hermetically occluded, the blood would flow in sufficient quantities to fill up the dilatable cavity of the uterus. Air is not indefinitely compressible, and there is every reason to believe that the accurate occlusion of the vagina (which may be so easily obtained) would suspend the hemorrhage after the loss of a few ounces of blood, and without it being necessary to apply direct compression at the point from which the hemorrhage originates.

"231. A very simple experiment proves how easy it is to produce hermetical occlusion of the vagina:—I take a tumbler a quarter or half filled with water; in the upper and empty portion of this I place an air-plug, which I insufflate until it presses against the wall of the tumbler. If I then invert the tumbler the water contained in it will not flow out, but remains completely shut up, although no means is employed to retain the air-plug in position. This precisely illustrates what takes place in plugging, and shows the action of the distended plug in opposing the escape of blood. It is further worthy of notice, that so closely does the caoutchouc adhere to the walls of the tumbler, that by pulling the tube attached to the air-bag, the tumbler is easily raised; and the result is the same, whether the tumbler be more or less filled, and whether it be cylindrical in shape, or conical like a wine-glass.

"232. By the use of these air-plugs we may entirely control all hemorrhages from the nasal fossæ, a circumstance explained by the solidity of their walls; but it is necessary to be careful that one extremity of the plug reach as far back as the pharynx; for, without this precaution, blood might continue to flow into the œsophagus.

"233. We must be prepared for a symptom which may occur when the plug is too much distended by insufflation. The patient complains of a painful sensation at the epigastrium, he grows pale, and the forehead is covered with perspiration. These phenomena continue as long as the plug preserves its exaggerated distension, but disappear as soon as a little air is allowed to escape, and the volume of the plug is reduced to more convenient proportions. To what cause may these symptoms be attributed? Partly, no doubt, to the fact that the air-bladder, by its excessive distension, occludes simultaneously both the posterior openings of the nasal fossæ; but the symptoms of dyspnœa and of threatening suffocation are probably not entirely dependent on mechanical obstruction to respiration; there is reason to believe that they are in great measure due to compression of the par vagum.

"234. Whence the important precept never to carry the distension of the air-bladder to excess, and never to leave the patient immediately after the application of the plug."

Those who are interested in the matter will find the various applications (compression, dilatation, confinement, plugging, &c.) of the vulcanised caoutchouc bags further referred to in the passages indicated by the following references:—

Gazette des Hôpitaux, 1849, No. 141.
Gazette Médicale, 1849, No. 45.
Lancet, Dec. 1, 1849, p. 579.
Brit. and For. Review, Jan. 1850, p. 269.

—*Med. Times and Gazette, July* 16, 1859, *p.* 64.

137.—*Hydrocephalus.*—[At various times tapping for chronic hydrocephalus has been tried, but very rarely with success; we have done it in two or three instances ourselves, but always without success.]

The statistics of the operation have been carefully considered by Dr. West, and the subject has been investigated by Dr. Battersby, of Dublin, Dr. Winn, and others, and all agree in the opinion that tapping is useless.

The per-centage of actual cures, as compared with the deaths, is exceedingly small.—*Lancet*, *Dec*. 25, 1858, *p*. 654.

138.—*Constipation in Children.*—Wherever, for the last few years, I have met with obstinate constipation in infants, I generally succeeded in giving speedy and full relief, by ordering some sweet sugar-water to be taken every day, besides the breast. I am satisfied that the chief cause of constipation in nurslings is the insufficiency of sugar in the breast-milk, the proportions of which are naturally not the same with every mother. Wherever caseine exceeds the proportions of the other parts of the milk, it becomes indigestible, by a relative want of lactic acid in the contents of the stomach and intestines.—Dr. JACOBI, *in New York Journal of Medicine*, *Sept*. 1858.—*Dub*. *Hospital Gazette*, *March* 1, 1859, *p*. 79.

MISCELLANEOUS SUBJECTS.

139.—A TEMPERATE EXAMINATION OF HOMŒOPATHY.[*]

No. II.—The Principles of Homœopathy, with a few Hints on the Nature and Cure of Disease.

By W. Braithwaite, Esq., Leeds.

In our last volume we endeavoured to explain "The Doses and Provings of Homœopathy." We will endeavour in this, our 40th volume, to point out the *Principles* of Homœopathy, and show that these principles are inconsistent with the true principles and practice of medicine, and dangerous to be trusted to when applied to the healing of diseases. We wish to examine this subject in a fair and temperate way, so as, if possible, to convince even the homœopaths by fair and simple arguments, how inconsistent their principles are with the modern science and art of medicine. We wish also to say that we only state our own views, which may probably differ from the views of numerous other medical practitioners. If any one, therefore, see fit to answer this pamphlet, let it be done by fair argument, and in a proper spirit, by keeping to the point, and not by flying off to other questions.

In our last pamphlet we asserted that the homœopaths used what are called dilutions in mixing their medicines, and that each dilution was represented by adding two ciphers to the original unit. Thus, the first dilution of a drop or grain was the 100th; the second dilution the 10,000th; the third, the 1,000,000th; the 20th, the 100,000,000,000,000,000,000th; and the 30th, or decillionth, the 1,000,000,000,000,000,000,000,000,000,000,000.000,000,000, 000,000,000,000,000,000th part of the original drop. We endeavoured to show that the Atlantic Ocean does not contain a decillion of drops, and that the physician might just as well drop a drop of laudanum or any other medicine into the sea, and, after being *well mixed*, take a few drops out and give them to his patient.

You frequently see in homœopathic writings that the *billionth* of a drop was used. What is a billion?—a million times a million. No man could count a billion. If you counted two hundred in a minute, this would be 12,000 in an hour, 288,000 in a day, and 105,120,000 in a year; it would take 9,512 years, at the above rate of 200 a minute, to count a billion! And yet this is only the 6th dilution used by homœopaths, viz., the 1,000,000, 000,000th of a drop or grain. Some, however, will dilute their drugs to the decillionth part of a drop. This no one will or can deny. It is a fact which any one who reads their writings may prove for himself. No man, we say, can count a billion; much more difficult would it be to count, or even to comprehend, a *decillion*. We give the following figures, which will show the various dilutions of homœopathic medicines, so that they who practice this art may know at once how much of the original drop of medicine is contained in the mixture which they are giving or prescribing:—

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[*] We hope that our professional friends will excuse the popular style in which this tract is written. It is not likely that the public could understand the subject if written in any other way. Our object being to enlighten the public on some of the simple laws or principles of medicine, in order to enable them to perceive the errors of homœopathy, this article will be published separately in a cheap form, and may be obtained (as well as the preceding pamphlet "On the Doses and Provings of Homœopathy,") in quantities at nearly cost price from our publishers, Simpkin & Co., London; D. I. Roebuck, Printer, Leeds; or through any bookseller.—No. 1. 2nd edition, price 2d., or 6s. for 50. No. 2, ice 4d., or 10s. for 50.

Decillion. nonillion. octillion. septillion. sextillion. quintillion. quadrill. trillion.
1,000000, 000000, 000000, 000000, 000000, 000000, 000000,
billion. million.
000000,000000,000000.

Now this puts the practice in so absurd a light, that the homœopaths try to get out of the dilemma in all kinds of ways. Some say they do not depend on doses; others say that these infinitesimal doses are rendered powerful by *shakings* and *triturations*, Hahnemann believed that infinitesimal division of drugs produced a real *spiritualization*. "Long trituration," he says, "if carried very far, will make the drug *quite ungovernable*." In another place he says, "by the succussion and trituration employed a change is effected in the mixture, which is so *incredibly great*, and so *inconceivably curative*, that this development of the spiritual power of medicines to such a height by means of the multiplied and continued trituration and succussion of a small portion of medicinal substance deserves incontestibly to be reckoned among the greatest discoveries of this age."—(*Lesser Writings*, p. 818.) But this, if possible, is outdone by Dr. Mure, who is described as the "Apostle of Homœopathy." The disciples of his school were obliged to subscribe to the following article:—"I believe that all substances in nature, even those regarded as the most inert, possess the power of acting on the vital dynamism, because all contain a *spiritual principle* which they derive from God."—(*Brit. Journal of Homœopathy*, vol. vii., p. 535. See *Dr. Simpson's Work*, p. 23.) We think it must be this *vital dynamism* which is alluded to by Dr. Craig in his late pamphlet; but as he has only slightly alluded to this extraordinary power, we hope that he cannot hold such a foolish theory. He writes, however, of the "*dynamic* or *vital agents*" with reference to homœopathic medicines, in such a way that one cannot wonder that those who call themselves *Homœopathists*, "incur (as he confesses) the enmity and ridicule which the very name provokes."

Another mode of accounting for the power of drugs, or rather another mode of communicating power to them, is by *Infection*. Hahnemann, in his "Lesser Writings," says, "Thus much is deducible from Korsakoff's experiments, that, since a single dry globule imbibed with a high medicinal dynamisation, communicated to 13,500 unmedicated globules, with which it is shaken for five minutes, *medicinal power fully equal to what it possesses itself*, without suffering any diminution of power itself, it seems that this marvellous communication takes place by means of proximity and contact, and is a sort of *infection*, bearing a strong resemblance to the infection of healthy persons by a contagion brought near or in contact with them."—(*Lesser Writings*, p. 859.) We need make no comment on this extraordinary theory. We leave every man of common sense to think as he likes of it.

The way in which other homœopaths defend the infinitesimal dose system is sometimes curious. One of their best writers is Dr. Dudgeon. In one of his letters, in defending the infinitesimal doses, he says, "Calculate the quantity of matter you leave on the ground in a five-mile walk, which shall reveal your track to your sagacious dog; the weight of the musk conveyed in an odorous billet that shall make a sensitive lady faint; the quantity of ipecacuanha that escapes from an uncorked bottle, that shall give some idiosyncratic individual a fit of asthma; the weight of the odour from a rose, that was wont to make an eminent cardinal swoon; the cubic contents of a fit of anger that shall give your irascible neighbour the jaundice," &c.—(*Med. Times*, 1850, p. 605.) We cannot perceive that this is any argument to prove the value or efficacy of an infinitesimal dose of laudanum. You *can* divide a drop of laudanum, so as to get the 100,000,000,000,000,000,000th, or the tenth dilution, but how can you *weigh* or *divide* a *smell* or *odour*, or *measure a fit of anger?* The thing is ridiculous; and very ridiculous to defend infinitesimal doses in such a way. And even if you could *weigh a smell*, we suspect the cause and effect would correspond, in the same way that the light of a star, which we see at an immense distance, corresponds exactly to its size, distance, and quantity of light. There might be a little more argument in Dr. Dudgeon's illustration, if he had previously proved, as facts, that the dog had traced his master from New York, when his master was at that moment on the Rocky mountains; or, that the ipecacuanha escaped from a bottle at Bombay, and gave the asthma to a member of his family in his own comfortable house in England; or, that the odour of the rose was emitted from Pekin, and struck down with its odour the cardinal at Rome. But even these effects would be insignificant when compared with the effects of the 100,000,000,000,000,000,000th of a drop of laudanum in disease.

Now that the modern homœopath has got himself into this ridiculous dilemma about his small doses, he begins to *back out* of this difficulty, and to say that the *principles* of his creed have nothing to do with small doses, that he can give any doses he likes without violating his principles. It is true that the *principles* of a science are very different from the *practice*, and that the principles can be worked in different ways, either well or ill: for example, a sailor may work the principles of astronomy very badly, and wreck his ship; another may work them well, and arrive at his port in safety. So one homœopath may work his principles by small doses, and another by large doses, and although each may work his principles well or ill, neither may really violate his principles. But how is it, when the homœopaths repudiate these ridiculously small doses—such as the 1,000,000,000,000th (billionth) of a grain or drop of cocculus in colica menstrualis; or the 1,000,000th (one millionth) of a grain of ipecacuanha; or the 10,000,000 (ten millionth) of a grain of belladonna; or the 1,000,000 of a grain of bryony; or the 100,000,000th of a drop of aconite; or the 100,000,000th of a grain of phosphorus—*that these are the very doses used by Dr. Fleischmann in the Vienna Hospital*, on whose reports the homœopaths lay so much emphasis, and spread these same reports far and wide as specimens of success? Here we can convict the homœopath of throwing dust into our eyes. He says, "We do not depend on these ridiculous doses," and in the next breath he says, "Look at the success of homœopathy in the Vienna Hospital—we cure more cases than the allopath by our mode of treatment!" The contradiction in these two assertions would be very unlikely to be found out, except by one who examined the medicines and doses used by Dr. Fleischmann. We have done so, and found out the so-called cures were said to have been performed by the doses we have just mentioned. We assert, therefore, that whatever the homœopaths may say to the contrary, *they do give and do depend on these ridiculous doses*, however they may deny this to the public. We must look to their published writings for an explanation of their practice, and not to what they say privately. Even our friend, Dr. Sharp, of Rugby, in his pamphlets, frequently alludes to these infinitesimal doses, and says that he himself most frequently uses the *one-millionth* of a drop, but has seen good effects from the 1,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000th of a grain or drop. when we occasionally heard it asserted by the homœopaths that these doses had nothing to do with the principles of homœopathy, we at once referred to their practice in the Vienna Hospital, and found the cases almost invariably treated by infinitesimal doses. We were, therefore, greatly surprised that the homœopath repudiated, in a great measure, these small doses, and in the same breath extolled their good effects. (The only excuse we could find for this contradiction was, that they had not studied their own *statistics*, and merely took for granted what other people said.) The reader will find a full report by Dr. Balfour of numerous cases treated in the Vienna Homœopathic Hospital by Dr. Fleischmann, in the British and Foreign Medical Review for October 1845, p.567, in which report the dilutions of nearly all the medicines used are accurately noted. Amongst numerous other cases, catarrh and bronchitis were treated with the 100,000,000th of a grain of sulphur, and with 1,000,000th of a grain of aconite; spasm with the 1,000,000,000,000th (billionth) of a grain of ignatia, once daily. We could go on multiplying these instances, but we have said enough to show that, when a homœopath denies or repudiates these ridiculous doses of medicines, he is not supported in his assertions by the best writers of his own school; they are nearly all against him. The Principles, therefore, of homœopathy, and the Practice, are necessarily two distinct things; but the practice of homœopathy, nevertheless, is based on the most ridiculous and absurd doses of medicine ever invented; and now that the thing is exposed, the homœopath is beginning to be ashamed of it himself. To such an extent is the infinitesimal dose system carried by some, that Dr. Balfour states, that in Vienna "one practitioner often contents himself with allowing his patient to *smell* the remedy, waiting patiently for some weeks or so, for the completion of the cure, not even permitting a second smell." Hahnemann, we are told, did the same: and one lady, "having been subjected to this process, *passed the fee before the doctor's nose*, and then—replaced it in her pocket." (*Dr. Lee's Homœopathy*, pp. 7, 8.)

Our chief object in this pamphlet will be to examine "The Principles of Homœopathy;" but, before doing so, we will first explain a little more at large than we had room for in our last pamphlet, our objections to the way in which the homœopathists endeavour to spread their doctrines, and endeavour to gain an advantage over regular physicians and surgeons. We blame no one for differing from us in opinion, but we do blame any man for endeavouring to spread his opinions in such ways as the homœopaths adopt.

It has been one of our greatest pleasures for the last twenty years to take almost daily notice of the progress of medicine in the various medical works which have been published. We have generally omitted noticing the *errors* of our fellow practitioners, for obvious reasons; at present, however, we must deviate from our general rule, and notice some of the errors both of doctrine and practice which have crept into our ranks. We shall endeavour to do this in a temperate way, so that none may find fault with us on this head. If a little humour or ridicule occasionally escape, we hope that people will take it in good part. We shall mean nothing ill by it; our object will be chiefly to defend our noble, humane, and scientific profession from assaults which have been lately made against it, more in a rude than in a scientific and gentlemanly way.

Few sciences or arts have more improved than has the Science of Medicine within the present century. But its career has been silent, and unobserved, except by the medical practitioner. We need not refer to the various improvements in the practice of Surgery, such as operations on the eye; restoring lost parts, such as the mouth, the palate: amputation of limbs; taking out diseased joints and joining the limbs together again; curing club-feet and all kinds of deformities; treating aneurism, by compressing the artery and without tying it; curing hernia radically, so as to save the use of a truss, and prevent the risk of strangulation; operating on the strangulated hernia without opening the sac; and innumerable other improvements, which every well-informed surgeon is aware of. We will more particularly refer to the improvements in finding out and treating internal diseases, and then ask if the modern science of Medicine deserves the "nick-name" sometimes given to it of "the old system." In our opinion there are no sciences or arts more *new* or modern. The modern practitioners of medicine by no means go by the old ideas, which are sometimes unjustly quoted by homœopathic writers in disparagement of living practitioners. Among modern improvements we may enumerate our increased knowledge in all diseases of the nervous system, viz., delirium of all kinds, convulsions, and paralysis; diseases of the chest, which, by the modern improvements in auscultation we can accurately distinguish the one from the other, foretelling from the nature of the sounds the progress of the disease, and its results to the patient; diseases of the kidneys, which have become much more clearly understood by means of chemistry and the microscope; diseases of the liver, which, by the microscope and testing the urine, we can much more accurately define; diseases of the blood also have become more intelligible and amenable to treatment, besides many others which we have not space here to mention. Whatever homœopath, therefore, alleges against the modern practice of medicine, that it is the "old system," is evidently no reader of modern books, but of old ones. He is being left behind by his reading competitors, and trusts to the knowledge which he has picked up in days long gone by, when a young student under what he would call "the old school."

If he had even turned to one of our last Retrospects, vols. 38 and 39, published only twelve and six months ago, he would have found numerous articles by different writers, in nearly every one of which there is some new and improved idea and method of treating diseases. He would have found something new on Cancer, Rheumatism, Scarlatina, Myalgia, Neuralgia, Tetanus, Nævus, Asthma, Diphtheria, Pneumonia, Diabetes, Uric Acid or Gravel, Chloroform in Labour, the Womb, Ovarian Dropsy, Diseases of Women, Carbonic Acid as an Anæsthetic, with numerous other very interesting observations. If moreover, he will turn to the present and several preceding volumes, he will find recorded *every year* nearly 300 suggestions and some decided improvements in the theory and practice of medicine, by various authors,—some very valuable and remarkable improvements.

We mention this, in order that the public may give due credit to the progress of medicine, and not be led away by homœopathic writings which attempt to mislead people to suppose that scientific medicine is at a *stand-still*, whilst the homœopathic system is said by them to be advancing. We shall see as we go along, whether it be honourable and decorous in homœopathic writers perpetually attempting to impress upon people that theirs is the *new system*, whilst ours is the *old system*. We have been particularly struck with this unjust nick-name given to regular physicians of high character and long success in their particular localities; and we would recommend the homœopath, if he wishes to gain any respect, to depend on his own knowledge, and not to make reflections of an injurious and unjust character on gentlemen not to be surpassed in practical knowledge and success.

It is another great mistake to call a regular, or rational, or scientific practitioner of medicine, an *allopath*, or *antipath*. It is another *nick-name*, which no right-minded man ought to give to another. The homœopath has given himself his own name, and therefore he must take it; but there is no such person as an *allopath* or antipath in regular practice. Allopathy, which we shall use in preference to antipathy, includes the method of opposing nature in her efforts, but this is not true of any one. Another mistake made by homœopaths, and a more discreditable one cannot be made, is to depreciate other medical men in the estimation of the public, by attempting to throw discredit on the practice of medicine adopted by other people, always excepting themselves—after picking holes in other people's coats and making them as ragged as possible—they adroitly display themselves in their own new dress. We have been particularly struck with the way in which they have made extracts from the writings of regular physicians, nearly always selecting their candid confessions of failure in certain cases, and never exhibiting the other and brighter side of the question, viz., where they have succeeded; or perhaps they have selected passages from books so as to make the science of medicine as objectionable as possible, but the inaccuracy of which the unprofessional reader cannot perceive. What would be thought of a clergyman, educated at Cambridge or Oxford, and having received his degree and honours there, and been ordained by his Bishop—who seceded from the church, and not only stated that there was no way of salvation within her pale, but who took every opportunity (although still possessing, and being proud of, his degree of A.M., or D.D.) of finding all sorts of fault with the college education, the principles taught, and the religion entertained; especially if his abuse of others was suspected to add to his own pecuniary advantage—would he not be *cut* by his former associates? Or what would be thought of a provision dealer, if he issued placards and advertisements that he had found out an improved method of preparing flour, that all the flour in the town, except his own, was adulterated with an inferior article, and moreover contained poison in large doses—would he not also be *cut* by his fellow tradesmen? So it is in our profession. We find no fault with people entertaining different opinions; but when we see all kinds of ways of making invidious distinctions between themselves and others, mixing their medicines in particular ways so as to be something *new*, publishing and asserting that regular physicians exhibit medicine in large and poisonous doses, with various other ways of gaining notice, we think it derogatory to a respectable and learned profession. But it is particularly unfair, and untrue, to allege that regular physicians employ strong poisons as medicines, and give such large doses that the constitutions of their patients become injured—whereas they, the homœopaths, give such small doses, that all this danger is avoided. We must assert that all this is a system of proceeding highly discreditable.

We could *disgust* every one of our readers by telling them of some of the remedies which some homœopaths use. But we consider that these remedies are used only by some, and can be no proof of the general practice of the body. We should not think, therefore, of using these prescriptions in an argument against homœopathy, neither is it fair in the homœopaths to pick out some of the most objectionable old prescriptions from old books, and bring them forward as specimens of what they call the *old system*, without at the same time candidly telling the public that this kind of practice has long since passed away from the ranks of all well educated surgeons and physicians. As an illustration of the way in which the practice of regular physicians is shown by the homœopaths, you have only to turn to the pamphlet of Dr. Horner, of Hull, who states at p. 5—"Vegetable drugs, as aloes and colocyth, which had been administered even *some years* before, remained deposited in the system, producing a miserable and wholly shattered state both of mental and bodily health." But this is outdone by Dr. McLeod, of Benrhydding, Ilkley, who states, that "Such drugs as aloes were extracted tangibly and washed out of the compresses that had encircled the body: it also made the patient's room offensive with the distinct vapour of aloes." (*Dr. Horner's Pamphlet*, p. 53.) This is really too bad! We naturally read a little further, expecting to find the next wonder to be that this vapour of aloes had been condensed and again made into the identical family pills which the unfortunate patient had been swallowing; and that they were actually to be seen exposed as a curiosity in one of the windows of Benrhydding!

The medicines employed by modern physicians and surgeons are not to be called poisons; for when poisonous medicines, such as arsenic, opium, belladonna, aconite, mercury, &c., are used, they are prescribed in such small doses that they no longer act as poisons. It does sometimes happen, that medicines act more energetically than was anticipated, but this is a rare exception; and it is wrong to take these rare exceptions and spread them before the public as the rule. The object of the homœopathists is, we are sorry to say, a very discreditable one: it is evidently to excite the fears of people that they are always taking poisons when attended by regular surgeons and physicians—a very wrong idea indeed, but one which we feel certain they are anxious to propagate. The public, however, may rest satisfied that the modern system of medicine in the regular ranks of the profession, is to do with as *little*, and not with as *much* medicine as possible; with as *mild*, and not with as *strong* medicines as possible. The reader has only to refer to the writings of Dr. Sharp, of Rugby, if he wishes to be convinced of the truth of these remarks. Amongst other things, he will find in one of his Tracts, a *wood engraving* to illustrate the way in which hot irons are prepared to be applied to the patient in surgical cases! This engraving would remind one of the tortures of the Inquisition; but how unworthy to exhibit these things to the public in order to excite their fears and prejudices. Why cannot a respectable man, like Dr. Sharp, whom we have known for thirty years, make use of fair argument, without first attempting to damage his opponents in public estimation. We might think it excusable in Morrison, Dr. Coffin, and such like men, but for respectable and well-educated men to do this is highly unworthy.

Another objectionable practice of the homœopaths is to publish Statistical Tables, which, if not incorrect, are at any rate so placed before the public as to mislead. For our own part, we think that most statistical tables may be fallacious, except those of the Army and Navy, which must be the most correct of any statistics of this kind which are published. The error of these homœopathic tables is at once seen by reference, for example, to the Statistics of the Leeds Infirmary. One homœopathic writer, Dr. Craig, of Leeds, asserts that the per centage of deaths in all diseases in Homœopathic hospitals is 5.8 per cent., while in Allopathic hospitals it is 12.4 per cent. Moreover, this writer, in a feeble attempt to answer our first pamphlet (but which is no answer at all), publishes this error at the end of his pamphlet, together with his account of the officers of the London Homœopathic Hospital, all of which had nothing to do with what he was writing about, but which were placed there in the way of display. If this said writer had examined the Report of the Leeds Infirmary for the last year, he would have found the following figures: "Patients admitted since 1767, 247,911; cured, 165,639; died, 8161; relieved, 30,723." So that we have about 8161 deaths out of 247,911 patients. We make this account to give 3.2 per cent. of deaths; but making allowance for mistakes, we think that we may calculate about 4 per cent. of deaths, *and not 12.4 as Dr. Craig asserts!*

We believe all the hospitals of Great Britain will not average much above 5 per cent., including all accidents and dangerous surgical cases. One of the most dangerous operations on the human body is cutting for stone. The average per centage of deaths after this operation is only about 13; so that if every patient admitted into all the hospitals of Great Britain and Ireland had been cut for stone, as many would have recovered as Dr. Craig makes out from common and ordinary diseases.

We are quite aware that it is difficult at all times to arrive at a correct conclusion respecting Hospital statistics, if we include out-patients. It is very possible in such an hospital as Fleischmann's Homœopathic Hospital, at Vienna, for the cases to be selected, as we know to be the case. In general hospitals, such as the vast hospitals of Vienna and Paris, only the *worst* cases, both medical and surgical, are selected for admission, often quite incurable from the commencement. It is obviously unfair, therefore, to calculate the per centage of deaths among the *in*-patients and exclude the *out*-patients; both *in* and *out*-patients ought to be reckoned. But here is the difficulty ;it is easy to calculate how many die, or are cured in the hospital, but it is not so certain when we refer to the *out*-patients. The calculation of the Leeds Infirmary is of both *in* and *out* patients. The statistics of some of the hospitals of Vienna, Paris, and London, and indeed of all *in*-patients of hospitals, show a much higher per centage of deaths, for the reasons which we have stated, but we repeat that this is not a fair calculation. It only respects the cases which are selected *as the worst* which apply for relief, and generally excludes the milder *out*-patients; we believe, however, that taking the whole together, and including all accidents, and surgical operations, the average will not be above 5 or 6 in British Hospitals. In our next pamphlet we will endeavour to calculate this as well as we can. Such an institution as the Leeds Public Dispensary, is, perhaps, as good an example as can be used to show the per centage of all kinds of cases, good and bad, medical and surgical. These cases are prescribed for in the institution, and the bad cases are visited at their own homes, so that the register of deaths must be pretty accurate. We find in the report for 1858 the following figures :—"Patients admitted since 1824, 111,100; cured, 90,156; greatly relieved, 5750; died, 5476." This gives us 4.02 per cent. of deaths, a very different thing from what the homœopathists would wish the public to believe. Dr. Craig further carelessly states, that in Homœopathic hospitals only 5.7 per cent die of inflammation of the lungs, while 24 out of every hundred die in Allopathic hospitals. We only request Dr. Craig to examine one of the last official Army returns to the Colonies. Of 12.271 cases of inflammation of the lungs, he will find that only 413 died; this is about 3 per cent., and in some cases only 2 per cent., i. e., less than *half the number* which Dr. Craig himself acknowledges die in Homœopathic hospitals! What, therefore, can the public think of Dr. Craig's statements? In our last volume is an interesting paper on lithotomy, abstracted from the lectures of Mr. S. Smith, of the Leeds Infirmary: out of every 8 cases operated upon for stone, only one has died; this gives us about 13 per cent. If this gentleman had operated for stone on all the 247,911 patients admitted into the Leeds Infirmary for the last 93 years, he would probably have had no more deaths than what Dr. Craig says take place from common diseases.

We requested Dr. Blakelock, the Secretary of the Leeds Infirmary, to calculate the per centage of deaths in that institution, and the following is his letter to the author:

LEEDS GENERAL INFIRMARY,
November 5th, 1859.

"My Dear Sir,—According to your desire I have calculated the average per centage of Deaths occurring amongst the patients of the Leeds Infirmary. The whole number of patients admitted during the 93 years which have now elapsed since the foundation of this institution has been 252,937, amongst whom there have been 8276 deaths, giving an average of 3·27 per cent. During the first 12 years the average mortality was 5·1 per cent. During the last 12 years, it has been 3·33 per cent. The very slight increase of the last figures over the general average is accountable for by the great mortality which occurred after operations during the summers of 1852, 53, and 54. It must be borne in mind that these figures include a large number of deaths from serious burns and mill accidents, many of the patients dying within a few hours of admission. If these could be excluded, the mortality would be still farther reduced. No register is kept of the number of patients admitted for each disease separately, so that the per centage of deaths in pneumonia cannot be ascertained. I, however, find that only four patients are entered in the death-book as having died from pure pneumonia during the last four years.

"I am, dear Sir, very truly yours,
"W. Braithwaite, Esq." "SAML. BLAKELOCK, Secretary.

We think this statement of Mr. Blakelock's highly creditable to the officers of this noble Institution, and will be gratifying to its numerous subscribers. We hope also that it will silence the attempts of certain individuals in Leeds to cast reflections on the modern practice of medicine. We wish and hope that it may also gratify them; but of this we are more doubtful. We are afraid they would have been better pleased to have had it proved that the practice had been very unsuccessful, and that 12 out of every hundred had died instead of little more than three. We are aware, that in all the hospitals at Vienna, *there is very great fatality*, amounting probably to what Dr. Craig states. Why does he not candidly tell the truth to the public, and acknowledge that these tables do not refer to the hospitals of Great Britain, nor to the Army and Navy. Why does he leave the public to believe that our own hospitals are included in these statistics?

We will now give the reader the words of Hahnemann on the real Principles of his creed, and proceed to make some remarks on this question.

"A weaker dynamic affection is (says Hahnemann) permanently extinguished in the living organism by *a stronger one*, if the latter, whilst differing in kind is similar to the former in its manifestations." Again, "All the rapid and perfect cures that nature ever performed were always effected by the supervention upon the old disease of one of a *similar* character."—(*Organon*, p. 165.) And hence he says "for this mode of procedure (by Homœopathy) we have the example of unfettered nature herself, when to an old disease there is added a new one similar to the first, whereby the one is rapidly and for ever annihilated and cured." (p. 175.) "The stronger disease annihilates the weaker." (p. 145.) In another place, he says, "In order that the artificial diseases producible by medicines may effect a cure, it is before all things requisite that they should be capable of producing in the human body an *artificial disease as similar as possible* to the disease to be cured, in order, by means of this similarity, *conjoined with the somewhat greater strength*, to substitute themselves for the natural morbid affection, and thereby deprive the latter of all influence upon the vital force." (p. 133.) In order to accomplish the end above described, certain medicines are to be chosen which will produce *symptoms as similar as possible to the disease to be treated.* The homœopathic mode of treatment threrefore is that "in which there is employed for the totality of the symptoms of the natural disease, a medicine capable of producing the *most similar symptoms possible* in the healthy individual." (p. 175.) But in order that this medicine should be efficacious and more powerful than the disease, it must at first, for a longer or shorter period, produce an *aggravation* or *exaltation of the symptoms of the disease*, and not their relief or abatement. The disease indeed ought to be made worse at first. The drug must first produce "a medicinal disease somewhat *stronger* or *greater* than the malady to be cured." (p. 237.) In other parts of his work, Hahnemann says, "The similar artificial diseases excited by medicines are stronger than the natural disease." The physician "produces a disease very similar but *stronger*" than the natural disease. "The curative power of medicines therefore depends on their symptoms, *similar to the disease*, but superior to it in strength." (p. 126.) So that a disease can be removed "solely by one that is similar *in symptoms* and is somewhat *stronger*, according to the eternal, unchangeable laws of nature." (p. 151.) So that in a severe, dangerous, and rapid disease in which life depends on the next few hours, and any increase of which disease must *necessarily* be fatal, the symptoms ought at first to be increased in severity. A homœopathically selected remedy, Hahnemann observes, "usually, immediately after ingestion, for the first hour or *for a few hours*, causes a kind of slight aggravation (where the dose, however, has been somewhat too large *for a considerable number of hours*) which has so much resemblance to the original disease that it seems to the patient to be an aggravation of his disease. But it is in reality nothing more than an *extremely similar medicinal disease*, somewhat *exceeding* in *strength* the original affection." (p. 237.) Hahnemann further says that "the sum of all the symptoms in each individual case of disease must be the *sole indication*, the sole guide to direct us in the choice of a curative remedy." (pp. 119, 120,) "By the removal of the whole of the perceptible signs and symptoms of the disease the sum total of the disease is at the same time removed." "It must be the *symptoms alone* by which the disease demands and points to the remedy suited to relieve it, and, moreover, the totality of these its symptoms must be the principle or sole means whereby the disease can make known what remedy it requires, the only thing that determines the choice of the most appropriate curative agent, and thus, in a word, the *totality of the symptoms* must be the principal, the *sole thing* the physician has to take note of in every case of disease, and to remove by means of his art." (p. 113.)

Now, when we read the above principles of homœopathy, we are struck with two or three remarkable fallacies which might mislead any one who had not studied the nature of disease. The great error in these principles is to mistake symptoms for disease, the effects for the cause. A patient has a violent pain in the face, it is called tic-doloureux—the cause of this is a decayed tooth. The symptoms are the tic, the cause is the tooth. Another patient has a violent headache, or dizziness from a loaded stomach—the headache is merely the symptom, the cause is the loaded stomach. To treat the pain in the face by giving medicines which would resemble tic, would be called an empirical mode of treatment, *i.e.* treating only symptoms—to extract the tooth would be eradicating the cause. To treat the headache in the same empirical way would be nonsense, but to unload the loaded stomach would be treating the cause radically. Another great error in the above dogmas or principles of homœopathy, consists in generating a more powerful set of symptoms similar to the disease, in order to overcome the original disease. This is a dangerous error, and may account for the startling fact, that the homœopaths seem to have considerably more deaths, according to their own acknowledgment, than regular surgeons and physicians, if we may judge from the statistics of the Leeds Infirmary.

In order, however, to discuss this question properly, it will be necessary for us to explain in as familiar a style as we can what are the *modern views of disease*, which are gradually becoming better known; and also what we mean by a *principle*. As we go along, the reader will better understand how the homœopath mistakes a principle for an art, and a disease for a symptom. The following ideas on the nature of disease are explained in a familiar style, and some of them may be deemed not satisfactory by the profession. We acknowledge that the subject is a difficult one, and that all have a great deal to learn on this question; and we think that the day is not distant when this very interesting subject will be worked out more clearly. It is very probable that our opinions may be much criticised by some of our professional friends. This we are prepared to meet with: but our object is not only to show the errors of homœopathy, but to endeavour to point out where, in our opinion, the true principle of medicine exists. We shall endeavour to show that in most, if not all diseases, there are, 1st, the disease itself; 2ndly, the effects and *necessary* symptoms or consequences of the disease; and 3rdly, another and quite different set of symptoms, *which are not always nor necessarily connected with the disease*, and which are the *efforts* of *nature*, either too strong or too feeble, or just right, to rectify the disease. The error of homœopathy consists in simulating and even aggravating the second class of symptoms, or those which are *necessarily* connected, and sometimes said to be *identical*, with the disease. The object of Modern Scientific Medicine is to regulate the third class of symptoms.

What, then, is Disease? It is on the definition of this important question that most of the errors in practical and theoretical medicine have arisen. It is in ignorance of the nature of disease that the system of homœopathy has arisen. It is this ignorance of disease which induces the homœopath to treat *symptoms* of disease instead of the disease itself. In this consists his great mistake, as we shall endeavour to show; but in attempting to point out this mistake, we shall be compelled to make some preliminary remarks, in a familiar style, in such a way that the public may understand us. These remarks would be quite unnecessary to educated medical men, but we must remember that pamphlets of various kinds have been distributed by the homœopaths to the public, which seem very plausible, but which, in our opinion, are very unsound both in argument and facts.

It is alleged by the homœopath that the scientific or regular physician has no principles to guide him, but that in homœopathy there is a certain and infallible *principle*. A *principle* means a *truth* which is unerring. A stone is thrown up into the air, and it invariably falls down, owing to the principle or truth of gravitation. We have always day and night, owing to the *principles* of astronomy. To be a real principle, it must never fail. If homœopathy be founded on a true principle, it ought never to fail—all its cases ought to recover if treated properly, and if they do not recover it is owing to their having been treated improperly. If you point your ship to New York by the guide of the magnet it will surely arrive at the spot: it will not sometimes arrive at New York, and at other times at Halifax—it will never fail you. If you arrive by mistake at Halifax instead of New York, either your magnet is wrong, or you have not followed its principle. If the homœopath says he has found out a true principle of medicine, but frequently fails in his cases, either the principle is wrong, or he has mismanaged it. If he answer that the principle is right, but that circumstances occurred which prevented him following it, we should say that the principle is not worth much if it be not trustworthy in a storm as well as a calm. If the sailor tells us that he arrived at Halifax by mistake instead of New York on account of the storms, we should say that his magnet must have been of very little use. He was obliged to manage his ship according to circumstances, and to trim his sails according to the storm; his *principle* of navigation was of comparatively little use—his *art* of sailing was of more use, and his *art* preserved his ship from destruction.

We consider that the whole practice of medicine depends on an accurate knowledge of what is *Scientific* in its principles, and on how far *Art* can carry out, or work on, these principles. We can best illustrate our definition in a few words by pointing out that the *art* of navigation is founded on, or the working out of, the *principles* of *astronomy* and the *magnet*. The sailors of old had few or no principles to guide them; they navigated their ships from Greece to Italy, or from Tyre to Carthage, by the *art* of navigation chiefly. They understood the position of the stars, and the sun and moon, and these were their chief guides, but they depended also on the shores and mountains along which they passed. Astronomy was in its infancy, and the magnet was not known. Navigation, in fact, was an *art*. By and by, however, the magnet was discovered—its principles were found certain and infallible, it could not mislead. The sailor could now boldly plunge his ship across the Atlantic with unerring course; his *art* became founded on a *science*, or a *principle* which was infallible. His art might vary in the following of this principle, but he always kept his magnet before him. The man at the wheel was steady at his post, although the sailors were busy with the sails. The man at the wheel was guided by a *principle*, the sailors during the storm were guided by an *art*. The principle or science could not vary; the art might vary according to every direction of the wind. So it is in the *science* and *art* of medicine. The *science* of medicine is difficult to discover; it is only just now beginning to be seen. The *art* of medicine has long existed, but only as an *art* founded on experience.

One great error which has blinded the minds of medical men in observing the true principles or science of medicine is in confounding *symptoms* of disease with the *disease itself*. Thus, a man has a bit of dirt in his eye, perhaps a bit of sharp sand blown on his cornea by the wind; he cannot displace it himself—what occurs? The eye begins to inflame, and looks red and angry; inflammation comes on and the eye looks *diseased*. But is this disease? No; it is only the *symptom* produced by the bit of sand—the sand, in fact, is the real disease, and the inflammation is nothing more than an effort of nature to get rid of the bit of sand. The inflammation (i.e., the falsely-called disease) in short is a *conservative process*. It proceeds somewhat as follows: the blood rushes to the spot where the bit of sand is imbedded with such force that the blood-vessels around it are completely *clogged up*, or blocked up with red blood-corpuscles. You might as well have sent a whole army of red-coated cavalry at a gallop to pass at full speed along the Strand, or through Temple Bar blocked up by an omnibus—they cannot do it, they are stopped by the narrowness of the street, which becomes more and more narrow, till it ends in the omnibus blocking up the gateway. They come to a dead stand, although the soldiers in the rear are still pressing on. The crush becomes so severe that the soldiers in advance are at last so jammed up that they *perish*. It is somewhat similar with the red blood which is jammed up all round the bit of sand—the blood perishes, and the parts immediately around the bit of sand die also; being dead, they lose all hold of the living parts, they decay and drop off, but in doing so they bring away the bit of sand, and thus rid the eye of the source of irritation. The hole is then filled up by new and living structure, and the eye is perhaps no worse for the process. But the whole thing illustrates what we should call *the wonderful processes of disease*. In short, we cannot look upon it as disease at all. We perceive the same thing occurring all through the animal economy, although the source of irritation may not be a bit of sand, but some other foreign body, or poison, or depraved or perverted secretion, or excretion—such as the poison of fever, gout, rheumatism, and many other diseases. Instead of the bit of sand in the eye, suppose that the source of irritation is a quantity of sand in the blood, called gravel or lithic acid. Now the kidneys are the great cleansers or depurators of the blood. Their office is to keep the blood free from certain things which get into it, and which otherwise would impair and even destroy its usefulness. Amongst other things, lithic acid or gravel is frequently formed in this blood, and the kidneys set to work to get it away. But they often fail in doing this work, and the acid accumulates in the system. What is to be done? The skin would be willing to do its part to take it off, but the patient is unwilling to go through the exercise in the open air to enable the skin to act, and it refuses to act whilst the patient remains in the arm-chair or study; so the joints or the great toe come forward and offer to do the work, much to the joy and relief of the kidneys. The toe inflames and swells, and at last gets well. The patient is now quite relieved, and is better than he has been for months. He has been relieved of some *peccant humour.* This you will find deposited round the joint of the great toe. It is swollen; cut into it if the patient will consent to the experiment, and you will find the sand, which just before was in the blood, and making the man ill; it is in the form of a chalky product, (lithic acid combined with an alkali), which could be detected in the blood previous to the attack, but which has now disappeared from the blood, and found a place of rest round the toe. The same kind of irritation is found in several other parts of the body, especially in what are called mucous and serous membranes, which are evidently doing their best to assist the kidneys in their work. But do you call this *disease?* We call it *a wonderful vicarious process by which nature is attempting to get rid of something which is vitiating the blood.* The *symptoms* of gout are not the disease; the disease is that which is poisoning the blood. We at once attempt to cure, not by producing *symptoms* by means of medicine as similar to those of gout as we can, but by exercise, and fresh air and diet, and also by giving alkalies, such as potash, which destroy the acid in the blood. We do not give medicine to simulate the pains of gout or rheumatism, but to counteract the poison at its source; just the same as in the eye we pick out the bit of sand, so in gout we pick out the sand in the blood, which we cannot do with a lancet or needle, but which we can do with alkaline medicines, which act remarkably on the acid which causes gout.

We are attempting now to illustrate the error of mistaking *symptoms* for what is called *disease,* and the error which runs all through the system of homœopathy, of pretending to generate a set of symptoms, which may be common to diseases of the most various kinds. We may have similar symptoms, very difficult to distinguish from each other, in functional diseases of the nervous system, and in those which are more inflammatory; in diseases which require good diet, and in those which require low diet; in diseases, in short, whose *symptoms* are alike, but whose treatment ought to be quite dissimilar and opposite.

Quite different diseases cause symptoms exceedingly alike. These diseases are not only quite unlike each other, but require quite different treatment. Now it is clear that, on the homœopathic principle, if you can by means of medicine, generate similar symptoms to a given disease, this medicine is the remedy. But how can it be the remedy for the two different diseases of worms in the bowels, and Bright's disease of the kidney? By referring to a paper in this volume, page 60, by Dr. Heslop, Professor of Medicine, Queen's College, Birmingham, the reader will find a description of the symptoms of worms, as connected with the nervous system. We find, amongst other symptoms, "pale complexion, obstinate headaches, general neuralgic phenomena, senses dulled, skin dry, thirst, &c." Dr. Heslop then says: "There is the strictest similitude between the cerebro-spinal symptoms of some forms of Bright's disease and those attendant on worms—particularly the tape-worm. So close is this likeness, that in cases where I should otherwise have predicted the presence of worms, my mind has been suspended in doubt, until after the urine had been subjected to scrutiny." It is well known also that worms will produce other symptoms, such as of hooping-cough, convulsions in children, hysteria with violent jactitations or profound repose, sudden insensibility without convulsions, violent laughter—all of which symptoms are relieved by getting rid of the worms. M. Wawruch, of Vienna, has noticed 206 cases of tape-worm. The following is his summary of symptoms:—"Dull pain in the forehead, giddiness, buzzing in the ears, dullness of the eyes, dilated pupils, emaciation, alternate loss and excess of appetite, cravings for particular articles of food, itching of the nose and arms, grinding of the teeth, &c., &c." Now, on the homœopathic principle, Bright's disease of the kidney ought to be treated by medicines which produce nervous symptoms, similar to worms; and worms ought to be treated by medicines which will produce nervous symptoms, similar to Bright's disease of the kidneys! But the regular physician goes to the root of the matter at once, with respect to the worms; instead of producing similar symptoms, he gives medicines which get rid of the worms, and the symptoms cease. A child or woman who had been treated homœopathically for such symptoms as are caused by worms, might go on for ever in this way without relief; whereas a single dose or two of turpentine, or kousso, or male fern, might relieve the symptoms at once. Every candid homœopath must at once see the force of this argument, which could be continued in numerous other cases. The argument may be summed up in a few words, thus—Where two or more diseases of diametrically opposite natures have particular symptoms almost exactly resembling each other, and yet are acknowledged to require quite *different* modes of treatment, it cannot be correct to treat them both in *the same way*, by generating symptoms similar to each. This is only one example amongst numerous others, of the unscientific and even absurd practice of treating symptoms, instead of the disease which causes those symptoms.

To assist nature to throw off a disease, does not consist simply in giving a medicine which in health would generate symptoms similar to the disease. This is a curious and serious mistake, which might appear plausible in some instances, but which in other instances would be foolish. For example, a man is attacked by a common cold. Now what are the *symptoms?* Shivering, or a cold skin, running from the nose, sneezing, &c. Now if you give medicines which in health would generate similar symptoms, you do not assist nature in curing the case; but if you have observed how, in another case, nature has cured, viz., by perspiration, &c., and if you thus imitate her, and generate a sympton which does not belong to the case, and which, in fact, is no symptom of a cold at all (perspiration), you easily cure your patient. We merely use this simple illustration, which might be extended to numerous other examples, to show that, to give a medicine which will, either in health or disease, generate symptoms similar to a disease, is a very unscientific method of following out nature's mode of cure. Take another example, which, however, can only be understood by medical men, but which, we hope, the homœopathic physician will candidly consider. We allude to a case of disease of the mitral valve of the heart. The disease is in the valve of the left side of the heart; the blood is either obstructed in its course forwards, or is thrown backwards, and thus regurgitates. What are the symptoms? The peculiar systolic bellows murmur, most distinct towards the axilla, difficulty of breathing, lividity of the skin, intermittent pulse, a bilious or yellowish skin, with scanty and high-coloured urine. Now, let the homœopath try to generate such like symptoms in a healthy man, and then give the medicine in this case in order to cure or relieve his patient. Let him give a medicine which, in health, would cause difficulty of breathing, blueness of the skin, intermittent pulse, a yellow skin, or scanty urine, and see if he can relieve his case. He will utterly fail. On the other hand, what is the way in which nature will sometimes endeavour to relieve all this venous congestion? The secretions from the bronchial surfaces become loose and copious, mucus is copiously expectorated, bile finds its natural outlet into the bowel, watery purging may come on, and the kidneys, being relieved of their congestion, begin to secrete urine. Now, all these are not symptoms of the disease, for they may exist in many other dissimilar diseases. They are no more symptoms of the disease than the perspiration was a symptom of the cold. They are the efforts of nature to relieve both *the disease and the symptoms*, and we can assist her very materially. Give a single grain each (provided the urine is not albuminous) of calomel, digitalis, and squill, to act on the bronchial surfaces, the liver, and the kidneys, to be repeated occasionally either with or without the calomel, and at the same time give what is called a watery purge, composed of a little jalap and cream of tartar in a large quantity of cold water, or a small dose of Epsom salts in cold water, and you will relieve the patient in a rapid and extraordinary manner. The object is to relieve the venous congestion which exists all over the body, and you can accomplish this by very simple means, and almost always with success. We have here a beautiful example of the way in which the physician can assist nature—not by increasing or generating or simulating *symptoms*, but by imitating the way in which nature would relieve the patient if she could, and the way in which she does sometimes succeed. Let us, therefore, study these beautiful laws of relieving disease. Let us take nature, or the way which the Almighty has pointed out to us, as our example, and not violate her teachings, as is the way of some, or simply trifle with them, as is the way of others.

The common symptom of disease called vomiting, will serve again to illustrate how fallacious it would be to treat a *symptom* of a disease for the disease itself. A patient may be affected with vomiting from numerous causes; he may have taken too much, or improper food; or his stomach may be inflamed, so that it will not bear the contact of food; or the man may be seized with a fit of syncope (fainting); or he may have a disease of his brain, near the origin of the nerve which supplies the stomach with the power of movement; or he may have water or pus form in, or upon his brain; or, if in a female, she may have disease or something wrong in the ovaries or uterus. All these things may cause the same symptom which may be the most prominent symptom in the case; but simply to allay the vomiting would not be to cure or to treat the case. To treat these different diseases homœopathically, you must give a medicine in proper doses which, in the healthy man in certain other doses, would cause vomiting, such as ipecacuanha; but, to treat disease of the brain in this way would be a most dangerous nullity, it would be simply trifling with the case. Or, to treat a case of vomiting or sickness in the female by simply attending to that symptom, and neglecting the uterine or ovarian irritation, would be trifling with the feelings of the woman; and to simulate this irritation or disease of the womb or ovaries by giving medicines, would be not only impossible but absurd. Or, if the vomiting depended on cancer of the pylorus, or on pressure of the left portion of the liver on the outlet of the stomach, would it not be equally improper to give medicine to simulate the symptoms, and impossible to give medicines to simulate the diseased pylorus or liver? We mention these things in passing, which ought to shake the confidence of the firmest believer in homœopathy, and make him suspect that his principles are erroneous and dangerous, when relied upon in the treatment of a disease. He may thus be led away to treat symptoms only, and entirely to neglect the disease which gives rise to those symptoms.

A very interesting and very decided proof that the principles of homœopathy are erroneous, is the effect of the Woorora poison as an antidote to strychnine and a remedy for lock-jaw. This virulent poison, which is used by the Indians to poison their arrows, has been used successfully on the human subject in a case of lock-jaw or tetanus, by M. Vella, in the Military Hospital at Turin, during the late Italian campaign. He dissolved two grains in nine drachms of water, and applied this solution to the wound which caused the lock-jaw. This remedy produced a complete relaxation of the muscles. It is a direct sedative of the motor nerves, and an antidote not only to lock-jaw produced by disease, but also to strychnine, which causes symptoms almost exactly similar to lock-jaw. This will be seen by referring to Dr. Harley's paper in the Lancet for 1856, vol. I., pp. 619, 647, and in our Retrospect of Medicine, vol. 34, p. 30. Here we have a remedy, the effects of which we can see and appreciate at once—a remedy directly allopathic, or antipathic, producing symptoms not like, but as unlike as possible, to the disease. Lock-jaw, or tetanus, *tightens* or *contracts* muscles; Woorara poison relaxes them.*

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* For an account of the treatment of tetanus in the Turin Hospital, see Lancet, Oct. 1, 1859, p. 345, and to the 40th (the present) vol. of our Retrospect, p. 37.

It is on the same principle that we can often, by remedies, rapidly cure an attack of asthma; take the following case reported by Dr. Salter:—"A poor woman was brought into King's College Hospital, supposed to be dying, quite unable to move or speak from the violent action of the respiratory muscles. The suffocation was of the asthmatic kind. I at once administered chloroform. After a few whiffs, the spasm began to yield. *In ten minutes after entering it she left the hospital well.*" (*Brit. Med. Jour.*, *Oct.* 1, 1859, p. 794.) Now here is another case, in which we can see and appreciate the effects of a remedy. The woman is attacked with an affection in which the nerves and muscles of respiration are in a high state of excitement, almost amounting to spasm, and a direct sedative, which relaxes spasm, is given to her, and acts instantly. It is the same as if a man was tightening a rope by pulling it, and some one instantly laid hold of the same rope and *slackened* or relaxed it. The homœopathic principle would be to tighten or stretch the rope in order to relax it (which in fact is a ridiculous Irishism); but when you want to relax it, is it not better to do so at once? In both tetanus and asthma, the muscles or ropes of the body are in a high state of spasmodic tension, and we have valuable remedies to relax them very rapidly. Even the effect of ipecacuanha in asthma and vomiting, which is so dwelt upon by some homœopathic writers, is quite misunderstood by them. Ipecacuanha and antimony act as powerful sedatives of muscular action, and relax both the muscular action of the bronchial tubes and the muscular coats of the stomach and the diaphragm, in cases of asthma and vomiting: thus, in fact, acting antipathically, when given in such doses as to prostrate muscular power. If you proceed cautiously with ipecacuanha and antimony, you may make them into beautiful sedatives of muscular action, and thus act both on the muscular coats of the stomach and the intestines; and on the diaphragm and heart in many cases, such as pneumonia, gastritis, diarrhœa, &c.; but in these cases the remedy is truly antipathic. It directly contradicts the action of the muscles which is going on, and the action of which muscles, such as the heart in pneumonia, is keeping up a good deal of the mischief.

We have been a good deal interested and amused to see these singular mistakes of the homœpaths respecting the real power and specific action of medicines.

We come, then, to the conclusion, that in nearly all, if not all, diseases, there are the attempts of nature, or a wonderful conservative power of the constitution to correct something which is interfering with the regular working of its machinery. In many of those attempts nature will do the work herself—in others she is over-doing the work, and in others she is coming short; so that in the first case the medical man must let her alone; in the second he must arrest or regulate her; in the third he must assist her. This, we think, is the GREAT PRINCIPLE OF MEDICINE, which we can discern, but which, as yet, we cannot work out completely in all cases. Our duty is to investigate the first commencement of diseases of every kind, and to see how nature proceeds in her work; we have also to see how she works in perfect health, and next endeavour to take our lessons of practice from her own proceedings. We can, however, discern so much in this, that in many cases she has to be checked or arrested, in others assisted, and in others let alone. We consider that the practice of homœopathy is simply the last, viz., *to let her alone*. A man has imbibed the poison of typhus fever, his blood is poisoned by some unknown material, and becomes decomposed, and its corpuscles broken up, so that they escape in some measure through the sides of the blood-vessels; blood oozes out upon the internal surface of the body. Nature will get rid of the poison if the patient live long enough, but you must *help* her to keep the patient alive with ammonia, wine, brandy, and food. Or, in another case, the man is labouring under violent suffering from undigested food in the stomach, giving rise to all kinds of symptoms. The stomach cannot relieve itself, although it tries hard to do so. You give a gentle emetic of ipecacuanha, or mustard and water, and the relief is rapid. In these cases you assist nature. In others you arrest her. For example, the inflammation in the lungs is from a bad cold; the blood has been driven from the skin and has collected in the lungs, the heart sets to work to pump the blood back again to the skin, but in doing so it has first to go through the lungs, which are already too full. The attempts of nature here are well meant, but they are misapplied. You arrest or regulate her. You give small doses of ipecacuanha so as to nauseate the patient, and thus temporarily to *weaken* the force of the heart. The heart, by so doing, does not act so powerfully. It sends less blood to the gorged lungs, and you have time to draw the blood to the skin by perspiration.—Or, perhaps the patient's bowels have been violently irritated by large doses of fruit in autumn. Nature carries off the irritating material, but cannot stop herself in the purging which she has caused. The case, therefore, ends in cholera or dysentery. You must, then, check nature. Give a little ipecacuanha to stay the violent action of the muscular coat of the bowels, and a little opium to modify or check its irritability, and you will soon cure the case.

Or, lastly, the man has simply got a bad cold and is very feverish. You put him to bed, strike off all high food, and give him one drop of water every two or three hours. This last method is homœopathy in infinitesimal doses!

This subject is so interesting, and throws such light on the unscientific character of homœopathic practice that we will dwell a little more upon it. The nature of disease is not sufficiently understood. It is this misunderstanding which gives the public such a mistaken idea as to the nature and effects of medicines. It is supposed that disease is necessarily a destructive process, which seizes on the body, and which must in the end destroy it, or at least that portion which is attacked. This is a mistake; almost all diseases which we can see and examine, are, in reality, attempts of the body to overcome some secret and unseen action or poison, which is working or existing in the system. Disease, in fact, as manifested to our senses, is nothing more than a deficiency or excess of a natural and wholesome operation.* In some cases we cannot explain this satisfactorily, but in other cases we can see the beauty of this explanation, and no doubt as our knowledge increases we shall be able to carry the illustration further. For example,† a man is attacked by a tumour in his eye, and the eye is gradually pushed out and spoiled; when examined, it is found that a parasitic animal (an hydatid) is within the diseased portion, and has given rise to all the mischief. But the eye bursts, and the animal is got rid of. The same thing may go on in the liver, where it cannot be got rid of so easily, as the patient often dies during the process. In common itch, an insect is seen burrowing in the skin, and causing an eruption; or a vegetable may be growing on the skin, as in porrigo, causing a tiresome skin disease. In all these cases you seem to have what is called disease going on, but it is the body which is struggling against the attacks of animals or vegetables; it is doing its best to get rid of them. Or, another man may appear to have disease of the kidneys or bladder. He may be making bloody urine in small quantity—the real mischief is that the kidneys cannot throw off the increased quantity of *urea* which is existing in the blood, and the man is being poisoned by it.—Or, two men may be making an immense quantity of urine—in the one we find sugar, as in diabetes, in the other turpentine, which the man has been taking as a medicine; but the symptoms are similar, and nothing more than an effort of nature to get rid of substances which are only intruders.

It is wonderful how well nature works to right the system when anything attempts to disorder it. She makes the strongest efforts to maintain the original *type* which the Almighty has given to it, and when anything attempts to destroy this type of construction and chemical constitution, she tries to recover it. If you bend a bit of whalebone or stick, it resists your force as well as it can, and will make strong efforts to resume its original shape when your hand is taken off. So in disease, the body *resists* offending causes, tries of itself to counteract them, and invites the physician or surgeon to assist her. If he refuse to assist her, she will often do it alone. "You have a type of shape for every part of the body, a type for the hand, for the face, for the brain, for the leg—a type of texture for organs, for nerve, for cartilage, for bone, for blood, bile, urine, sweat."—(*Simon's Pathology.*) In short, disease is generally nothing more than a modification of healthy action either in excess or deficiency. In the first you have to arrest or check it, in the latter to increase or excite it. For example, in cyanosis the child is blue because the heart has been arrested in its development, and in hypertrophy the development has gone too far; in hare-lip, again, the part was arrested in its growth; and in old men the cartilage which gave rise to their elasticity in youth has become bone—but it is only in obedience to the laws of nature, you cannot call it disease.

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* When we use the term "Disease" it must be understood in the sense in which we have endeavoured to explain it in a previous page.

† We take some of these ideas from Mr. Simon's work on Pathology, published by Renshaw, Strand, London.

When we view disease in the light which we have been attempting to explain, we cannot but be struck with the laws of preservation which the Almighty has implanted in the organs of his creatures. He has not simply created these organs, and left them to take their chance, but he has implanted such a wonderful power in each organ to preserve its own shape and function, that even after what is called an attack of severe disease, it will make strong efforts to recover its original *type* of shape, texture and function. So, to use the beautiful words of Mr. Simon, "We find that disease works according to laws definite, constant, invariable; we find in it no contradiction to the laws of life; on the contrary, that the latter, in their simplicity and comprehensiveness, include and account for it; that the power of adaptation to circumstances, the power of resistance to casualties, the power of repair after injuries, would not be possible or conceivable attributes of the human body, except under conditions which impose the liability to disease. At every turn of the subject, and in every fresh illustration which new study reveals to us, we derive deeper and more steadfast convictions of the total absence of caprice, chance, or irregularity, even in the strongest influences of disease. We become habitual observers of that mystery which most of all tends to chasten and to elevate the mind—observers, namely, of the unbroken uniformity which prevails in the operation of natural laws. Standing, in the daily exercise of our profession, amidst an apparent chaos of darkness and suffering, where at first all seems, as of yore, to be 'without form and void,' it is our great privilege, that, by the aid of scientific insight, we are raised to a recognition of the 'Spirit which moves upon the face of the waters,' and which now, as in the first morning of creation, resolves that chaos into harmonious order, that darkness into intelligible light, that suffering into the feeble counterpoise of some greater and more extensive good." (*Simon's Pathology*, p. 17.)

Nature, in fact, works from a model. There is nothing more interesting than to observe nature in her processes both in health and disease. You have a child in infancy totally unlike its elder brothers, but watch its growth and you will see it gradually assuming the *type* of the family; particle by particle is added to its body, feature to feature, till the growth is completed; and then say whether the living statue has not been copied from another statue by a wonderful *sculptor*. In fact nature is the sculptor, and she has worked after a model with mysterious but correct power. So in disease, a part is disorganized by what is called disease, but nature is still the sculptor. She has not forgotten her model; she replaces the lost or disordered portion by parts which she models as nearly as possible like the original parts. She often fails, but her efforts are beautiful; and in some of the lower animals she quite succeeds. In these, a limb may be taken off and she will replace it by a new one resembling the original. It is the same effort all through the human body, which we call the conservative power of nature, an attempt to recover the original type, which disease may be attempting to destroy. We perceive this in the more severe, as well as in mild disorders. The disordered operations, then, of the human body are generally nothing more than its attempts to regain the path of health from which some cause has caused it to swerve; nature often succeeds without any aid—at other times she fails. The art of the physician is to watch her efforts, to *assist*, to *check*, or to *modify* them according to the best of his judgment.

Diseases attack the body either from without or within. For example, malarious poisons from without, or poisons formed within the body itself. They are known by their symptoms and effects, such as venous congestion, or dropsy from heart disease. The internal organs are like the crew of a ship in a storm, and instantly attempt to avert or throw off disease when it approaches. The storm may rage and the ship may give symptoms of the danger. The sails may be rent, the masts may crack, the deck may be flooded, and the fire of the engine may be damped. These are *necessary* symptoms and effects of the storm. But observe the crew: the sails are regulated, the steam of the engine is got up, the pilot is at the helm, and the compass is watched. These symptoms or effects are *not necessarily* connected with the storm. They are only an increase of the activity and watchfulness of the crew, which exist when there is no storm at all. To generate, or simulate, or aggravate the symptoms which are necessarily connected with the storm is a *bungling* kind of way of getting out of it; but to regulate the symptoms or activity of the crew, and to direct those symptoms or operations *which are not necessarily connected with the storm*, but which may exist at all times, is a beautiful and scientific object. Let the captain (the physician) therefore, *direct the crew*, furl or unfurl the *sails*, *get up the steam*, *pump the water* from the ship, keep her guided by the compass, avoid the rocks and shoals on all sides, *retreat or advance her according to circumstances*, and in 96 cases out of every hundred you will save the vessel. We must therefore be careful to distinguish between *the symptoms or effects of a disease, and those natural and conservative powers which throw off disease, but which are not necessary accompaniments nor symptoms* of the real disease, as these last symptoms, which we may call *conservative symptoms*, exist in the body at all times, even in a state of health. *The symptoms of disease can only exist as the consequence of disease. The conservative symptoms or powers can and do exist independent of disease. To increase or generate symptoms similar to the disease* is only *increasing the mischief. To assist or regulate the conservative powers*, so as to throw off the disease, is a beautiful and scientific principle, and worthy of a noble profession. It is an attempt to imitate those laws which the Almighty has placed in the body for its preservation. This we consider to be *the true principle of medicine*, and is gradually exalting the principles of medicine into a *true science*.

To prescribe for *symptoms*, therefore, and especially to endeavour to generate a similar set of *diseased* symptoms, is to sail across the Atlantic without a magnet, and simply to accommodate the ship to the wind. What would be thought of a man who attempted to cure a child's legs which had become bowed and crooked for the want of lime in the bones, by applying irons or supports to his legs, but neglected to give the child phosphate of lime to strengthen his bones. Simply to apply artificial support would be called *empirical*, prescribing for symptoms without a real principle to guide us; but to give the lime is a scientific thing. The same principle applies to spinal diseases.

We might go on for pages illustrating the error and want of real science, and even danger, of thus mistaking *symptoms* for *disease*. No doubt the most scientific man is often at a loss how to account for the symptoms before him, and, when so, he must submit to the humiliation of confessing that he can only make use of an *Art*, and prescribe for his patient according to what experience has taught him to have been useful in similar cases. But this empirical method is fast disappearing from amongst all well-informed medical men, and *art* is rapidly giving way to *science*.

In the previous remarks we have shown what an excellent physician Nature is, and that the recovery of a patient from sickness is no proof that the medicine given was the cause of the recovery. It may have been useful or not; but when we hear a patient say that he was attacked by illness, and recovered *after* taking certain globules or certain drops like water, and *therefore* his recovery must have been owing to these remedies, we are struck with the ignorance both of the patient and his medical adviser respecting the nature of disease, and respecting the wonderful powers of the constitution to throw off illness, even when unassisted by curative means. It reminds us of the sailor who whistles for a fair wind and if the wind becomes favourable *after* his whistle, he has the impression that the whistle caused the wind to change—it is the common "*post hoc propter hoc*" argument.

Success in medical practice does not necessarily prove the truth of a professed principle, unless we can be absolutely certain that the principle is strictly adhered to. There is nothing more common than for one physician to be more successful in his cases than his neighbour; both may be professing the same *principles*, but each differs from the other in his *practice;* in short, success in practice depends not only on a good knowledge, but on a certain ability, or talent, or *tact*, to bring that knowledge into exercise, so if a homœopathic physician likes, he can treat his cases either homœopathically or allopathically, or in any way he thinks fit, and all the time pretend to be a homœopathist. His homœopathy may be made a cloak to do just as he likes. Medicines are now made in so concentrated a form, that a drop or two may be a full dose. This may appear to the unprofessional man to be an infinitesimal dose, when given in a glass of water; but in fact, it is a full dose, and quite as large a one as a regular physician would himself give. Aconitina, Strychnia, Nicotine, Morphia, Atropine, are the essences of Aconite, Nux Vomica, Tobacco, Opium, and Belladonna, which every regular physician may use; but if he wished to appear somewhat wonderful, he might drop a very small quantity into a wineglass or tumbler and give it to his patient, with good effect—yet, he would in fact be giving a full dose of the medicine. The dose might *appear* to be a homœopathic infinitesimal dose, but it would *appear* to be what it was *not*—it would be a good dose. So that when homœopaths appear to be giving their infinitesimal doses, they may be doing so only in *appearance.* If a homœopathic physician practised in any way that he thought fit, and used any medicine whatever, without any respect to his principles, (all of which things he might do with the outward form of homœopathy,) and if, at the same time, he were of equal or superior natural ability to his allopathic neighbours, we dont see but that he might be equally successful. But there would be this difference, that the homœpath would be practising with a certain degree of deceit, and using the term homœopathy as a cloak to carry on his practices. It is this conduct which we look upon with dislike, and particularly so, when, at the same time, the homœopathic physician advertises himself as carrying out a *New System*, founded on *New Principles.* The whole thing is considered by the body of regular surgeons and physicians as, to say the least, discreditable to a body of properly educated men.

Let us next examine more particularly the principles of homœopathy, and compare the *modes of treatment* adopted by the homœopath and the scientific practitioner. The homœopath, we repeat, prescribes for the *symptoms* of a disease—the scientific practitioner prescribes for the cause of these symptoms, or tries to *regulate the conservative powers* of the constitution. Toothache is a symptom of a decayed tooth. Whether is it better to prescribe medicine to relieve the pain, or to attack the cause and extract the tooth at once? The homœopath would give medicine which, in a healthy man, would cause symptoms similar to toothache; the scientific practitioner would extract the tooth. The homœopath, when called upon to relieve a neuralgic pain, would give a medicine, if he could find one, to cause similar symptoms in a healthy man; the scientific practitioner would remove the cause if he could find it, and if he could not, he could easily relieve the pain by opium, or morphia, or aconite, or belladonna, and better still in most cases, by chloroform, either gently inhaled, or applied to the surface. All these medicines have a remarkable power of relieving pain, but they will not cause it when used in infinitesimal doses, nor in any doses at all—and yet homœopaths use these medicines in violation of what they call an infallible law or principle.

The great principle of homœopathy is, that whatever medicine given to a healthy man will cause in him symptoms similar to any disease, that medicine will cure the disease in the sick man. But is this the fact? Hahnemann states that Peruvian bark cures ague, because, when given to the healthy man, it causes symptoms similar to ague. This we deny altogether. How many people take quinine (made from bark) both in small and large doses, and we never hear of them having symptoms of ague! It is a curious fact, that when a person goes into a district infected with the poison of ague and other malarious fevers, he is nearly proof against all the symptoms if he take quinine for a certain time before he goes into the district, and for 14 days after he leaves it. This is well exemplified in a report lately issued by the expedition just returned from the Niger river, as will be seen at p. 648 of the Lancet, Dec. 18, 1858.* It is not a fact, therefore, that Peruvian bark produces symptoms of ague in the healthy man, but *just the reverse*—it prevents all symptoms from even showing themselves.

You could not have a more forcible example of the difference of treatment between one system and another than in the subjoined case of the treatment of tetanus. Tetanus, or lockjaw, consists in the rigidity of almost all the muscles of the body; it can be accurately simulated by giving a healthy man strychnine. The symptoms of poisoning by strychnine are exceedingly similar to the symptoms of lock-jaw. The homœopath ought, therefore, to have a certain remedy in strychnine. Exactly the reverse is the case, strychnine aggravates the symptoms, and was obliged to be given up; but aconite was found to cure. Now aconite prostrates and relaxes muscles, and never contracts them. In one of our previous volumes will be found a case in which chloroform was useful in relieving convulsions, and another case in which tobacco or nicotine was used with success in tetanus or lock-jaw. But all these remedies are exactly the opposite to strychnine, and every candid homœopathist must at once acknowledge that his principles are completely broken down before such examples of cases. The subjoined is an extract of the case treated by strychnine and aconite. It will be found in detail at p. 70 of the 39th volume of our *Retrospect*. Henry Blackwin, aged 15, was attacked by tetanus, Sept. 16, 1858, and admitted into the Middlesex Hospital. His symptoms became more marked in two days, and were gaining ground. One-tenth of a grain of strychnine was given every two hours, and continued in varied doses till the 20th, when "the symptoms were now becoming so urgent that the strychnine treatment could not be longer tried. It was evident that though it was producing its own specific effect, the paroxysms of the disease were in no way relieved. nor were the chronic spasms at all diminished. (p. 72, *Retrospect*, vol. 39. The patient was now put upon aconite in five minim doses every two hours, with an aconite liniment; this was assisted by an ointment of one part of extract of belladonna with two parts of opium rubbed into the thighs; "from this time the improvement was progressive, the countenance becoming more natural and the pain and anxiety diminishing." Another case is related in which the effects of aconite were still more marked; so much so, that by this medicine, "during the continuance of the disease, though at times he was alarmingly affected by the medicine, the severe tetanic symptoms were constantly subdued." (vol. 39, p. 74.) Another good case of recovery from lock-jaw (see *Retrospect*, vol. 38, p. 44) is given by Mr. Simon. Here nicotine (made from tobacco) was used. See also vol. 36, p. 38, where Dr. Hobart, of Cork, points out its value in tetanus, and also as an antidote to strychnine.

These cases of cure of lock-jaw by aconite, belladonna, and nicotine, are quite sufficient to overthrow all the arguments in the world, based on the theory that "like cures like." Here are cases fairly cured by remedies, the specific effects of which are quite *dissimilar* from the disease. In lock-jaw we have the muscles rigid and contracted—the effect of aconite is to relax muscles; the same more especially is the effect of tobacco or nicotine, and belladonna. Make a person sick with tobacco, and see his relaxed and sickly frame—quite the opposite to rigidity—and yet this remedy cures lock-jaw, in which strychnine has been proved to fail. Yet strychnine produces symptoms very similar to lock-jaw, and ought to cure it if homœopathy be correct.

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* Dr. Livingstone, in his present expedition in Africa, does not find the same power in quinine to avert malarious fever. (See *Med. Times and Gazette*, Nov. 12, 1859, p. 478.

If one thing more than another could convince us of the error of homœopathy, it would be the use of aconite by homœopaths; perhaps the most powerful and favourite medicine which they use. They call it "*the Homœopathic lancet*, as it is the *great remedy for inflammation*." Dr. Sharp says that it is "a most valuable remedy in simple and inflammatory fever. It must entirely banish the use of the lancet, the leech, and the blister, in such cases." "In its relation to inflammatory fever it stands at present unrivalled." (*Tract*, No. 10, pp. 12, 13.) We agree with these remarks in many respects. It is a most valuable medicine. It is not duly appreciated nor understood. Nay, even the homœopaths themselves are at downright *loggerheads* respecting it. Mr. Everest, a zealous homœopath and friend of Hahnemann, says, that a true application of the doctrine of homœopathy "excludes the use of aconite in almost every case in which it is now employed by homœopathists." Mr. Everest further says, "I assure you seriously, that absurd as the old system is, it is not more injurious than this, viz., homœopathy as practised by British homœopathic physicians and amateurs." This contradiction is amusing: but we here quite agree with Mr. Everest, that the use of aconite by modern homœopaths is a direct contradiction of all their principles! For what is aconite? Let any candid homœopath refer to Dr. Fleming's Monograph on this medicine, the best work ever written on Aconite. Dr. Fleming says, "that its primary action on the nervous system is purely *sedative* in its nature from the first, and the closest scrutiny fails to detect any symptom calculated to warrant the belief that a primary stimulant action is exerted." Again, he says, that its action on the circulation is also sedative; "when this is fully developed, the flow of arterial blood to the brain is much diminished, an occurrence which impairs the energies of that organ, *in the same way as excessive loss of blood;* between which, and the action of aconite, there exists a very strong analogy." "The action of aconite on the *muscular system* is directly and powerfully sedative." (*Fleming on Aconite*, p. 26, 30.) These are not simple opinions, but are founded on a series of experiments of the most interesting kind;—and yet, here is a remedy of a most sedative character used by homœopaths in fevers, inflammations, &c., as a substitute for the lancet, leeches, &c. They could not possibly have used a more *antipathic* or *allopathic* remedy! Let them candidly consider this, and acknowledge it. Don't let them *pick out* one or two solitary symptoms from amongst numerous others, which might seem to support their views, but let them take the general character of the medicine. In some medicines, when taken as poisons, you may have numerous symptoms of an anomalous character, and it is easy to select some of those symptoms so as to appear to give support to their views; but aconite, if viewed fairly, must decidedly contradict their principles.

We will give one more illustration, to show the error and danger of treating *symptoms* instead of the *disease*—*effects* instead of the *cause*. Apoplexy is caused by the rupture of a blood-vessel in the brain, and the blood escaping into the parts around the blood-vessel. Among the most important symptoms, we have what is called *coma*, a kind of stupor, with snoring and insensibility, resembling a man who is intoxicated, or one who has taken a large dose of opium. Both opium and alcohol, therefore, ought to be the homœopathic remedies. But how dangerous to use them in a man whose blood-vessels are already overloaded! When a water-pipe has burst in the street, and is deluging the surrounding earth with water, whether is it better to send more water along the pipe or to cut off the supply from the main-pipe or reservoir? The homœopathic remedies, opium and alcohol, if used at all, would send the blood still more to the broken blood-vessel—the lancet of the surgeon would stop the supply. We do not say that the homœopath *would* use opium and alcohol; we only say that as these agents produce symptoms in the healthy man resembling the coma of apoplexy, they *ought* to be the homœopathic remedies for such a state. Again, you see a man's eye angry and inflamed—you can produce this condition, the same as in the brain of an apoplectic patient, by giving opium or brandy: but, next time your own eye is bloodshot and inflamed, try a dose of opium or brandy, and see if it does not greatly aggravate the case. The next time your eye is in the same state, try cold water applied to it, take a little ipecacuanha to make you sickly, and thus to diminish the force of the heart, and the rush of blood to the eye, at the same time keep in a dark room, so as to keep off the stimulating effects of light; and strike off the supply of food, so that the quantity of blood made may be rapidly diminished, and see what effect this will have upon your eye. You will soon see the difference of treatment, and be convinced of the error of homœopathy, and that, after all, the *old system* is not to be condemned.

When, however, we find in the standard works of homœopathy that common salt is said to cause 450 symptoms, oyster shell 1090 symptoms, the ink of the cuttle fish 1242 symptoms, common charcoal 930 symptoms, and that, therefore, all these symptoms of the medicines may be a guide to us in applying them to disease, we naturally shut the book, shrug our shoulders, and confess that we cannot argue any more with such people!

You cannot have a much better example how beautifully what may be called antipathic or allopathic treatment is illustrated, than in the treatment of acute rheumatism. Every one knows what the *symptoms* of rheumatism are; but the *symptoms* are not the *disease*, and however accurately you may be able to resemble rheumatism by giving medicines so as to generate similar symptoms, you will not cure it by this treatment. Now, what is rheumatism? It is essentially a blood disease. The blood is found to contain a more than usual quantity of what is called *fibrin*, a material which chiefly composes, surrounds, and packs up all the joints, so as to strengthen them. This same material also exists in the valves and other tissues of the heart. Now, just look at the way in which nature or the powers of the constitution try to get rid of this *fibrin*, which is circulating in the blood to its great annoyance. The joints are chiefly composed of this material: the joints during their growth in childhood constantly absorb this material for their sustenance and increase. They continue to do so all through life, and therefore, when there is an unusual quantity in the blood, they are expected to absorb an increased quantity, in order to relieve the blood. This they do, and it is this process, always aggravated by fatigue, which brings on the *symptoms*, pains in the joints, muscles, and tendons. But the heart itself is also a kind of large joint, and does the same work. The heart, therefore, is liable to the same disease. Now, you certainly might *try* to treat this case homœopathically, and *attempt* to produce pain in the joints, or a heart disease, but in the first place you would fail, and in the second place, supposing even that you succeeded, you would aggravate the mischief already done, not because nature's removal of the fibrine is wrong, but because it is done in a manner which, though curing the rheumatic state of blood, injures the joints and heart. But if you give salts of potash, or bicarbonate of soda combined with nitre, you *dissolve* this fibrine in the blood, and wash it out through the kidneys, thus getting rid of the evil at once, and very rapidly curing the case.* Now we would ask the candid homœopathist if he can reconcile this with his creed. You might give potash and soda for ever, and you would never produce symptoms of rheumatism—and you would never produce fibrine in the blood by giving these medicines. On the other hand, potash and soda directly and immediately dissolve the fibrine. They are beautiful antipathic or allopathic remedies. We do not by any means intend to say that excess of fibrine in the blood is the only change in that fluid pathognomonic of rheumatism, nor that alkalies act only by diminishing the excess.

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* See *Guy's Hospital Reports*, vol. v., 1859; and the present vol., p. 27.

Consider the danger of homœopathy to individuals attacked by disease, who have not been previously experimented upon *in health*, as to the effects of medicines. A person is attacked by disease; he sends for the homœopathic physician, medicines are prescribed and given; but who knows what are to be the effects? One great principle of homœopathy is, that every medicine must have its powers and effects tried on *healthy* people. But every healthy man differs more or less from another, and no medicine has the same effect on two or more individuals. But the patient's life is in danger, and may terminate in a very short time. In this dilemma, the physician must give medicines which he has only tried on *other* individuals: in order to be effectual, they ought to have been previously tried on the patient himself *whilst in health*. This is one of the chief principles of the doctrine, and, consequently, when brought into practice at the bed-side, will almost always prove a broken reed to depend upon. The principle might be worth arguing if the same medicine had always the same effect on all individuals, but as it has hardly the same effects on any two people, it is worth nothing. Even if the drug had a tolerably similar effect on twenty people, but not on the next five, it would be a great misfortune for one of the five to be attacked by a disease for which the remedy was quite inapplicable; and supposing that his life was at stake, we should consider it a dangerous principle to be depended upon.

Consider, also, the danger of trusting to this system of medicine under the following circumstances:—"To effect a mild, rapid, certain. and permanent cure, choose, in every case of disease, a medicine which can itself produce an affection similar to that sought to be cured." (*Dr. Dudgeon.*) You may generate *symptoms*, but you cannot generate the artificial disease which is to resemble the real disease. No homœopath has ever yet pretended to generate *diseases* by means of medicines similar to certain original diseases. What medicines can generate *diseases* similar to mitral valve disease; to a clot of blood on the brain from a ruptured vessel as in apoplexy; to amaurosis or cataract of the eye; to the effusion of lymph or fibrin, as in croup; to a stone in the kidney or bladder, or to worms in the bowels; or to numerous other real diseases? You may attempt to generate some of the *symptoms*, but these are not the disease, and may exist in quite different diseases. To generate a symptom without also generating a disease is useless; the real cause of the artificial symptoms you generate is nothing more than a poison acting in certain ways on different parts, but very little resembling disease. But if you *could* generate an artificial disease resembling the real one, woe be to the patients labouring under numerous diseases, the least increase or aggravation of which would be destruction to them—such as a fatty blood-vessel in the brain ready to burst; a thin heart also ready to burst; an amaurotic eye, nearly blind; an ulcer of the cornea, just ready to penetrate that structure; an aneurismal tumour; a blood-vessel in the lungs ready to burst; an ulcer in the stomach or bowels, &c., &c. It must be remembered that in all these and numerous other cases, in which life hangs on a thread, the homœopathic principle is, that the artificial disease or symptoms will at first rather increase the original ones. There will, in fact, be an aggravation of symptoms for a time, and this slight aggravation will soon decide the matter in many diseases. We must protest solemnly against the practice of such a principle in the treatment of disease, and hold those who do practice it as responsible for many grievous mistakes both in the diagnosis and treatment of disease.

The homœopathist says, that one great difference between his system, and what he calls the old system, is, that the medicines which he gives he first tries on himself; whereas, in the *old* system these are first tried on the patient. This is a misrepresentation. We deny altogether that the homœopath first tries all his medicines on *himself*—his system depends almost entirely on the effects of medicines on *others*. Dr. Sharp, who has particularly stated the above doctrine, informs us that the reason why belladonna is effectual in scarlet fever is, that "Belladonna, when swallowed as a poison, produces a scarlet rash, a sore throat, fever, headache, &c., all which symptoms appear in scarlet fever." He likewise states, in another page, that "The homœopathic physician learns the properties of drugs by experiments upon himself, not upon his patients." Let this gentleman say whether his assertion is to be depended upon. Has he tried belladonna in these doses upon himself? Has he tried the effects of all the medicines which he uses on himself? If not, is it worthy of him to make such an assertion to the disadvantage of those who disagree from him? Moreover, we assert that belladonna is very variable in its effects, and does *not* always produce the symptoms above-mentioned; it is even said that it produces these symptoms very rarely. We have been accustomed to prescribe belladonna for thirty years, and we never yet knew this drug to produce the symptoms above-mentioned. It is not the rule certainly; we could give many instances in which this effect was absent. The reader will find some very interesting cases of this kind related by Dr. Fuller. In several cases of chorea in which this medicine was given in large doses, (70 grains daily to one girl of 10 years old, or 1,019 grains in 26 days; to another girl, aged 14, atropine 37 grains in 18 days), no bad effects whatever followed, and "in no instance was there any feverish heat, or any *rash* or erythematous blush on the skin." (*Brit. Med. Journal, Aug.* 27, 1859, p. 704.) From some cause or other there was great *tolerance* of this medicine in these cases, but they were cases of convulsions. We mention the fact, however, simply to prove that belladonna does not counteract scarlatina, owing to what are called its homœopathic virtues, as these virtues or effects are very exceptional. And, by the bye, many of these cases of chorea were relieved by the belladonna. Now chorea is a convulsive disease, and the effect of belladonna is to *relax* muscular action—in fact, a direct opponent to spasm or convulsion. In chorea, therefore, it was beneficial as an *allopathic* and not as an *homœopathic* remedy. The homœopath may say that in the above cases that disease existed, and therefore you could not judge of the remedy. In other cases, however, referred to in the same paper, it was given experimentally, but cautiously, to healthy children and adults, and still we find no rash produced.

One of the laws or principles of homœopathy is, that when a disease exists in the body, it can be cured by another disease with similar symptoms, provided the latter be stronger than the former, or, in the words of Hahnemann, "INVARIABLY, and in every case, do two diseases, differing certainly in kind, but very *similar* in their phenomena and effects, and in the sufferings and symptoms they severally produce, ANNIHILATE ONE ANOTHER, whenever they meet in the organism; the stronger disease, namely, annihilates the weaker." As an example of this law, the homœpaths state that an attack of small-pox destroys cow-pox. "*Small-pox coming on after vaccination, as well on account of its greater strength as its great similarity*, IMMEDIATELY REMOVES ENTIRELY *the Cow-pox homœopathically, and does not permit it to come to maturity; but, on the other hand, the Cow-pox when near maturity does, on account of its great similarity, homœopathically diminish very much the supervening Small-pox, and make it much milder, as Mühry (in Robert Willan, on "Vaccination") and many others testify.*" (*Organon*, p. 148.) This assertion of Hahnemann shows how carelessly and incorrectly some of his boldest opinions and matters of fact are adduced in proof of what he wishes to be believed. Will the reader believe it, that what Hahnemann here states is perfectly incorrect, and that Robert Willan, our celebrated English writer, asserts exactly the opposite? Dr. Willan's words (in his work on *Vaccine Inoculation*, p. 3,) are, 1. "That when a person was inoculated with vaccine and variolous matter *about the same time*, both inoculations proved effective; for the VACCINE VESICLE PROCEEDED TO ITS ACME in the usual number of days, and the maturation of the various pustule was attended with a pestular eruption on the skin. 2. That these effects took place, without much variation, in all cases where the interval between the two inoculations *did not exceed a week;* but, 3. That when variolous matter was inserted on the *ninth* day after the vaccine inoculation, its action seemed to be wholly precluded," (the system being now sufficiently fortified against it by the due protecting effects of cow-pox). So much for the opinion of Hahnemann, which proves just the contrary to what he states. We must do Dr. Sharp, of Rugby, justice in stating that he dissents from these extraordinary fallacies of the founder of his creed. He says, "Truly never was hypothesis based upon more slender materials; never did assertion and inadequate proof appear more conspicuously side by side than in these paragraphs!" We wish this estimable and talented writer had been equally discerning to find out the other fallacies of his system. We still hope to see such a mind as his candidly acknowledging its errors.

Another principle of homœopathy is, that the effects of a remedy must not be *identical* with, but "as similar as possible" to the disease, or, in exact words, "In order," says he, that "the artificial diseases producible by medicines, may effect a cure, it is before all things requisite, that they should be capable of producing in the human body an *artificial disease*, AS SIMILAR AS POSSIBLE to the disease to be cured, in order, by means of this similarity, conjoined with the somewhat greater strength, to substitute themselves for the natural morbid affection, and thereby deprive the latter of all influence upon the vital force." (*Organon*, p. 133.)

The example adduced to support this law is that if a person has on his skin an herpetic or miliary eruption, and he be attacked by measles, the measles cures the previous disease. But we perceive very little similarity between measles and herpes, or measles and a miliary eruption. We might as well say that small-pox and erysipelas resemble each other, because the skin is affected in both cases. Measles and herpes, if they counteract each other, do so for just the opposite reasons to what Hahnemann adduces. They counteract each other from their *dissimilarity*, and not on account of their *similarity*. This seems even to be the opinion of Hahnemann himself, for he goes on, curiously enough, to show how, in confirmation of his great principle of "like curing like," small-pox cures inflammation of the eye, amaurosis, deafness, difficulty of breathing, swelling of the testicles, and dysentery. It is almost laughable to see how the zeal of man will mislead him. What similarity have the symptoms of small-pox to the above-named disease? We should again say, they are as dissimilar as possible, and only prove the weakness of such an argument. These cases of Hahnemann prove what has long been known to us, that it is difficult for two diseases of any kind to exist at the same time in the human body, and much more difficult for diseases of a dissimilar, than of a similar kind to exist together.

The homœopathic law which states "It is not possible to perform a cure *but* by the aid of a remedy which produces symptoms similar to those of the disease itself," is daily contradicted, not only by cases met with by the scientific modern physician, but by the homœopathist himself; and the very instance brought forward, viz., that ague is cured by bark, *because* bark produces symptoms similar to ague in the healthy individual, is quite contradicted by every-day experience, as we noticed in a preceding page. If this principle were true, we must, to be successful, possess medicines which would in the healthy man produce symptoms similar to all the diseases of the body. Now many of these diseases we *can* simulate by medicines, such as the coma of apoplexy by opium; the delirium of inflammation of the brain by alcohol, Indian hemp, &c.; inflammation of the eye by a drop of oil of vitriol; inflammation of the stomach by arsenic; inflammation of the kidneys by Spanish flies;—but we would ask any candid homœopath if these would be proper remedies for the diseases themselves? On the other hand, we should like to know what medicines given to a healthy man will produce symptoms of typhus fever, consumption, scrofula, cancer, and a host of other diseases—and if the above quoted dogma be true, and if we cannot produce these symptoms in the healthy man, then we do not possess any remedies for this host of diseases on the homœopathic principles. Moreover, facts are against this hypothesis, as, for example, in *sea scurvy*.

You cannot by any known medicine produce the symptoms of sea scurvy in a healthy man, and especially not by lemon-juice—but let a sailor be attacked by sea scurvy, and let him have lemon-juice, and you will soon see the good effects. Again, you cannot produce *goitre*, or swelled neck, in a healthy woman by any known medicine, and especially not by iodine—but this is cured by iodine. It is urged, with apparent plausibility, that nitrate of silver causes inflammation in the healthy eye, and cures it in the diseased one;—but oil of vitriol, a bit of sand, a hot iron, caustic potash, violent winds, and many other things produce a similar kind of inflammation—but do not cure it. Inflammation of the eye consists in a *dilatation* or swelling of the blood-vessels, so that these vessels admit a larger quantity of blood than in health—nitrate of silver *contracts* these vessels and prevents all this blood from passing along them. The remedy, in fact, contradicts the disease so called—the disease is *dilatation*, the remedy is *contraction;* precisely the same as, in typhus fever, the disease is a poison prostrating the powers of the body—the remedy is a stimulant, such as brandy or ammonia.

The assertions of homœopathic writers have no doubt shocked some of the more sensible disciples, but they prove that there is no unity either in the ranks or practice of this system. One writer says, "If the remedy given be homœopathically selected, it *will* cure in whatever dilution it may be administered." (*Brit. Journal of Homœopathy*, vol. 5, p. 532,) So that, whether a drop of tincture of aconite be mixed with a hundred drops of water, or in a lake 250 miles square, it *will* cure, if selected homœopathically! But other writers and practitioners differ; for in the same work, we find that with respect to doses, "There is no homœopathic question in which there exists greater discrepancy of opinion. In fact, we may almost say there are as many opinions as practitioners, and each is prepared to prove the superiority of his own by an imposing array of cases." (*Ibid*, p. 257.)

This contradiction of opinions amongst homœopaths is equally strong with respect to the medicines used, as well as the doses. Thus, Hahnemann considered that aconite was chiefly useful in pure inflammatory fever, and one or two other disorders; but others use it indiscriminately in chronic and acute diseases, and so commit, according to Mr. Everest, one of their disciples, "most extensive mischief." Thus, in Dr. Hempel's Domestic Homœopathic Physician, we find that aconite is used in almost all affections, "from nettle rash to consumption, from toothache to apoplexy, aconite is the principal, the best, the infallible remedy." Again, Hahnemann maintains that arsenic is the true specific for typhus, while Dr. Wurmb maintains that it is of no use whatever in this disease.

The homœopath asserts that his medicines have not been fairly tried by the regular physician. This assertion is incorrect. There is not a more respectable man anywhere than M. Andral, of Paris. Dr. Marsden, a homœopathist, speaks of him as the "prince of physicians." M. Andral tried the system on 130 or 140 patients, in the presence of the homœopathists themselves. In describing the results, he said, "he was decidedly opposed to the project of allowing the homœopathists a dispensary: humanity should not be trifled with by the experiments of these people. He had given their system a fair trial; he had treated above 130 or 140 patients homœopathically, *in presence of the Hahnemannians themselves*. M. Guibourt had prepared the medicines; and *every* requisite care and precaution were duly observed; yet in not one instance was he successful. He had tried various experiments on his own person, and several other professional friends had followed his example, in order to ascertain the actual effects of the homœopathic doses; but the results were not as Hahnemann and his disciples described them. He (M. Andral) had taken quinine in the prescribed globules, but had contracted no intermittent fever; he had taken aconite, but without being affected with symptoms of plethora; sulphur he took, to try if he should catch the itch, but he caught nothing: neither, upon swallowing certain globules of arnica, did he feel pains as if he had suffered contusion: and so with various other substances which he and his friends took in obedience to the Hahnemannian precepts. With respect to the attempt to cure disease by this method, he said that in every instance he was obliged to return to allopathy, inasmuch as under the homœopathic treatment the symptoms went on from bad to worse." (*Dr. Simpson*, p. 275.)

"A German homœopathist (observes Dr. Lee), practising in Russia, was invested by the Grand Duke Michael with full powers to prove, if possible, by a comparison of facts, the advantages of homœopathic measures over the ordinary modes of treatment; and a certain number of patients in the wards of a military hospital were intrusted to his care. At the expiration of two months, however, he was not permitted to proceed further. For, in comparing results, it was seen that within this period, out of 457 patients treated by the ordinary means, 364, or three-fourths, were cured, and none died; whereas by the homœopathic method, tried on 128 patients, one-half only were cured, and five had died." *(Dr. Lee's Homœopathy, p. 26.)* The Russian government, it is further stated by Dr. Lee, tried in two hospitals the comparative treatment of a number of patients with homœopathic globules, and a number of other patients with no drugs of any kind; and the results were found very similar in both instances.

The medical council therefore recommended that the homœopathic treatment should be discontinued for the following reasons:—

"1. Acute diseases require energetic means of treatment, which are not to be expected from homœopathy. 2. The homœopathic treatment of external lesions and surgical diseases is altogether out of the question. 3. Some slight affections get well while under homœopathic treatment, but similar affections disappear equally well, without any medical treatment, by the adoption of an appropriate regimen, good air, and cleanliness." *(Dr. Lee's Homœopathy, p. 27.)*

At Naples, too, a commission was appointed to examine this system, and came to the conclusion—"1st. That the homœopathic treatment produced no effect. 2ndly. That it had the serious inconvenience, in several of the patients, of preventing the employment of remedies by which they might be cured." *(Ibid*, p. 29.*)*

Dr. Lee further states, at p. 20, "in looking over the history of several of the cases treated at the London Homœopathic Institution, I found, what might be anticipated, that they were very analogous to the above, viz., the ordinary slighter ailments usually met with in dispensary practice, which seldom require a long treatment, though most of the cases reported in the Homœopathic Annals required two, three, or four months attendance before the patients were dismissed." Again, "we have had, I think, sufficient proof that disorders and diseases are of much longer duration under the homœopathic than under appropriate allopathic treatment, and also, that in acute and serious diseases the mortality of patients homœopathically treated greatly exceed that of those treated by the ordinary appropriate means." (p. 21.)

In conclusion, therefore, we consider that homœopathy is unscientific in principle, and, as just shown, unsuccessful when applied to the treatment of disease, and leaves the case to the unaided powers of nature, sometimes even increasing the disease present, or entailing a lingering recovery.

140.—ON THE INFLUENCE OF VITALITY UPON THE EXCRETIONS.

By Dr. T. Inman, Liverpool.

[In the present paper Dr. Inman calls the attention of the profession to the influence of a debilitated condition of the system upon secreted and excreted matters.]

All of us are more or less familiar with the fact that the excretions do not decompose, as a general rule, while they are in the body. The fæces, retained though they be sometimes for days, and in some rare cases for weeks and months, in the colon or the rectum, do not decompose there, as they do when they are expelled from the body; and this observation holds good, even though the bowels are distended with flatus, and the fæcal matter is in contact with a gas differing in no degree from common atmospheric air. The urine, when retained in the bladder, becomes denser, and undergoes some change; but it does not decompose in the same manner in the interior of the body as it does when expelled therefrom, and kept in a close, well-stoppered bottle. We account for this by saying that the excreta have a certain vital power as long as they remain in the body, which enables them to resist the ordinary laws governing the inorganic world.

But we know from experience that the vital power does not immediately leave a limb after it has been amputated; and it becomes, therefore, a question whether vitality may not also remain in excretions for some time after their expulsion from the body. In other words, do the excretions lose their vitality as soon as they are expelled? or is there a period during which they successfully resist chemical laws? And, if such period exist, is it definite for all? Do the excretions of some decompose, *cæteris paribus*, sooner than those of others? and, if so, can we associate that phenomenon in any way with the condition of the individual at the time?

My attention was first called to this subject in the following manner:—When M. Ledoyen came to Liverpool, about twelve years ago, to demonstrate the deodorising power of his "disinfectant" fluid, with other experiments, the following was made. The alvine dejections of a certain number of patients, ill with fever and various other diseases, were all placed side by side, to the number of thirty or more, in a small room attached to the pauper hospital. They remained all night in the chamber, and the next day M. Ledoyen commenced operations. After demonstrating the general advantages of his compound, he proceeded to sprinkle a few drops of his diluted mixture into each utensil. The amount used was the same in each case; but the appearance produced varied immensely; and, according as the chemical change was excessive or otherwise, he judged of the condition of the individuals who had passed the "motions." "This patient," he would say, "is not very bad; that one is seriously ill; this one is dying; this one is nearly dead," &c. As his observations were correct, he was asked how he judged of the danger the patient was in? His reply was, "that he had found, in the course of his experiments, that fæces decomposed rapidly or otherwise according to the debility of the individual passing them."

For a long period, the principle here enunciated seemed to belong to the class of interesting but useless facts. More recent observations have, however, shown that it may be turned to good practical account.

If any one will diligently consult the napkins used by infants, he will find that, during the time the motions are of a good healthy yellow colour, they have a peculiar odour, which they retain for twelve hours at least; but if, from any cause—*e. g.*, debility in the nurse, or inappropriateness of the food—the child loses its healthy condition, the motions not only change in colour and consistence, but in smell, and decompose in a very short time after being passed. Where there is diarrhœa and excessive depression of the vital powers, the motions are often found to be decomposed in a few minutes. We may notice, too, that a similar result is met with at the same time in the other secretions of the child; and that the urine decomposes quickly, and the breath is foul or sickly.

But it is not in children alone that this change may be detected: it is equally evident in adults. If, for example, the doctor is called to attend a case of diarrhœa, where there is always more or less debility present, he may consider it necessary to inspect all the alvine discharges that take place. His visits are at intervals of twelve hours only; and he has on each occasion placed before him perhaps as many as six motions in different utensils. He is probably struck with the different odour exhaled from the various specimens, and notices a difference in the colour; but a few words from the nurse soon explain the mystery. The dark brown stinking ones are those passed the longest period ago; the healthy looking and smelling ones are those passed only a short time before the doctor's visit.

Simple though this fact seems to be, it is one which is not universally acknowledged and acted on; I have known "motions" which have simply become decomposed taken for "foul secretions," and the patient dosed with mercurials, under the impression that they would improve the condition of the bowels. The result has been what might have been anticipated; the patient has got weaker, and the bowels no better. The following case came under my notice some time ago. It is valuable as illustrating the danger resulting from inattention to these points:—

An elderly gentleman was under treatment for indigestion. He was improving upon a tonic plan of treatment, when he was induced by his friends to have "a second opinion." When the physician called, he was shown a motion which had been passed twelve hours before. It had undergone decomposition, and was pronounced to be extremely "vitiated;" and, with the intention of improving the secretion, a mercurial alterative was prescribed. This acted freely; and when the visit was made the next day, the motions were all inspected, and as that passed the last seemed to be the most healthy, the natural conclusion was that the medicine had done good. It was, therefore, persevered with; on the next occasion, and for some time subsequently, only the last motion passed was inspected, and, as it had not had time to be decomposed, it was thought to be healthy, and the patient was supposed to be "better," as his secretions were no longer "vitiated." But, notwithstanding this opinion, it was clear to the first attendant that the man was getting more feeble and debilitated day by day. If the medicine did improve the secretions, it certainly impaired the strength. It was then doubted whether the first inference was correct; and, to decide this, the nurse was directed to save all the "motions," and arrange them in the order in which they were passed. When they were inspected, the same order of things was noted as at the first examination; but the older ones appeared more vitiated than ever, and the most recent ones had begun to change in colour. It was interesting to know how the same fact struck the two doctors. One remarked, "that with such excretions, there was greater necessity for an alterative than ever;" the other said, "that seeing such had been the effect of the alteratives which the patient had already taken, the sooner they were suspended the better." This led to a warm debate, which was ultimately decided by an appeal to the nurse. Thus: "Nurse, which is the last motion passed?" "This" (the healthy-looking one.) "How long has it been passed?" "An hour." "What was the appearance of the other motions when they were passed?" "They looked precisely the same as the one first referred to." "Then they have all changed in appearance since they were placed here?" "Yes." "Do the motions change in appearance now more rapidly than they used to do?" "Yes." This confirmed the idea that the "vitiated character of the excretions" was, so to speak, a *post mortem* appearance, and simply indicated a smaller amount of vital force (as opposed to chemical) than is usually possessed by vital products when separated from the body. The termination of the case showed the justice of this view, for the patient's strength continued to diminish, and he died shortly afterwards of pure debility and exhaustion. It is useless to speculate upon what might have been the result had the phenomena been read correctly from the first.

As decomposition takes place in the alvine secretions very rapidly in fevers and all diseases marked by great debility, the practitioner must ever have his attention alive to the fact, that what he calls "vitiation" may be more apparent than real, and a sign which calls for stimulants and strengthening remedies rather than mercurial alterative medicines, whose invariable effect is to make a weak patient weaker.

The excretions of the body being comparatively few, and for other reasons, we cannot prosecute our inquiries into the influence of vitality upon them in the same way as we did concerning the function of secretion. We are not in the habit of requesting our patients to retain their saliva, their tears, their leucorrhœal or catamenial secretions, &c., for our inspection; consequently we know little about the phenomena of decomposition in these fluids and the changes effected thereby.

There are, however, two other excretions with which we are tolerably familiar; namely, the breath and the urine, respecting which we may say a few words. As a general rule, we know that the breath of healthy children is free from any odour perceptible to our senses; and we may say the same of that of healthy adults. But as soon as a child begins to suffer from debility, we know that we can recognise in its breath a variety of unpleasant smells, which we designate as sour, sickly, or putrescent. These odours are increased in intensity and duration by every cause which tends to augment the existing debility; and they go away as soon as the health is restored. In adults, the influence of debility in producing "foul breath" is very remarkable. I know individuals in whom it is invariably produced by a day's fatigue, by the occurrence of the catamenia, or by the too prodigal use of aperient medicines. In these instances, it is cured by wine, tonics, and rest, as certainly as diarrhœa is checked by opium, &c. In others, it comes on in consequence of indulgence in such passions as anger, or from excessive fear, anxiety, and disappointment. In others, it precedes and accompanies asthenic indigestion, and in them a debauch is as surely followed by foul breath, as it is followed in others by nausea and headache. Of course, it may be argued that this foul breath is not due to decomposition of the expired air after it leaves the lungs, but simply to the exhalation of "crudities" already existing in the blood. I acknowledge the force of the argument; but it matters little which solution we accept, so long as we recognize in such phenomena the presence of impaired vital force, and the necessity that exists for adjusting the work to be done to the constitutional power to do it.

There can be no such difficulty respecting the urine. Under ordinary circumstances, this excretion continues for about four-and-twenty hours, without undergoing any perceptible decomposition; but when the individual passing it is weak, decomposition takes place with a rapidity varying according to the amount of debility present; nay, we may go further, and say that in some instances—as in paraplegia, where the vitality of the kidneys and bladder is very low—the urine is actually decomposed ere it leaves the body. The decomposition is recognised in various ways: by the smell, by the occurrence of vegetable growths, vibriones, torulæ, &c.; by the abundant formation of ammoniacal salts; by turbidity, &c.

It is scarcely necessary to remark that we are fully alive to the fact that atmospheric conditions have a great influence in promoting the rapid decomposition both of urine and fæcal matter. What we want to call attention to is, that if on a cool day and in a cool room the urine of a patient shows evidence of decomposition in twelve hours, it is evidence of a debilitated condition of the bladder, the kidneys, or the system generally. As such conclusion seems very dull and meagre however, when thus announced, we will endeavour to point it by recording the case which determined the writing of this communication.

A medical friend sent me a bottle of urine, with the laconic request that I would give my opinion upon it, and my views of the treatment to be recommended. The sole information he imparted to me was, that it was passed that morning by a boy about ten years old. After allowing the fluid to stand some hours, it was examined microscopically and by simple chemical tests, &c., but nothing wrong could be found with it, except that it had begun to decompose. The first impulse was simply to communicate that fact to my friend, and leave him to draw his own inference; but as it would be a useful mental exercitation to put down the conclusions which might be drawn from that fact alone, I resolved to give an account of the symptoms that I thought the case would present.

The following was the train of argument: As there is an absence of much vesical mucus, triple phosphates, &c., there is presumptive evidence against the idea of vesical or renal disease. There is then general debility; and the presumption is that there will be debility in all the organs of the body, and consequent disorder of function. The signs by which deficient vital power in the brain and nervous system, in the respiratory, digestive, cardiac, and o her sets of organs, might generally manifest itself, were then mentioned; and the note ended by recommending a strengthening plan of treatment. In reply to this communication, my friend reported that not only were the symptoms correctly described, but that some had been mentioned in the note which had not been recognised before its receipt. The plan suggested tallied entirely with his own views, and he felt his hands proportionally strengthened thereby.

The practical conclusion to be drawn from the foregoing observations is obvious. If the physician finds that the excretions of any of his patients decompose more rapidly than they would do during health (under the same external circumstances of light, air, and heat), he may feel certain that the vital powers are seriously impaired; and if under the treatment he adopts the decomposition occurs at an earlier period than it did before, there is reason to inquire whether such phenomenon is due to the augmentation of the disease itself or to the medicine administered for its cure. It will then be a subject for consideration, whether the plan of treatment is to be further developed in the same direction as before, or to be fundamentally changed.

Nor is this a matter of so slight importance as it might at first sight appear. Many seem to think that it is the simplest matter in the world to enable a patient to regain strength; and, consequently, they care little about employing medicines which, while they reduce the powers generally, seem to have some special influence over particular organs. But it is by no means an easy matter to enable a patient to regain health; and any one who systematically endeavours to do so will have to acknowledge that it is one of the most difficult problems in medicine. It is easy to reduce the strength, for that we have a host of drugs; but directly to increase it, we have literally none.—*Brit. Med. Journal, June* 11, 1859, *p.* 461.

141.—*On Tracheotomy.* By F. C. Skey, Esq.—[Mr. Skey made the following remarks on tracheotomy after operating on a child in whose trachea a tamarind stone was lodged. The stone was discharged through the opening by a sudden cough.]

Tracheotomy is an operation not very readily performed, and still less so when it is required to open the trachea low down in a child by gas-light. It is not always easy to hit the exact line of interval between the sterno-thyroid muscles, and this can only be effected by dissecting quite vertically from the surface, on which the outer wound occupies exactly the mesial line. When exposed, the trachea should be opened freely, without regard to the thyroid isthmus. Unless time be an object of consideration, (and it is rarely so urgent as to require the operation to be hastily completed,) any reasonable number of minutes may be devoted to the entire arrest of bleeding. The size of the aperture in the trachea will depend on the motive dictating the operation. If for the escape of a foreign body such as a plum or a tamarind stone, the opening should be large—in truth, as large as it can be made in a child of five years of age. In the case of this boy (as well as in that of a child operated on by Mr. Paget last year, who adopted my suggestion of considerably lengthening his incision in the trachea,) the opening was very large, extending, I believe, through at least five or six rings of the tube. I doubt the expediency of the attempt to remove the offending body by means of forceps of any description yet invented. Preferable is it to await the return of cough, which, in the act of expiration, will inevitably carry the foreign body with the current of air through the larger and the nearer orifice in preference to the smaller and more remote. Inasmuch as we are supposed to be acquainted with the nature, and therefore can form a tolerably accurate idea of the size, of the foreign body, we can in some measure judge of the magnitude of the opening required for its escape. I am not aware of any great increase of danger or difficulty created by the division of a greater over a less number of rings, or, in other words, in making a large opening instead of a small one.—*Lancet, Aug.* 13, 1859, *p.* 160.

142.—ON IDIOSYNCRASIES.

By T. W. Nunn, Esq., Assistant Surgeon to the Middlesex Hospital.

Instances of the poisonous effects, on certain constitutions, of drugs ordinarily of moderate action, are not unfamiliar in medical practice. Ipecacuanha is perhaps one of the best examples of the drug that, even in a state of the minutest subdivision, is capable of producing, idiosyncratically, extreme results. But some articles of diet also are, to individuals, poisonous; a numerous class of persons might be easily found, the members of which could not take, without inconvenience one or other of the various alimentary substances. There are, doubtless, scattered over the field of unrecorded medical experience, very many interesting and curious examples of this idiosyncratic incompatibility or susceptibility. The production of spasmodic asthma seems to be the most frequent symptom of idiosyncratic poisoning. Irritation of the mucous and cutaneous surfaces is also a common one; direct influence of a severe character on the nervous system, a rare one.

The object of this communication is, however, not to discuss the physiological questions which naturally arise in the consideration of such a subject: it is rather to offer an instalment of facts. I am the less willing to submit any theory on such a purely medical subject, since I have perceived that Dr. Hyde Salter (from whose able pen much information may be expected), in his paper on the Varieties of Asthma (Edinb. Med. Journal, May 1859), promises to enter fully into the subjects of hay asthma and asthma from animal emanations.

Case 1. *Poisoning by Rice.*—J. M., an occasional patient, cannot eat rice in any shape without extreme distress. From the description given of his symptoms, I believe spasmodic asthma to be the cause of his discomfort. On one occasion, when at a dinner party, he felt the symptoms of rice-poisoning come on, and was, as usual, obliged to retire from the table, although he had not partaken of any dish ostensibly containing rice. It appeared, on investigation, that some white soup, with which he had commenced his dinner, had been thickened with ground rice.

Case 2. *Rice-Poisoning.*—A gentleman who, as in the preceding case, could not eat rice "without being suffocated," took luncheon with a friend in chambers. The fare was simple—bread, cheese, and *bottled* beer. On the usual symptoms of rice-poisoning seizing him, he informed his friend of his peculiarity of constitution. The symptoms were explained by the circumstance of a few grains of rice having been put into each bottle of beer, for the purpose of exciting a secondary fermentation.

Case 3. A gentleman, some time since under my treatment for stricture, informed me that he could not eat figs without experiencing a most unpleasant formication of the palate and fauces; and that the fine dust from split peas produced the same sensation, accompanied by a running at the nose. The father of this gentleman suffers from hay-fever at certain seasons.

Case 4. Mr. P., himself a gentleman of a peculiarly nervous temperament, states that his father cannot endure the sensation produced by handling a russet apple. He also communicated to me

Case 5. that of Mr. T., who cannot remain in a room in which there is a cooked hare, on account of the peculiar effect produced on his system.

Case 6. Miss ——, after eating egg, suffers from swelling of the tongue and throat, accompanied by "alarming illness."

Case 7. Miss ——. In this case, somewhat similar effects follow the taking of honey of any kind, and especially honey-comb, into the stomach; viz., swelling of the tongue, frothing of the mouth, and blueness of the fingers.

Case 8. The following is an extract from a note received by me from a lady, who says:—"I had on three mustard plasters—one on the throat, one on the back of the neck, and another under the left shoulder. They remained on half an hour. Cotton wool was applied on their removal. About thirty hours afterwards, a painful stinging sensation commenced in the back of the neck, followed by violent twitchings of the muscles of the face, arms, and legs, which continued in regular succession through the whole night. It yielded, after about twelve hours, to hot fomentations of poppy-heads applied to the back of the neck." It cannot be ascertained that any medicine containing strychnia was taken.

Case 9. A gentleman, a member of the medical profession, with whom I am well acquainted, suffers from nettle-rash after eating veal. Veal has the reputation of being particularly indigestible. The above instance of the production of urticaria from its use is, doubtless, not an uncommon one.

Case 10. I have been informed of a lady who cannot remain in a room in which there is a cat. Although the cat may be concealed, the lady's peculiar sensations immediately declare to her the presence of the animal.

Case 11. A patient under my care, since dead of cancer, was invariably thrown into a state of nervous excitement by the exhibition of the compound infusion of orange-peel.

Case 12. A personal friend of my own suffered from erythema nodosum after eating shrimps, although they were perfectly fresh. I believe shell-fish generally is particularly liable to excite unpleasant consequences.—*Brit. Med. Journal, July* 11, 1859, *p.* 460.

143.—ON PURE WATER.

There is not any form of sanitary improvement in which medical practitioners and officers of health are more highly interested than the supply of mechanically and chemically pure water for household and portable consumption. In the face of the great difficulties which encumber the purification of the Thames, the source of our water supply, we Londoners are especially interested in whatever can effectually and cheaply remove the animal poisons and putrid abominations with which the fluid in our cisterns and water-vessels mischievously abounds. Of filters, the name is legion. But a substance has lately been introduced to the notice of scientific men, as a new filtering and purifying medium, of which the qualities are affirmed to be of so remarkable a character that they deserve a careful investigation from our eminent chemists and sanatarians. This substance is the feroso-feric carbide of iron, or magnetic carbide of iron, the properties of which have been investigated by Professors Brand and Clarke, and Mr. Thos. Spencer, and are very ably described by the latter gentleman in an interesting letter lately printed, and addressed to Mr. Charles May, F.R.S. As a mechanical filter, the carbide of iron is peculiarly effective and rapid in its action, by virtue of its high magnetic power; for it is by the high magnetic attraction of the light bodies held in suspension by water that mechanical filters generally, such as sand and other similar media, effect filtration. Where the magnetic power is low, a finely-grained layer of filtering material must be employed; and the higher the magnetic power the coarser may be the grain, the larger the interstitial spaces, and therefore the more rapid the filtration. The fact stands as to the considerable power of the carbide, and this is the theory by which Mr. Spencer explains it.

The chemical purification of the water is effected yet more remarkably. The magnetic carbide possesses the singular power of attracting oxygen to its surface, and condensing it there, without entering into any chemical combination with the gas, although catalytically affecting its properties; just as a magnet will attract a loose heap of iron filings, polarize them, in arranging them in stiæ, and endow them with properties other than those which they possessed before. It will be remembered that Schlönlein first observed that the oxygen of the atmosphere in the vicinity of an electrical machine, which had been recently employed, contained an altered form of oxygen, which he called ozone—a form of oxygen which possesses all the powers of that gas in an intensified degree, and has the great quality of combining with and neutralizing every kind of noxious body of organic origin. This ozone, the great natural agent of purification, is generated in quantity on the surface of the proto-carbide, and energetically manifests its presence by the exercise of its splendid chemical powers of purification. Passed through this filtering medium, water is deprived of all colour, taste, and odour; nearly all deleterious gases which it can contain, as sulphuretted or phosphuretted hydrogen, are rendered innocuous by the forced combination with oxygen; soft water thus treated has been proved by Mr. Spencer to have no action on lead; and, finally, water so filtered has very little, if any, tendency to give birth subsequently to animal or vegetable organisms.

These properties are so valuable, so highly interesting in a chemical and physical point of view, and so serviceable to the sanatarian, that important results must arise from the further application of the powers of the magnetic carbide.—*Dublin Hospital Gazette, Aug.* 15, 1859, *p.* 255.

144.—*Antidote to Strychnine.*—Dr. Bewley, wishing to kill a mangy cur, and having read in Magendie's "Report on Strychnia," that the sixteenth of a grain will kill the largest dog, determined to make sure of this very little animal by giving it about half a grain. But either Magendie's statement was incorrect, or the drug was adulterated, for at the end of ten minutes the dog, though suffering frightfully, was not dead. Dr. Bewley resolved to put him out of his misery at once, and accordingly mixed half a drachm of prussic acid with a little milk, and put it under the dog's snout. He lapped the milk with avidity, and in less than a minute vomited, got upon his legs, ran away, and recovered.—*Med. Times and Gazette, Aug.* 6, 1859, *p.* 149.

145.—*Tests for the purity of Chloroform.*—M. Berthe gives the following directions in the "Moniteur des Hopitaux:" Chloroform may contain chlorine of elaidine, alcohol, various chlorides, amylic and methylic combinations, and aldehyde. By adding caustic potash to chloroform, containing chloride of elaidine, the compound is transferred into chloride of acetyle, the fœtor of which is immediately noticed. In order to ascertain the presence of all the other compounds which may be mixed with the chloroform; especially alchoholic compounds, pound a small quantity of bichromate of potash in a little chloroform, and add to this mixture a few drops of sulphuric acid. If the chloroform is pure, a reddish-brown precipitate of chromic acid is formed; if not pure, the acid is reduced, whilst the precipitate, or sometimes the liquid itself, assumes a green colour, dependent on the presence of the sesqui-oxide of chrome.—*Lancet, Aug.* 27, 1859, *p.* 218.

146.—*The Male Fern in Snake-Bites.*—We learn from the "Journal of the Society of Arts," that a public trial has been made in Melbourne of the value of an antidote for snake-bites said to be known to a Mr. Underwood. The experiments were made in the rooms of Messrs. Easy and Co., Auctioneers, Collins-street, in the presence of about five hundred spectators. The snakes employed by Mr. Underwood were a whip-snake, about fifteen inches long, and two diamond snakes, one about twenty inches, the other three feet six inches, in length. The larger of the diamond snakes Mr. Underwood provoked till it bit him on the lower part of the forefinger. A rabbit was bitten several times by the whip-snake; but neither the rabbit nor Mr. Underwood appeared to be in any way inconvenienced by the bites. The experiments were declared, however, not to have been satisfactory, and the secret of the antidote was not revealed.

In the same journal is inserted the following extract from the "Hobart Town Mercury:"

"According to the 'Cornwall Chronicle,' 'the secret so long confined to the heart of Underwood,' in reference to his antidote to the bite of snakes, has at length been discovered, and the common male fern—*polypodium filix mas*—is stated to furnish the remedy. This very common plant has been long known as a specific in the cure of worms, especially the tape-worm—the powdered root being generally used for this purpose; but from circumstances which have transpired it would appear that Underwood uses a decoction, or broth, of the leaves near the root, as being stronger, perhaps, than those near the apex of the plant. Its power might probably be augmented if used in the form of a tincture; that is, with an ounce of the leaves steeped for a fortnight in a pint of rum or brandy; in which state it could be kept for any length of time, if well corked, without deterioration by fermentation or otherwise."—*Lancet, Aug.* 27, 1859, *p.* 223.

147.—*Arsenical Poisoning; Good Effects of the Carbonate of Iron (Sesquioxide.)*—L'Union Médicale of the 26th ult., gives an extract from the Italian journal, Il Filiatro Sebezio, in which paper M. Trapani has published a case of poisoning with arsenic. After emetics had been freely used upon the four patients affected, who all presented the usual symptoms of arsenical poisoning, the indication was to give the hydrated peroxyde of iron, the efficacy of which in such cases is universally acknowledged. But it is not always easy to procure it, hence it becomes important to ascertain whether other martial salts will act, to a certain extent, in the same manner. The carbonate of iron (or rather the sesquioxyde, as the carbonate, when kept any time, soon passes into this state) was here given, and with the best results.—*Lancet, Aug.* 20, 1859, *p.* 189.

148.—ON THE HYPODERMIC TREATMENT OF DISEASE.

By Charles Hunter, Esq., late House-Surgeon to St. George's Hospital.

[In a previous paper the writer detailed some interesting cases, (see *Retrospect*, vol. xxxix, p. 50,) of which, at the commencement of the present one, he gives a slight summary.]

The cases detailed were fifteen in number, including the two in which I first tried the local injection of Dr. Wood. A brief review of the cases is the more satisfactory for these reasons—1stly, because in all of them the same narcotic, the acetate of morphia, was used; 2ndly, because they had all been under other treatment previously without avail, and in most of the cases it was the same preparation, administered either by the stomach, or skin, which had previously failed.

All the cases were affections of the *nervous system*; nine of the brain, two of the brain and spinal cord, and four of particular nerves.

Although a narcotic, and the same narcotic, was used in every case, it was not always with the same object; thus, in some it was to procure sleep, in others, to ease pain or allay spasm, and in others, again, to attempt to palliate or cure soma neuralgic affection.

Three of the cases were *neuralgia*, which had all failed to receive benefit under other treatment. although in each case it had been very varied. The two first received great benefit, firstly, from Dr. Wood's plan, and subsequently from my own, as stated in the Medical Times and Gazette, October 30, 1858; but the ultimate result of the treatment in these two cases has not yet been given.

Case 1.—The man with constant tic-doulonreux of four years' duration went out cured, the injection of the morphia being at one time used three times a-day, so as to keep up a full and continued influence to prevent, as long as possible, a recurrence of the attack; it was then gradually left off, and the man went out free from neuralgia about a week after the cessation of treatment. The injection was always made in one or the other arm, the site being varied each time; no local inflammation ever occurred, nor was there ever any sicknes.

Case 2.—The girl with neuralgia and disease of the eye went on with the treatment till the pain gradually diminished; she then left the hospital. The treatment in this case, owing to its nature, could only be palliative. The disease has now attacked the brain (having destroyed the eye), and she is now (June) being treated by this plan of treatment in another of the London hospitals, as nothing else seems to ease the pain or give her sleep. No sickness ever followed the injection of narcotics, but frequently did their administration by the stomach.

The third case of neuralgia was cured by a single injection, although the case had resisted many other forms of treatment. Sickness took place in this case, and was considerable.

The cases of delirium tremens were two. Morphia in the first, and opium in the second, had been given in large and repeated doses, with no decided effect. In both, the first injection caused sound sleep of many hours' duration; and both went out cured in a few days. There was no sickness in either case.

The cases of *mania* were two. They both showed that sleep could rapidly be obtained by the injection of morphia, which effect had not been obtained by doses as large, and larger, given by the stomach. There was no sickness in either case.

In the first case of *puerperal mania* the injection was used only once; sleep followed in eight minutes. The patient subsequently recovered by the administration of large doses of Battley's sedative. In the other case, several injections were employed, sound sleep following each time. There was no sickness in either case.

In both cases of *wakefulness*, sleep was rapidly induced; and in one patient, excitement, almost amounting to delirium, was quelled by a single injection.

In the case of chorea the effect of the injection was always very rapid, sleep resulting in about four minutes if the quantity injected was large, and if small, the violence of the movements was diminished or arrested. No sickness was ever produced.

In the case of tetanus sleep resulted at once, although the spasms remained unaffected. In a second case of tetanus in which this treatment has been used, two hours' sound sleep followed the injection, and afterwards the patient dozed for several hours; laudanum had been previously given every hour without effect. The spasms were never violent in this case, and were observed to cease during sleep.

Both cases of sciatica went out free from all pain, the one that was cured was a little sick after the first injection; in the other case the pain returned in a milder form.

Such was the result of the treatment in the cases already given; the result was different in the various cases, even as the object was different with which the injection was employed. Sleep was the result in most cases, but not in all; spasm was quieted in some, and pain was relieved or cured in others by a single or by more injections.

With regard to the sleep occasioned by the injection, it must not be looked on as a necessary effect of the treatment, it may follow at once, after a time, or not at all, according to circumstances. Thus it may follow—*At once*, if the quantity injected is large, and the object is less to ease pain than to procure sleep. *After a time*, if the quantity is large (say one grain of morphia), and much pain exists; in which case the pain is generally quieted directly, and sleep follows in from fifteen to thirty minutes. *Not at all*, when the quantity injected is small, and there is much pain, spasm, or cerebral excitement going on, in which cases, as the quantity is small, the whole effect of the narcotic is expended either in subduing the pain and spasm, or allaying the excitement.

The Occasional Sickness.—In the fifteen cases, sickness occurred *in only two*, in one it was distressing, in the other but very slight.

In *another series of fifteen* cases, of which I have notes, sickness occurred in *four*, in one it was considerable, and in the others to no extent.

Consequently in *thirty* cases only *two* patients, both of them women of very nervous temperament, and both suffering from tic-douloureux, had considerable sickness; in the other *four*, all men, with sciatica, the sickness was trifling; in fact, the patients themselves thought nothing of it, nor did they think it due to the injection. This cannot be called a large proportion when it is recollected how often morphia causes sickness when given by the stomach; I have constantly seen it in quarter of a grain, and laudanum in equivalent doses, cause sickness, so administered.

The *time* when the sickness comes on varies; in the two cases in which it was severe, giddiness and nausea were felt almost immediately, then faintness, till in about five minutes sickness took place; in both it continued on and off for several hours, with intervals of sleep. In the other four patients the sickness did not come on for many hours, in fact, only as a kind of ultimate effect of the morphia after a good sleep.

Cause of the Sickness.—From the preceding remarks it appears that sickness may be looked on as either a first, or as a last, effect of the narcotic. That when a *last* effect, it is but trifling, coming on after many hours' sleep, preceded for a little while by nausea, and disappearing generally after the patient has been once or twice sick, with scarcely a straining effort. A slight excess of the narcotic m y be looked on as the cause of this sickness. But in both the cases in which the sickness was urgent, the patients, both women, besides being, as above stated, highly nervous, were both badly affected by narcotics, however administered; in both, the quantity injected was *less* than was employed in the cases *where slight sickness* ensued, and in many cases where none at all took place—in the one less than a third of a grain, and in the other a little more than half, was the quantity injected. The *cause* of the sickness in these two cases, as it was so immediate, seems to have been due rather to a peculiarity in the constitution of the patients, than to the amount of the narcotic employed.

Ought the occasional occurrence of severe sickness to cause the injection of remedies to fall into disuse? I think certainly no more than it ought the introduction of narcotics in *other ways*, because sickness occasionally follows *their* administration. But as medicines, hypodermically introduced, act with greater rapidity and effect than when administered by other methods, it behoves us to be the more careful in the selection of *proper cases;* and where it is desirable to employ the treatment in such cases as the two where sickness occurred *directly*, to inject a *much smaller quantity than usual;* for it is not improbable that in those two cases, as benefit resulted in both, that a still smaller quantity would have sufficed.

The Choice of Cases.—This plan of treatment is no specific; because it acts marvellously in some cases of neuralgia, or of pain, is no reason why it should cure all. For this reason, *a due discrimination of cases ought to be made.* The same caution is given by Dr. Wood in his paper on the treatment of neuralgia by *local* narcotic injections: "Another caution I would offer is, that you choose the proper patient for the use of the remedy."* When to employ the injection, and when not, must depend in the general way on the particular circumstances of each individual case, such as the nature of the disease, its urgency, the object in view, &c.

There are some cases in which I think the hypodermic injection may almost be employed *as a rule*, and be put in force *before time is lost* by the adoption of other measures. I mean those cases of high cerebral excitement, of delirium tremens, and of mania, in which the speedy administration of a narcotic is indicated. In this class of cases more than any other the value of the injection is seen. I have already detailed *seven* cases of this nature, and could give many more, but there hardly seems the necessity. In these cases, to procure sleep and allay excitement is the object, and that as soon as possible; the stomach is often irritable, or in such a state that it will not absorb medicines; the patients often refuse to swallow; everything, in fact, points to the necessity of some more sure, speedy, and active mode of treatment than the more ordinary one of stomachic administration.

There are cases of sudden, violent, acute pain, in which the injection might also be tried as a *primary measure;* for instance, during the passage of a renal calculus, in such a case the pain is at times almost insupportable, and as the stomach gets quickly irritable, sickness often taking place, another reason is furnished for the trial of this plan.

Then there are cases in which the injection ought not to be tried at first, but after such general treatment as is clearly indicated has been first tried —such as purging, or the internal administration of alteratives or tonics. Tic-doloureux, sciatica, and many other neuralgic affections are of this class. In all such cases, due discrimination being made, it is astonishing what benefit follows the injection in most cases, and how quickly in many, a cure is effected by it when other treatment has altogether failed. It is often, too, in those cases the origin of which is most obscure, that the injection seems to answer best.

Rheumatism is a disease in which occasionally the use of the injection into the cellular tissue will be found highly serviceable. Two cases have been treated by it at St. George's Hospital:

1. (*Case* 16.) Under Dr. Page, a man almost crippled by rheumatism, had the pain greatly mitigated by the injection.

2. (*Case* 17.) Under Dr. Pitman, a man that could not move his arm after acute rheumatism on account of pain in the shoulder. The pain was removed by a single injection.

3. (*Case* 18.) A gentleman under my own care, who suffered acute pain in the shoulder and arm which prevented his moving the limb and sleeping at night. It was removed after two or three injections.

There are many diseases in which the pain accompanying them must be looked on, not as the essence, but as a *very important* item, which keeps up the disease and which prevents treatment doing any good, but which, once subdued, then the inflammation or whatever the case is, rapidly becomes affected by the treatment which it had up till then resisted. In such diseases the injection may do much, and that speedily, and certainly deserves a trial.

Case 10.—The following is a case where the patient, having a peculiar destructive inflammation of both eyes suffered almost uncontrollable pain, which was unexpectedly cured by the hypodermic injection:—

T. B., aged 41, was admitted April 12, 1859, into St. George's Hospital, under the care of Mr. Tatum. For the first three weeks he suffered acutely, the conjunctiva of both eyes was greatly chemosed, red, and tender; for the greater part of this time he *seldom closed his eyes day or night*. Notwithstanding leeches, blisters, calomel, and opium, and finally morphia, a quarter of a grain every three hours, were employed. The injection of half a grain of morphia into the arm was then tried—it eased him for some hours, but did not cause sleep. Two days after the injection was again employed, one grain being used this time, the patient describes the effect of the injection "as something which instantaneously ran through his frame, round his head, and which seemed to go out of the back of it." The *pain was gone*, and he slept in about ten minutes. The sleep lasted six or seven hours. The patient went out about three weeks after the second injection, during which time he had no more pain, and slept well every night.

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* My object in alluding to this passage (British Medical Journal, August, 1858,) is this: that as I do not find localisation to the painful part necessary, my plan is available in many *diseases* in which that of Dr. Wood is not—as mania, delirium tremens, &c. It is also applicable in some cases of pain and of neuralgia in which Dr. Wood's cannot be—viz., those in which the nerve cannot be reached for injection. There is, therefore, a greater need of caution to discriminate proper cases where the range is *wider*, than where it is more limited.

Thus there are cases where this treatment may be employed, and, as shown above, with the greatest advantage—1. As a primary measure. at the onset of the disease, without delay. 2. As a secondary measure, general treatment being first used; and, 3. As an ultimate measure, to attempt to palliate or cure, where other treatment has failed.

The Choice of a Narcotic.—This must depend entirely on circumstances, such as the sex, the peculiarities of the patient, the disease, and the object in view. It is not my intention here to go into these points as they require *almost* equal consideration before the administration of a narcotic by any method, but rather to indicate in this place the *most eligible preparations for injection.*

Tinctures may, and can be, used with good effect. Thus Mr. Burns used equal parts of the tinctures of opium and hyosciamus. I have employed both these tinctures separately and have found this objection to them, that they cause a little hard lump beneath the skin which *may* last a considerable time, and gradually disappears if left quiet and alone. There is not this objection if the tincture be evaporated to one-half or one-third, and be used while it is fresh.

Dr. Wood has used a solution of morphia in sherry wine as "it would not irritate and smart so much as alcohol." He also says that "nepenthe" produces less sickness than opium, and is therefore preferable as an injection. Professor Simpson has used a *solution* of the bimeconate of morphia in coccodynia.

For my own part, I *prefer solutions to tinctures;* they are rapidly absorbed, they produce no irritation if properly made, and they have the advantage, viz., of exactness, so that no mistake need be made about the strength of the preparation, or the quantity injected.

A solution of acetate of morphia is the preparation I have used more than any other, prepared with acetic acid, but so freed from excess of that agent that it causes not the least irritation. Mr. Williams, of Liverpool, who has tried the narcotic injection in delirium tremens, has proposed to me the employment of the sulphate of morphia on account of its ready solubility.

A solution of the sulphate of atropine is a good preparation for injection and not liable to irritate. I have several times employed it, and produced sound *sleep* with doses varying from the one-twenty-sixth to the one-tenth of a grain. The injection of this salt has been employed with a different object by Mr. Benjamin Bell, viz., to counteract the effects of opium poisoning as first suggested by Dr. Thomas Anderson.

Is Chloroform applicable for Hypodermic Injection? Chloroform is a narcotic which may be, with safety, injected into the cellular tissue in *urgent* cases: it rapidly produces cessation of spasm, and causes sleep.

[But chloroform thus employed produces local effects in some cases which renders its employment thus unadvisable, except as stated in *urgent* cases. The author then comes to the following practical conclusions, as to the value of medicines injected beneath the skin.]

1. That certain medicines may be introduced into the cellular tissue beneath the skin with safety and with advantage.
2. That medicines so introduced have a *general* as well as a local effect.
3. That the general effect of medicine so introduced is exceedingly rapid.
4. That this mode of administration is *more certain in its action* than stomachic doses are, for the *exact* amount introduced is known, and the

whole of it takes effect, which *may* or *may not* be the case with stomachic doses.

5. Medicines are *more purely received* into the system by this method than when given by the stomach, in which organ they may become contaminated or decomposed.

6. A given amount of medicine employed hypodermically has a greater effect than the *same* amount administered by the stomach; *it also acts more quickly*.

7. A given amount of a medicine employed hypodermically has a greater and more rapid effect than when employed *endermically*.

8. That the medicines for which this mode of introduction is especially applicable are the various *narcotics* and *sedatives*.

9. That the *diseases* for which this plan of treatment is especially indicated are for the most part *affections of the nervous system*:—

1stly. Where the immediate and decided effect of a narcotic is required.

2ndly. Where narcotics administered by the usual methods fail to do good, and yet are indicated.

3rdly. When the effect of a narcotic is required, and the patient *refuses to swallow*.

4thly. Where from irritability of the stomach or other causes (such as idiosyncrasy, &c.) the patient cannot take the medicine by the stomach.

10. That to produce a general effect it does not signify whether the remedy be injected into the cellular tissue of the body or of an extremity.

11. That to relieve or cure a local neuralgic affection there is no necessity to localise the injection.

12. That whether the object be to treat a local or general affection, it seems advisable each time to change the site for injection, should it be more than once required.—*Med. Times and Gazette, Sept.* 10, 1859, *p.* 253.

149.—*Medicated Subcutaneous Injections.*—Dr. Alex. Wood's method of injecting narcotic solutions into the cellular tissue is finding favour in France. M. Béhier, an hospital physician of Paris, has made numerous experiments respecting this mode of relieving pain, and has communicated the results to the Academy of Medicine.

The fluid injected in these experiments was a solution of sulphate of atropine, six grains to an ounce of water, which gives a proportion of the fiftieth part of a grain to every five drops of the solution. Fifty-three patients, affected with various kinds of neuralgia, were injected close to the seat of the pain with this solution; twenty-two others with a solution of sulphate of strychnine, in the same proportion as had been observed for the sulphate of atropine. A solution of muriate of morphia was also injected in a case of slight lead colic. Pain was always relieved, and cures effected in all the cases where the injections were sufficiently repeated—namely, in thirty-one cases out of the fifty-three. Signs of belladonna poisoning occurred in all, which was combated by opium.

M. Béhier had tried to remove pain by injections into the cellular tissue at a distance from the seat of the uneasiness, so as to put the assertions of Mr. C. Hunter to the test; but always unsuccessfully. The same physician thinks that injections of medicated fluids into the cellular tissue afford very great advantages in cases of neuralgia and paralysis; and that these injections will yield the best results in other affections, where it it is important that the medicinal substances should act upon the organism at large. —*Lancet, Aug.* 20, 1859, *p.* 189.

150.—*A New Diuretic.* By Isaac Byerley, F.L.S., Seacombe, Cheshire. —Gentlemen living near the coast, where there are sandhills, will have ample opportunities of testing its efficacy, as it grows abundantly in such localities. It is the "Erodium Cicutarium," the "Stork's-bill."

A neighbouring medical friend, Dr. Parr, who had seen the plant tried, in one instance by an amateur, and in the other under his own superintendence, brought me a specimen that I might name it. He described its diuretic powers as somewhat marvellous, and stated that in each case the curative effects were speedy and permanent; although the varied means usually resorted to in such cases had been steadily but unsuccessfully employed for a length of time previously. A case of my own, in which there is ascites with hepatic disease, is improving under its use, the quantity of urine having increased considerably, with much diminution in the amount of effused fluid. The mode of preparation is to infuse an ounce of the dried plant (every part of it) in three pints of water, stewing it in an oven until two pints remain. The dose for an adult is four or five fluid ounces three times a day; probably more may be needed in some cases.

It would be satisfactory to know the results which may arise from a further application of the remedy in other hands.—*Med. Times and Gazette, Sep.* 10, 1859, *p.* 268.

151.—*External Use of Mucuna Pruriens as an Excitant.* By J. RHODES, Esq., Glossop.—Having often been disappointed in rousing patients when comatose, in apoplexy, &c., by ordinary external stimuli, I have on a few occasions applied the mucuna pruriens on the chest, arms, and legs, in cases where hope of returning consciousness existed. In one or two minutes after its application, the legs and arms have begun to start, and the patient has violently attempted to rub his chest, &c., with his hands; and in a few minutes consciousness has returned. It might be useful in comatose states from chloroform or narcotics in general, and in drowning. I sprinkle it upon the part myself, gently rub it in, and repeat it when necessary. When it has acted sufficiently after the patient is roused, a little olive-oil is applied if its action is too severe.—*Brit. Med. Journal, July* 23, 1859, *p.* 594.

152.—ON THE EMPLOYMENT OF COMPRESSED SPONGE.

By Dr. BATCHELDER.

The compression of the sponge is best executed by means of a copying machine, or in default of this, by laying it between pieces of board upon which heavy weights are laid. If desired to be at hand at all times it must have been kept for weeks or months under the compressing power. When wanted to be used as a tent, the compression should be effected by winding some thread or thin cord around a piece of clean, well-moistened sponge, removing the thread after the sponge has become thoroughly dried. Or, the sponge may be soaked in a solution of gum-arabic before winding, and the tent afterwards smoothened into a proper shape by means of a knife. The winding is facilitated by transfixing the sponge by an awl which is afterwards removed. The tent should always be prepared with mucilage, and not with water, when intended to be used for dilating the canal of the cervix uteri, or any other part where moisture might induce premature expansion, i. e., before it can be properly inserted; also when it is desirable to avoid rapid dilatation, which sometimes causes considerable pain and uneasiness. When the tent can be introduced with facility, the mucilage may be dispensed with; and when it is to be introduced into any internal part it should be transfixed by a needle and thread for its withdrawal.

The production of firm, equally-diffused, and softened pressure by the application of plates of compressed sponge, and then saturating these with water, was first tried in *inflammation of the breast.* In the tent form it has been used to dilate the cervix uteri in *sterility, difficult menstruation,* and other affections of this part. The tent should be introduced by means of a pair of long-bladed forceps, or by fixing it at the end of a small, round, slightly-curved stilet, about twelve inches long, reduced to a point, with a shoulder about five-eighths of an inch from its extremity. The portion between the point and the shoulder should be small enough to be received into the base of the tent, and long enough to keep it steady when being introduced, while the shoulder enables the operator to push it safely home. When, on account of the smallness of the canal, the tent can only be introduced with difficulty, the cervix should be dilated by means of fine metallic dilators, successively increased in size. They should be about an inch and a quarter long, and have a thread attached to them, which being attached to the patient's clothes prevents their being dropped or lost. In some cases a few days, and in others weeks, may be required for such dilatation. The tent should not be allowed to remain longer than twenty or twenty-four hours, and when it is removed the vagina should be well syringed out. When it is desired to induce premature labour, the tent must be continuously applied, or be replaced by a larger, after it has become expanded. In cases of pregnancy attended with hemorrhage, we sometimes find the *os* partly open, but inflamed, or indurated and unyielding—not dilating so as to allow the embryo and placenta to pass in cases of abortion. There may be, indeed, slight parturient pains, but not sufficiently strong. A sponge tent which is sufficiently large to fill the canal should be inserted. This will not only constitute the best tampon, but cause efficient uterine pain, and, at all events, dilate the canal sufficiently to admit of the introduction of instruments to complete the delivery.

Dilatation of Sinuses.—These may be dilated to any desirable extent by the tent, and most of them will heal when a free discharge is thus promoted. From day to day the tent should be lengthened as well as enlarged.

Fistula in Ano.—Many of these cases can be cured by the tent without operation. When an abscess forms near the rectum, it may be separated from this by a mere septum, which remains unruptured as long as a free external discharge is secured. If soon after the bursting of an abscess the tent be fairly and fully inserted, it will not only absorb the matter and prevent its burrowing or pressing upon the septum, but secure it a ready exit by keeping the external orifice open. An opening in the gut is prevented, and the sore heals from the bottom. Every surgeon knows that a communication between the abscess and the intestine does exist sometimes prior to the external opening; but in the majority of instances the external opening first takes place.

Diseased Bone.—The use of the tent is especially appropriate when a fistulous passage leads to diseased bone. "In the course of a fortnight, I have in such cases dilated the sinus, so as to be able to place my thumb and three or four fingers directly upon the diseased bone and remove the sequestrum, if one existed, or if there were caries to remove the diseased portion of bone by repeated application of the sponge. Now caries is an ulceration of bone, which in some respect resembles cancer of the soft parts. In this disease, the relative proportions between earthy and animal matter entering into the composition of the bone seem to be disturbed. In some cases the animal matter, or vascular portion, exceeds the earthy, and then we may have decaying bone with fungous, bleeding masses. In others, the earthy matter may predominate and produce irritation or cause exfoliation. Nature's method of cure, particularly in the last case, is to get rid of the earthy matter, either by solution or absorption, or by wholly depriving it of its vitality, when we have exfoliation or necrosis. The reason why caries is so tedious and difficult of cure, is that the earthy matter, partially or wholly deprived of its vitality, keeps up a constant irritation, which prevents a portion in the immediate vicinity from being duly nourished. Nature, as suggested, causes the death of the diseased portions, and then throws them off. The surgeon, for the same purpose, applies the external cautery or the gouge and chisel; but the action of the sponge is less dangerous, painful, or repulsive, and more certain." Its action upon diseased bone was discovered by the author accidentally, and he has since found most cases of caries yield to its application. Applications of plates of compressed sponge have also been very successfully resorted to in cases of enlargement of the bones, especially in syphilitic nodes.

Stricture of the Rectum.—For dilatation of *stricture* of the *rectum* the sponge tent may be used with great effect. It may usually be prepared without mucilage, except when there is much irritability, when it dilates too rapidly. In some of these cases its toleration has to be secured by introducing, an hour or two previously, a suppository formed of watery ext. of opium, gr. ii., ext. hyoscy. gr. i, ext. belladon. gr. ½.

Hemorrhoids.—The sponge tent introduced just within the sphincter may be successfully resorted to for the compression of hemorrhoidal tumours, and also for the suppression of hemorrhage from the rectum. It should be transfixed with a thread.

Stricture of the Urethra.—A full-sized metallic instrument should be first carried down to the stricture, and pressed firmly against it for a longer or shorter time, according to the degree of uneasiness it produces, when it should be withdrawn, and a tube, open at both ends, containing the tent and a piston, introduced and pressed against the stricture. When the tent has been forced out of the tube, the piston may be withdrawn, and a few drops of water passed down the tube to the tent, which is kept steadily in or against the stricture. In a short time the tube also may be withdrawn, and the tent having expanded so as to retain its place, may be allowed to remain as long as may be deemed necessary. To prevent all accidents, the sponge is transfixed by a thread, to the other end of which is affixed a button. There need be no hurry in withdrawing the tent, the attempts at its expulsion during micturition helping to dilate the stricture. If the stricture is very irritable, a few drops of saturated solution of ext. belladon. and watery ext. opii. may be passed down to the stricture half an hour before the introduction of the tent, the patient passing water shortly before the introduction of the tent or the solution.

Dilatation of the Female Urethra.—The tent is admirably adapted for this purpose, and when incontinence follows its use, this seldom lasts beyond a week or ten days.

Malignant Growths.—Dr. Batchelder has found the sponge successful in some cases of cancer, and thus explains its operations: "The pressure occasioned by the expansion of the compressed sponge disturbs the cancer cells, and forces them out of their place, affects their consistency, and causes them to be dissolved; and the tumour, thus freed from its malignant ingredients, may be more readily removed by absorption, or if not absolutely removed, it may be so divested of its malignity as to remain harmless for years. . . . This remedy may, and undoubtedly will, fail when either the constitution or the surrounding parts are to any considerable degree affected. The cancerone diathesis, even when somewhat apparent, does not, however, seem to preclude its use, as can be seen by the following case, . . . When it can be borne, whether compressed or not, I am satisfied that the sponge is an excellent application to cancerous and certain other ulcers of a vitiated character. The secretions of such ulcers seriously affect the health, and often break down the constitution, and ultimately destroy life, all of which deleterious consequences the sponge, by absorbing the secreted fluid, tends to prevent. In the removal of the elevated, indurated edges of the callous ulcer it is exceedingly useful. The sponge should be large enough to overlap the edges of the ulcer by at least half an inch.

Swelled Testis.—The testis should be turned up on the abdomen and the sponge and spica bandage, properly modified, applied. For a day or two uncompressed sponge may be used; and when the patient has become used to this, compressed sponge may be bound on, the patient being kept in bed, and the bandage reädjusted whenever it slips off. The removal of the swelling, as also of the tumefaction of the glands of the groin is much promoted by the occasional administration of an emetic.

Vegetations of the Glans Penis or Prepuce may be speedily removed by turning the organ up over the pubes, and applying the sponge and a bandage, a flexible catheter being kept in the urethra. The growths are often destroyed with great rapidity, the greatest inconvenience being the confinement to bed.

Non-malignant Tumours.—Enlarged lymphatic glands may by this means be speedily removed; and it is almost a certain means of preventing the suppuration of bubo. In some cases of enlarged inguinal glands, a hernial truss, with a stiff spring and a glass pad, has also been used, the pressure being graduated by the interposing pads of cloth between the spring and the body on one side or other of the tumour.

Enlarged Joints.—Whether the enlargement has arisen from effusion into the joint from synovitis, or from infiltration of the cellular tissue surrounding it, no remedy has succeeded like the sponge, especially under the latter circumstances.

As a Styptic.—This the author illustrates by its efficacy in wounds of the palmar and plantar arteries. "The phenomena attending these wounds are often somewhat peculiar. The blood, forced extensively into the surrounding cellular substance—perhaps under the aponeurosis—seems to come welling out from a considerable surface, which renders it very difficult to find the wounded vessel. If the surface be scraped again and again with the handle of a scalpel, the extent from which the blood issues will be lessened down to a small space. Upon this let a piece of compressed sponge, shaved to a point, be applied, and perhaps another broader piece, followed by a compress and bandage, the wounded part being then brought to an acute angle with the limb above, to which it may be bandaged or confined as closely as consistent with comfort. The uneasiness occasioned by this procedure may be relieved by extending the limb a very little from time to time, just to give temporary relief. Although it is undoubtedly a fact that the position of the limb alone will in most cases control the hemorrhage, in order to be sure I employ the sponge also. With the influence of this arrangement of parts upon the arterial circulation of the hand and wrist I became accidentally acquainted many years ago, by failing to feel the pulse when the arm was much flexed at the elbow. On further examination, the same result, though less decisive, was found to follow the flexure of the leg on the thigh, and still more that of the latter upon the trunk. In some, these positions will render the pulse at the wrist, and also the beat of the anterior and posterior tibials, very weak, or quite imperceptible at the points where their pulsations are most readily felt—a fact of too much importance not to be made available in practice, especially in the treatment of wounded arteries of the hand or foot. It remains to suggest the trial of position in the treatment of popliteal aneurism. The object in such cases being the coagulation of the blood in the tumour, might not this be effected by bending the thigh upon the pelvis and confining it in that position for several days, lessening its irksomeness by the administration of opium, or occasionally ether or chloroform, or by slightly altering the position of the limb?"

Varicose Veins.—In a case recently treated by the author compressed sponge was applied over the internal saphena as it passes behind the condyle, and kept on by a vulcanised India-rubber bandage. When the sponge was wetted it effectually compressed the vein, and the relief was complete.—*New York Journal.*—*Med. Times and Gazette, July* 16, 1859, *p.* 66.

153.—ON THE CHRONOMETRY OF LIFE.

By Jas. Paget, Esq., F.R.S., Assistant Surgeon to St. Bartholomew's Hospital.

The design of this discourse is to illustrate the law that the processes of organic life are regulated with a regard to time, as exact as that which is observed by them in respect of size and weight and quantity of material employed by them; and to show that such an observance of time is characteristic of life, depending essentially on properties inherent in the living bodies themselves, and not on conditions external to them. Laws indicating the limitation of the organic formative processes, in respect of quantity, are evident in the facts that, in the ordinary conditions in which each living being is found, it and all its parts have appropriate size and weight and mutual proportion. These may, indeed, be modified by the variations of external conditions, or by events that are of the nature of accidents; but the range of possible variations is, in nearly all cases, comparatively narrow; and the boundaries are soon reached, in which changes of external conditions become incompatible with life. An instance of corresponding limitation of the organic processes in regard to time might be noted in the natural duration of each creature's life. It is, indeed, not possible to assign any exact number of hours, days, or years, as the constant limit of life in any species; but it is enough to prove a law of time, as limiting the total duration of the organic processes in each, when we see that, in man, and in other species, the length of life, when not diminished by disease or violence, is as fixed as the natural weight or stature is, and that the term of life is marked by changes whose source is inherent in the living body. Watching these changes in the senile degenerations of the human body, it is evident that life does not cease, naturally, because of any change in the external conditions of living; and that the body is not, with advancing years, gradually worn out, as if there were a gradual consumption of a store of material or of force; but that, as, at a set time, the development of the body ceases and growth goes on, and then growth ceases and the body is only maintained in its perfection, so, after a time of such maintenance, the method of the formative processes in the body changes, it slowly degenerates, and through degeneracy dies. And all these stages are alike natural, constant, timely; all, too, are together, characteristic of life; there is no such succession of events to be traced in any form of dead matter. Observance of time may, again, be noted in the formative processes concerned in any of the organs whose changes mark the divisions of a life into its chief periods; *e.g.* in the teeth. Considering merely the conditions in which the teeth of the first set are placed before they project from the gums, there appears no reason why one should be cut before the other, or why they should not all grow with equal speed. Yet while they all grow alike in regard of structure and composition, they have very different rules in regard to the time-rate of their formation. And a yet more marked instance of time-regulation is in the contrast of the teeth of the first set with those of the second. In all essential characters, except those of strength and size, the two sets are much alike; yet there is the widest difference in the rates at which they are formed, and in their duration. The second teeth require as many years for their formation as the first require months; the first live but a few years, the second should live as long as the rest of the body, and sometimes do so. Now there appears nothing to which, as to an efficient cause, this difference can be referred. Its utility and final cause can be discerned; but, as to that which verily determines the rates of growth, and the durations of the teeth, it can only be referred to a First Cause; or it may be said, as of other things subordinate to a First Cause, that it depends on some of those properties which each living being inherits from its parents, and through which it results that, in respect of time, as well as of method and quantity, the formative processes in the offspring are a repetition of those of the parent. The observation of the development and changes of the teeth affords, moreover, an excellent instance of the punctuality with which time-work is regulated in the organic processes, and of the manner in which several different, and really independent, processes, being set to the same time-rate, are made to co-operate to the end of utility in the economy. This is evident in the coincidence of the development of the teeth of the second set, with the removal of those of the first; and in the coincident growth of the jaw, and all its muscles and other apparatus for mastication. In all of these (and the same might be said of any other system of organs in any species) the formation of every part is achieved with an admeasurement of time as precise, and as perfectly designed as that of its shape, or size, or structure. For examples of organic processes, adjusted to be complete in definite periods of time, the germination of seeds and the hatching of eggs could be cited. In plants, and in cold-blooded animals, the time varies according to temperature, yet not without evidence of a proper time-rate; but among birds, each species has its own time for incubation, as fixed as its other specific characters. In other words, the development of the structures of an egg into those of a young bird, appropriately fitted for life in the open air, is timed to a certain rate of progress; so much work is to be done in so many days, neither more nor less; and on each day its appropriate and special portion of the work. And it is evident that the time occupied in the process is determined by the inherent properties of the egg itself. For if the eggs of any number of species be exposed to the same heat and other conditions, in a hatching machine, then, as surely as the bird produced from each will be like its parents, so surely will it be hatched in the same time as its parents were; in other words, the observance of a specific time-rate in the process of development is as exact as that of any other specific character. With this observance of time in the development of the young might be noticed that which is, commonly, coincident in the parent. Not to cite the example of all the mammalia, that of pigeons might be taken, in which, during the incubation of their eggs, the crops of the parents are remarkably developed, so that they may be fitted for the secretion of a fluid destined to make the food of their young offspring more suited for their sustenance. The correspondence of these time-rates, observed at once, in the development of the young pigeons, and in that of the crops of the parents, demonstrates, in both, a provision for chronometry in their organic processes, as clearly as the faces of two clocks, constantly keeping time together, would prove that they both have some apparatus for chronometry within. Further, the provisions made by parents for their future young afford evidence of the time-regulation of organic processes, in so far as those provisions seem to indicate a reckoning of the time necessary for their completion. For example, certain turtles lay their eggs in hollows made in the sand, leave them there to be hatched and at the time of hatching return to them for the sake of their young. It might be asked, how can these creatures, and many others in similar cases, reckon the passage of time? Most probably, they do not reckon it at all: but just as the timely attained fitness of their organisation for preparing and filling their nests impelled them to those acts, so some time-regulated organic processes, taking place in them after the laying of their eggs, bring about at length a new condition, of which a dim consciousness becomes an impulse to them to return to their nests. Such an explanation would involve little guess-work; for changed organisation is, manifestly, often the source of impulse to instinctive actions, and the parental organisation does commonly change at a rate commensurate with that of the development of the offspring. And a similar reference to chronometric processes in the body, might explain many, though probably not all, other instances in which animals seem to have a power of reckoning the passage of time. The phenomena of disease, especially in fevers, agues, the consequences of injuries, and many cutaneous eruptions, would afford abundant instances of the observance of time in the organic processes. The vaccine disease might be generally watched as an illustration, being characterised by a vesicle at each place of insertion of the virus, which vesicle begins to appear on the third day, and on the following days passes through changes which are as exactly regulated in time as they are in visible characters. The changes in this vesicle are, moreover, indicative of a coincident succession of events in, or produced by, the virus inserted, which, in the blood of the vaccinated person, increases, and, incorporating itself in the vesicle, reaches its highest development and greatest inoculating power on the eighth day, and then degenerates. The vaccine might, in most essential points, be regarded as a type of morbid poisons, *i. e.*, of such as are the products of disease. Whether inserted in the blood by inoculation or bred therein, they commonly occupy definite periods of time in their development, and increase, and decline; as with a life which is chronometric in all its phases and in its total length. The instances of morbid poisons would supply examples of organic processes timed to various numbers of days; and many that are completed in a day, or in given portions of a day, are traceable in the events of sleep and waking in animals (and, perhaps, also in plants), in the daily variations of the pulse, and of breathing, the returns of hunger and thirst, the regulated times of the digestive functions, &c. In man, indeed, consciousness and will are so concerned in some of these functions, that they may seem to lack that regularity which belongs to merely organic processes; but, if studied generally, and in other species as well as man, they all tell of such processes accomplished with regular measurement of time, and not determined by the external events or conditions of the day or night. Thus, for sleeping and waking, and the times of hunger and thirst, man's independence in regard to day and night, or light and darkness, and the habits of different species whose times of activity are severally, in the early or later day, in twilight or at night, may prove that the earth's diurnal changes are not the causes of these diurnal peculiarities of animal life. The very cause of sleep, and of that which is yet more mysterious, waking, may be unknown; but they are evidently connected and correlated with those alternating conditions of the structures, of which men, and probably all animals that sleep and wake, are conscious in the sensations of fatigue and of refreshment. The ordinary activities of one portion of the twenty-four hours, the activities, especially, of the muscles and nervous centres and the senses, produce an amount of structural, or chemical, change which is exactly repaired in rest during sleep. In other words, the organic processes for the repair of structures changed (as all structures are) by exercise, are adjusted to such a rate, that in general, and on an average, in the time of sleep, they may completely restore the parts that are impaired in the activity of waking time. And so, of that replacement of substances in the several structures and in the blood, which is the purpose of feeding; the processes of digestion and of the several stages of assimilation are so timed, as to accord exactly with the times of daily taking. The most minute observances of time in organic processes might be noted in organs that have rhythmic motions, as in hearts and breathing muscles, ciliæ, the vacuoles of certain zoospores, as Volvox and Gonium. In the Croonian Lecture at the Royal Society, in 1857, the speaker had endeavoured to prove that these and other rhythmic movements in plants, as well as animals, are due to corresponding time-regulated nutrition. He had expressed his belief that "rhythmic motion is an issue of rhythmic nutrition, *i. e.*, of a method of nutrition, in which the acting parts are, at certain periods, raised, with time-regulated progress, to a state of instability of composition, from which they then decline, and in their decline may change their shape and move with a definite velocity, or (as nervous centres) may discharge nerve-force." And this would be still maintained; but whether it were true or not, the rhythmical nutrition of rhythmically-acting muscles would be certain. If not a cause, it must be a consequence of such acting; for it is inconceivable that the heart (for example) or the diaphragm, or any other rhythmic muscle, should be free from waste or impairment in its action, or from the necessity of being renovated in its rest. Difference of mode of action could not determine a difference in the immediate effect of action. With long exercise, muscles become so changed that their changed state can be felt in the sensation of weariness, and proved by chemical analysis. But the change thus proved is only the accumulation of the changes wrought in many muscular actions, each of which has contributed a share to the whole amount, just as each revolution of a wheel contributes to the final wearing out. Similarly, every action of the heart, or of the breathing muscles, is attended with change or impairment of composition; but the impairment is repaired in the next following period of rest or relaxation. In other words, the alternating actions in shortening, and rests in lengthening, of the muscular fibres are correlative and synchronous with their alternating impairments and repairs of composition. The chronometry of such organic processes seems perfect; nutrition is in them divided, as it were, into units; and for each unit, there might be reckoned a unit of time.

Two results of this constant maintenance of rhythmic muscles are remarkable; viz., the enormous power they are capable of exerting, and their freedom from fatigue when only naturally acting. The latter result is proved to depend on the constant maintenance of the muscles, in their timely intervals of rest, by the weariness which is produced in the same muscles when they act otherwise than rythmically, as in the muscles of respiration when employed in any voluntary movements, or in coughing, or other violent respiratory acts. The instances adduced thus far might supply examples of organic processes adjusted to periods of time varying from the length of human life to less than a second. They were all examples of large classes of facts, from which might be filled up the instances of observance of other and very diverse periods of time; and in all of them, the time-rate is essentially determined, not by external conditions (though these may, in some measure, modify it) but by the inherent properties of the organic bodies themselves. In another large group of instances, those, namely, in which vital processes are completed, or attain some climax, in a year or in a set portion or season of a year, an independence of external conditions appears less evident. The higher organisms, chiefly by reason of their having in themselves the power of generating heat, may manifest their own time-laws with comparatively little disturbance from without. But in the vegetable world, and in the lower animals, the organic processes are, for the most part, suspended during part of the year, for want chiefly, of the heat which is a necessary condition of their activity, and the variations of which, for the rest of the year, very greatly affect their rate. Yet even in these, there appear sufficient indications that the times in which the processes of organic life are accomplished depend, essentially, on the specific properties of the several organisms themselves. Thus, under the same external conditions, each species observes a proper rate of its own. All the plants, for example, of a given locality are subject to the same temperature, and other seasonal conditions; but their rates of living, like those of various eggs placed in the same heat, are different; each reaches the chief events of its life at a certain period of the year. Variations of the seasons may affect all of them; but their method of succession is not thereby changed; they observe the same proportions in the times severally required for their organic processes; and this unaltering proportion indicates a time-rate specific for each, though equally variable in all. Moreover, among plants, there are numerous examples of varieties, which differ from the general character of their species only, or chiefly, in regard to the times at which their vital processes are accomplished. Such are the variations that are known as "late," and "early," among flowers or fruits; of which some may be propagated by seeds. [Specimens were shown from two horse-chestnuts growing opposite to one another by the great gate of the Kew Gardens, of which one is, every year, three weeks earlier than the other, in all the processes of its life; and of varieties of Erythronium, Dens Canis, from the same gardens, the plants of which, growing side by side in the same bed, always present a similar difference in their times of flowering, &c., though in all other respects alike.] It would be difficult to imagine a variety thus marked only by a peculiarity in rate of living, if temperature, or the influence of the seasons, alone determined the rate of life in the species. The simplest explanation seemed to be that, as there may be varieties in size and number of organs, and almost all the other properties of a species, which together make up its specific character, so there may also be varieties in regard to that time-rate of the processes of organic life which, even by this variability, is indicated as essentially dependent on the properties of the organism itself. Again, there are some species in which there seems to exist a singular independence of external conditions. Instances of this are found in the Eriogastor lanestris, and the other moths mentioned by Kirby and Spence. If pupæ, formed in June or July, be "selected of the same size, and exposed to the same temperature, the greater number of them will disclose the perfect insect in the February following; some not till the February of the year ensuing, and the remainder not before the same month in the third year," (Vol. iii. p. 264.) The design of so singular an arrangement is, as they observe, to secure that insects, coming into active life in February or March, may not be utterly exterminated by the ungenial weather of a single season, or of two such seasons in succession; but the very cause of the differences among the pupæ, in their relations to the same external conditions, must be in their own properties. A somewhat similar instance of apparent complete likeness among seeds in all respects except that of time, is in those of a Begonia, which, if taken from the same pod, and all planted together, and all kept in the same conditions, will germinate, some in a day, some at the end of a year, and some at various intermediate times. To these indications of self-dependent time-rates in the lower organisms, might be added all the facts of another class, which show punctuality in the adjustment of several distinct processes. Scarcely an event of life could be watched which would not show it. [The instance by which it was illustrated was that of a Saxifraga, whose stamens, like those of Parnassia, arrive at their very maturity, not all together, but in pairs, and in pairs bend upon the pistil, each pair rising again before another pair bends down.] And, lastly, the influence of temperature on the rate of the formative processes in the lower organisms is scarcely, or not always, greater than that of nutriment and other external conditions, is on their quantity. The occurrence of "good" and "bad" seasons indicates the latter influence, as that of "early" and "late" seasons does the former. Plants of the same species growing, some in an arid, others in a rich soil, differ exceedingly in size; the one are stunted, the others exuberant; here nutriment modifies the quantity of formation, as, in other instances, varieties of heat will modify its rate. But this being so, it may be held that as a certain average size or quantity of growth is a characteristic of each species, and an issue of its very nature, so is a certain average time or rate of growth. Quantity and rate may alike be varied by external circumstances, but the standard or medium of both, as well as the limits of variation compatible with life, are determined by the natural and inherent properties of the species. Whatever evidence these and the like facts might supply, that, in connexion with the seasons, the time-rates of the organic processes in the lower organisms are essentially dependent on the inherent properties of each organism, similar evidence might be adduced for the case of the higher, and especially the warm-blooded animals. In these the varieties of seasons have less influence in modifying the rate, as well as all the other measures, of life; and the less influence, the higher the species, or the degree of development of the individual. Moreover, there are in birds some instances in which organic processes have a tendency to observe certain times of the year even when the seasons are changed. Thus among those brought from Australia to this country, some of the parakeets breed here in December; the black swan sometimes breeds in November as well as in May; the New Holland Cereopsis-goose has bred at the Zoological Gardens every February for five or six years. Among migratory birds, also, it has been observed that when they are kept in confinement, and removed from all the circumstances that might be supposed to induce or necessitate their journeys, they yet become restless at the return of the season for their migration. In these and the like facts there appear indications of a chronometry in the organic processes of warm-blooded animals, which corresponds with that of the seasons, but is essentially independent. And, if it be so, these might form a group of facts, in addition to those of the diurnal variations of the organic processes, in which vital changes are set to the same rules of time as changes of the surface of the earth, yet have their own proper laws; and concerning which it might be said, that the cycles of life, and of the earth, do indeed, correspond, but only as concentric circles do, which are drawn round one centre, but are not connected, except in design and mutual fitness. But, however this might be, all the instances of time-regulation cited in the discourse (all being examples of large groups of facts), would seem sufficient to prove that the observance of time in organic processes is as exact and as universal as that of any other measure; that each species has a certain time-rate for the processes of its life, variable, but not determined, by external conditions; and that the several phenomena commonly studied as the periodicities of organic life, are only prominent instances of the law which it was the object of the discourse to illustrate.—*Med. Times and Gazette, June* 11, 1859, *p.* 593.

154.—OBSERVATIONS ON THE MEDICINAL ADMINISTRATION OF OZONIZED OILS.

By Dr. THEOPHILUS THOMPSON, F.R.S., Physician to the Hospital for Consumption, &c.

[In this paper (which was read before the Royal Medico-Chirurgical Society) the author, after some general remarks on the properties of ozone, describes the results obtained from its administration in association with oils:]

The oils being ozonized by exposure for a considerable time to the direct rays of the sun, after previous saturation with oxygen gas, according to the process adopted by Mr. Dugald Campbell. The cases of fourteen consumptive patients to whom the ozonized oils were given are detailed; and the principal facts noted are also appended in a tabular form. The conclusion to which these experiments point is that the administration of ozonized oils has a remarkable tendency to reduce the frequency of the pulse. Of the fourteen patients whose cases are detailed in this communication, there are only two in whom no such effect was observed; and although in a few instances the effect may have seemed insignificant or transient, in the larger proportion it was very considerable, and must be attributed to the ozone rather than to the oil, since it was repeatedly manifested in patients who had taken cod-liver and other oils without any reduction, or even with an acceleration, of the pulse; and further, the effect on the pulse was nearly as distinct when the ozone was associated with the oil of the cocoa-nut, or of the sunflower, as with that of the cod liver. This circumstance is the more significant, since the administration of sunflower oil without ozone has not appeared to the author to manifest any important remedial power. The reduction of pulse was usually observed in two or three days, and often continued progressive. A reduction of twenty beats was observed in certain cases to occur respectively in two, three, four, and six days; in other instances a reduction was noted of twenty-four pulsations in fourteen days, thirty-four in thirteen, thirty-six in twenty-two, forty in eleven. In one patient the pulse fell as low as sixty—probably considerably below the natural standard; but in most of the favorable instances the reduction stopped when that standard was obtained.

The apparent effect of the remedy is one which, prior to experiment, the author would not have anticipated. No other obvious result was noticed, excepting a general improvement in the patient's condition. In some of the patients the use of simple and ozonized oils was alternated. In one case the alternation was made three times, and the result was in each interchange of treatment so direct and remarkable as to make that particular example equivalent in force to three experiments.

In addition to the patients under his own observation, the author refers to four instances noted by Dr. Scott Alison, who obligingly pursued the investigation during Dr. Thompson's absence from the hospital. In these four cases the disease was in the third stage. In two, a remarkable reduction in the rapidity of the pulse, amounting to about twenty beats, occurred under the use of the ozonized oil, while the improvement induced could not be referred to any other cause. Dr. Alison remarks, "I attach some value to this observation; for I prescribed the oil totally divested of all prejudice in its favour, and I have always been reluctant on imperfect grounds to refer results to the operation of medicines. If ozonized oil can reduce the rapidity of the circulation—a feature of great prominence in phthisis,—this remedy possesses a most valuable property, rendered still more valuable by its contributing at the same time to improve the general health."

The author mentions having used ozonized oil of turpentine with marked and prompt advantage in some cases of hæmoptysis, but has not sufficiently repeated the experiment to feel entitled to express an opinion as to its remedial superiority over ordinary turpentine. He adds that, should more extended observation establish for ozonized oil the property indicated by these experiments, it will prove a valuable addition to our list of remedies, especially in consumption (which is a disease peculiarly characterized by hurried action); but not, perhaps, exclusively in this disorder, since there are other morbid conditions in the treatment of which it is very important to lower the pulse without reducing constitutional strength.—*Lancet, July* 9, 1859, *p.* 35.

155.—ON THE USE OF SACCHARATED LIME IN MEDICINE.

By Dr. John Cleland, Demonstrator of Anatomy in the University of Edinburgh.

[The solubility of lime in water is much increased by the presence of sugar, a direct chemical combination taking place. Ordinary lime water being far too weak a preparation to develope to advantage the therapeutic properties of lime, advantage may be taken of the above property to procure a solution sufficiently strong to be at once valuable as a tonic and antacid.]

Sugar combines in two or more proportions with lime, and on this subject I shall quote from M. Regnault (Cours Elémentaire de Chimie, vol. iv., p. 138.) "Two combinations may be obtained of cane-sugar with lime. The first is produced by pouring a solution of sugar on an excess of slaked lime, when a combination is formed very soluble while cold, and which is separated by filtration. If the fluid be heated to ebullition, the greater part of the combination is precipitated, for it presents the remarkable property of being much less soluble when heated than in the cold. It may even be washed in boiling water, and then redissolved in cold. This saccharate of lime, dried at 212°, has the formula $3CaO.2(C^{12}H^{11}O^{11})$. If, on the contrary, hydrated lime be added in small quantities to a concentrated solution of cane-sugar, until the last added portion refuses to dissolve, and alcohol be then poured into the liquor at 180 Fahr., a saccharate of lime is precipitated which has the formula $CaO.C^{12}H^{11}O^{11}$. The solutions of saccharate of lime have a strongly alkaline reaction; they attract carbonic acid rapidly from the air, and crystals of carbonate of lime, exactly similar to native crystals of that substance, are deposited on the walls of vessels containing them. Solutions of cane-sugar can, besides, dissolve very various quantities of lime, according to their concentration and temperature. On ebullition, they deposit strongly basic saccharates which contain from 3 to 4 equivalents of base."

For therapeutic purposes the first described compound is best suited, for it is the most soluble, and obviously it is advantageous to have as small a proportion of sugar as possible. Instead of pouring the sugar in the form of syrup upon the lime, I find it more convenient to mix the slaked lime and the sugar, and then add the water. The following is a very good formula: Slake 8 ounces of quicklime; rub up with it five ounces of white sugar; add 1 pint of water; stir for some time, till the hard stiff masses which the sugar and lime are liable to run into are as much as possible dissolved; then filter. The product should be perfectly clear, and of only a slightly yellowish tint. A solution made in this way will contain 18 grains of lime in every ounce by weight, and altogether about 106 grains of solid matter to the ounce. Taken undiluted, a few drops are sufficient to roughen the tongue. When diluted, the taste is at first an acrid one of lime; but this is immediately replaced by a sweet taste in the back of the mouth, admitted to be pleasant. Made as just recommended, the solution is not liable to decomposition unless it is exposed to the air. By employing a smaller proportion of water to lime, a still stronger solution may be obtained, but not with any practical advantage, as there is increased difficulty of filtration and greater tendency to decomposition. The strongest solutions are scarcely, if at all, affected by boiling; but if diluted, a copious precipitation takes place on application of heat. This, however, will not serve as a test of strength, as addition of sugar in sufficient quantity will make any solution, of whatever strength, remain clear on ebullition.

My first trials of the medicinal effects of saccharated lime were made in the winter 1856–7, with solutions made by myself and by Mr. Stewart, druggist, Inverleith Row. After I had quite satisfied myself of its value, in spring 1858, I gave directions for making it to Messrs. Duncan, Flockart, and Co., and recommended it to the attention of various members of the medical profession in Edinburgh.

I shall not enter largely into the therapeutic effects of this preparation, but only give a cursory indication of them, basing as I do my title to speak on the subject simply on my being the first to introduce it as a remedy. It is of course a powerful antacid, and probably the best we have, since it is stronger and pleasanter than magnesia, and does not weaken digestion like the alkalies. Far from doing so, its most important use is as a tonic of the alimentary system in cases of obstinate dyspepsia. As such, its action is much more powerful than that of the vegetable stomachic tonics. It is suitable for cases with too little as well as for those with too great secretion of gastric juice, no doubt because the former state of matters is obviously a result of atony, which the lime removes. It seems particularly serviceable in gouty constitutions. In the dyspepsia of hysterical and anemic cases it does not seem to be of great use. Care should be taken to tell the patient not to take it before breakfast, as it sometimes causes a degree of nausea in the morning, when the stomach is empty. It suits very well to take it immediately after meals; its alkalinity does not at all interfere with digestion. Practitioners seem generally to take up the prejudice beforehand, that saccharated lime must be liable to produce constipation, probably judging so from the action of chalk; but I wish particularly to insist that it has not, in the slightest degree, any tendency to occasion such an effect. On the contrary, it is a very valuable means of overcoming gradually that chronic constipation which is so frequent an accompaniment of dyspepsia; and persons who have for years been in the constant habit of using aperient medicines have been able to abandon them in great measure after taking this remedy for some time. In a single instance it acted as a purgative, so that its use could not be continued. It will be found serviceable in checking the diarrhœa of disordered digestion, acting as lime water does, only that the latter is so dilute that it is often impossible to administer it to adults in the quantity desirable. Patients who take saccharated lime habitually get to like the taste, and seem to think it exhilarating. It may be found useful also in allaying the cravings of the intemperate. I have no doubt that, if it be fairly tried, practitioners will find it an exceedingly useful remedy. It may be given in doses of from 20 or 30 to 60 minims or more, in a glass of water, two or three times a-day.

I shall venture, in conclusion, to suggest that the large and various set of remedies embraced in the general name of tonics tend somewhat to group themselves in three clusters, corresponding to the three great systems developed from the three layers of the embryo, viz., tonics acting primarily on the musculo-nervous system, such as quinine and strychnine; those acting primarily on the vascular system, such as iron and manganese; and, lastly, those acting primarily on the alimentary system, among which lime is most prominent, and, after it, such vegetable tonics as gentian, calumba, quassia, &c.—*Edinburgh Medical Journal, Aug.* 1859, *p.* 114.

156.—OF THE CHLORATE OF POTASH.

By Dr. Fountain, New York.

Dr. Fountain states that he became acquainted with the remarkable power exerted by this highly oxygenated salt in mercurial salivation in 1857, and consequently long before this had been observed by Herpin and others. In charge of the Hospital of the Panama Railroad Company, at Aspinwall, he often met with ptyalism in consequence of the free use of mercury, the peculiar type of disease met with on the isthmus rendering it necessary. Finding the various local applications of little avail, and recollecting Dr. West's recommendation of the chlorate in stomatitis of children, he gave in a bad case twenty grains every three hours, besides using it also as a wash.

The effect was almost magical, and subsequent experience has only confirmed its efficacy. And Dr. Adler, engaged in the same service, says: "Such has been the uniform success following the administration of this salt, that from my own experience I consider it as a specific. It has never failed." In some cases where mercury had to be given for a long time, and danger of ptyalism was feared, it has been given as a prophylactic, and, as Dr. Fountain believes, with good effect. Dr. Fountain also gives an account of the advantage he derived from the use of the chlorate, in Iowa, when, owing to a deficiency in the supply of vegetables, and poor persons living on salted meat, and the cheapest food, a mild form of land scurvy broke out —the manifestation of the disease being confined to its local effect on the gums, without the production of constitutional symptoms. Recovery rapidly took place under its use, even when the diet remained unchanged. In ulcerative stomatitis he has never known the salt to fail.

The author's chief object, however, is not to add illustrations to facts already known, but to call attention to a practical application of the chlorate not yet much noticed. It is as an *oxydising remedy*, capable of coming to the aid of the respiratory process; and of stimulating secondarily the function of absorption, that its agency acquires a vast importance. This was first exemplified to him in a case of cyanosis from heart-disease, in which the blue colour was remarkably removed during the employment of the chlorate; and two cases are also given, one in which respiration was impeded from effusion of blood into the thorax from a wound of an intercostal artery and the other an example of excessive dyspnœa from hydrothorax—in both of which the recovery seemed to be due to the use of the chlorate, which "supplied the blood with oxygen sufficient to sustain life during the time when respiration was seriously impeded." In a case of pneumonia typhoides, in which the imperfect aeration of the blood was the combined effect of an asthenic fever and a local engorgement of the lungs, the same benefit followed its administration. "The chlorate may answer another and valuable purpose in *promoting absorption*. It readily parts with its oxygen by the influence of a moderate degree of heat, or a weak acid; both of which exist in the body in sufficient amount to effect the decomposition to a greater or less extent. This is the theory; and practical confirmation has been made by the cases herewith reported. If by increasing the amount of oxygen in the blood, we enhance its power of removing deposits, then do we have in the chlorate another property of extensive application and great practical utility. It would render it doubly applicable to all cases where respiration is impeded by effusion within the chest of liquids or solids requiring decomposition for absorption, such as hæmatothorax, empyema, and the hepatised stage of pneumonia. As most of the products of inflammation consist of protein compounds in some form or combination, the addition of one or two equivalents of oxygen render them soluble by conversion into the deutoxide or tritoxide, and permits their absorption by endosmosis into the adjoining vessels. Even tubercular deposits are known to consist principally of protein compounds; and we all know that the most effectual means of retarding or preventing their development consists in active out-of-door exercise in the pure air of the country, by which the system is more freely supplied with oxygen, and the effete products of interstitial decay more rapidly removed."

"The chlorate of potash has been quite generally used as one of the favourite remedies in *scarlatina*. I think that it is justly so regarded, and has fallen into some disrepute of late by the error of depending too much upon its unaided influence. It has generally been given with the idea, that in chlorine, something like a specific or antidote had been found for the poison of the disease. Now, in truth, the chlorate is a very valuable remedy for meeting particular indications in the treatment of this disease; by arresting the ulcerative inflammation of the fauces, and by its arterialising properties, supporting the recuperative powers of nature when aided by other appropriate treatment. In this connexion I will state, that I have been in the practice, of late, of giving it in combination with carbonate of ammonia, with the best effects. Other aids must vary according to circumstances; but I will mention two which I have found of great value in almost all cases—the application of pure nitrate of silver to the fauces, and of glycerine to the whole surface of the body. Some considerations induce me to think that it is not the chlorine, but the liberated oxygen, which constitutes the chlorate a remedy of value in scarlatina and kindred diseases."—*New York Journal, July, pp.* 1-27.—*Med. Times and Gazette, Aug.* 27, 1859, *p.* 219.

157.—*Chlorate of Potash.* By WEEDON COOKE, Esq., Surgeon to the Royal Free Hospital.—In my hands chlorate of potash has proved a tonic of the very highest value in all adynamic conditions, and at all ages; but more especially in hectic states of the system when quinine and iron were inadmissible. So powerful is it in oxidizing or decarbonizing the blood, when the liver and skin have failed in their offices, that I believe, from a very large experience of its effects at the Royal Free Hospital, there is no tonic comparable to it in the sequelæ of the exanthemata (otorrhœa, anasarca, and cachexia of all kinds), in all scrofulous diseases, whether of bone, gland, or tissue, as well as in those indicated by Dr. Osborn—viz., necrosis, leucorrhœa, gleet, and secondary syphilis. Agreeing with Dr. Osborn in the inexpediency of its administration in all acute inflammatory attacks, I think that, in reviewing those numerous diseases in which there is want of power, I know of none in which the chlorate of potash, either solely or in combination with iron, quinine, or the other vegetable tonics, may not be advantageously employed.

I believe the dose of this medicine is now pretty accurately defined. From one to fifteen grains three times a day is the range I have found effective and safe. I give the child of one year old one grain; of two years old, two grains, and so on up to seven years; after that, a more gradual increase—say, a grain every two years, until about eighteen years, when the full dose of fifteen grains may be employed; and this dose rarely requires to be increased at any age.

Having said thus much of the value of chlorate of potash administered internally, I would beg permission to add a word or two on its remarkable topical effects in various ulcers. It is invaluable in foul chronic ulcers of the legs; in tertiary sores not of an inflammatory character; in ulcers of the mouth and tongue, arising either from syphilis or cancer, or cancrum oris, or necrosis of the jaws; and especially so in cleansing and deodorizing, and indeed healing, many of the foul cancerous ulcers occurring in various parts of the body. I have employed it in all these lesions at the Cancer and the Royal Free Hospitals, as well as in private practice, for many years, and am daily reminded of its inestimable benefit wherever there is an absence of active inflammation.

The strength of the solution must of necessity vary under different circumstances. In the greater number of instances, eight grains to the ounce of water is the strength I employ, but this is too strong for the mouth, and generally for the breast.—*Lancet, Oct.* 29, 1859, *p.* 449.

158.—*On the Use of Solid Perchloride of Iron.* By J. ZACHARIAH LAWRENCE, Esq.—A few months ago I drew the attention of the profession to the powerful local styptic properties of the *solid* perchloride of iron. I have since that time used it with the best effects, and, moreover, have the satisfaction of knowing, through the manufacturers of this substance (Messrs. Hopkins and Williams, of New Cavendish-street), that since the appearance of my letter it has been in frequent demand by the profession generally for the purposes indicated.

Since then I have found a superior method of employing it. If the solid perchloride of iron be kept in a bottle, a small portion of it, after a time, deliquesces into a thick, brown fluid, which is constantly kept in a state of supersaturation by the undeliquesced portions of the salt. This liquid, applied by means of a spun-glass brush to a bleeding surface, arrests the bleeding almost instantaneously. This mode of application is particularly valuable in applying the styptic to such cases as excision of the tonsils, bleeding from the deeper-seated gums, &c. I may further remark, that I have never noticed any inflammatory action following the use of the solid perchloride. —*Med. Times and Gazette, Aug.* 27, 1859, *p.* 219.

159.—THE COAL-TAR DISINFECTING POWDER OF MM. CORNE AND DEMEAUX.

The latest Parisian novelty in medicine—very appropriate to the fetid season of summer in towns, and particularly interesting to Londoners—concerns the disinfected properties of common coal-tar. The subject, brought forward by MM. Corne and Demeaux, appeas to be creating quite a sensation in the French capital. It has been laid before, and been discussed at the Academy of Sciences, and the Academy of Medicine, and has been experimented on at La Charité and at Alfort; and all the French journals are full of it. The history of the matter is sufficiently simple. M. Edmond Corne, physician at Libos (department of the Lot and Garonne), recently found out that a mixture of coal-tar and plaster in various proportions, when put in contact with fetid and putrifying substances, possessed the valuable property of deodorising them completely; and, at the same time, if the matters contained fluid, it absorbed the liquid, and converted the whole into a dry, solid mass, free of smell. Perceiving the importance of these facts in a sanitary point of view, M. Corne proposed to employ his disinfecting powder for the purpose of deodorising and solidifying the fæcal matters, which, in Paris instead of being drained off to pollute the Seine, are allowed to collect in covered pits, and are carted away in closed waggons early in the morning. The accumulation of the night soil, and the process of its removal, are attended with the horrible stenches which are so common in many of the houses and streets of Paris. Sanitary science, however, does not seem attractive to Frenchmen; and the discoveries of M. Corne might have remained long enough unnoticed, if the happy thought had not occurred to Dr. Demeaux and himself of applying his disinfecting powder to surgical uses, for the purpose of deodorising the offensive discharges from suppurating wounds and gangrenous ulcers. The first trial was made in the private practice of Dr. Demeaux, and succeeded beyond expectation. The authors then laid the subject, in this new point of view, before the scientific and medical institutions of Paris, and commenced a series of experiments at the Hospital of La Charité, under Professor Velpeau, who has afforded every encouragement, and has lent his powerful aid to bring the discovery into notice. The general results have as yet been very favourable; and from the great attention which is turned to the subject, the value of the proposed means will soon be satisfactorily settled.

The disinfecting powder consists simply of 100 parts of plaster mixed with 1 to 5 parts of coal tar, and thoroughly triturated. The mixture forms a gray powder, with a slight bituminous odour. It is applied directly to the suppurating surfaces. Usually the dry powder is sprinkled over the wounds; or it may be made up into a kind of ointment, by mixing it with olive oil, which binds the powder together without destroying its absorbent power; or it may be mixed with other fatty substances; the ointment being spread thickly on lint, and applied like a poultice. Usually the direct application of the powder is well borne; and, in consequence of its absorbing properties, no lint is required. Sometimes, however, the charpie is necessary. The dressing is renewed as frequently as possible.

The results of this mode of treatment, as experimented at La Charité under Professor Velpeau, before his numerous students, appear to have been uniformly successful. A gangrenous wound, with a profuse and fetid suppuration, when dressed with the powder, was instantly deprived of all disagreeable smell. In the case of an ulcerated cancer, with an ichorous discharge, presenting the characteristic fetor, the odour was in the same way instantly and completely destroyed, when the dressing was applied. Ulcers of the leg were entirely freed from smell in the same manner. Dressings, poultices, &c., saturated with fetid pus, were completely disinfected by contact with the powder; decomposing fluids, gangrenous products, mortified tissues, pieces of dead bodies in an advanced stage of putrefaction, were also instantly deodorised by the same means. The disinfecting substance seems to stop decomposition; and it keeps off insects, and prevents the production of maggots. The experiments at the Veterinary School of Alfort, under Professor Bouley, made on a large number of wounds and putrid matters, gave results entirely in accordance with those obtained by Professor Velpeau; so that, as far as facts have yet shown, the means promises to be of decided service in surgery.

It appears that the advantages possessed by the disinfecting powder as a surgical dressing are twofold: for it not only disinfects the discharges, but it at the same time absorbs them,—the former property being probably due to the tar, and the latter to the plaster. It is obvious that the powder may be applied to a great variety of uses besides surgical dressings, and in particular for preventing the smell and putrefaction in dissecting-rooms. One other advantage which the disinfectant presents is its cheapness; the ingredients may be had for almost nothing, and at the present time the powder of Corne and Demeaux sells for about a shilling the hundred-weight in Paris.

Of the importance of disinfectants there can be no doubt: they are the great desiderata of the day. It should not be forgotten, however, that the invention of MM. Corne and Demeaux, and its applications, have been to a great extent, anticipated in England. It is mentioned by M. Dumas, in the discussion at the Academy of Sciences, that coal-tar has been used in England for disinfecting animal matters, and has been even proposed to prevent decomposition of the dead bodies on the field of battle. But no notice is taken of M'Dougall's disinfecting powder, which has been known and used in this country for some time, and which presents a close resemblance in composition and properties with the powder of M. Corne. M'Dougall's powder consists of carbolic sulphite of lime and magnesia, with 5 per cent. of carbolate of lime; carbolic acid being the principal antiseptic and deodorising ingredient in coal-tar. It appears, therefore, that the powder of the French physicians owes its virtues to the same essential constituents which are present in M'Dougall's powder, viz., carbolic acid and lime. Professor Simpson has been in the habit of using M'Dougall's disinfecting powder as a deodorizing application in cancer of the uterus; he has employed it also for preserving dead bodies; and he has suggested that the embalming of mummies may be due to carbolic acid, and not to creosote, as Dr. Cormack has supposed. M'Dougall's powder was also proposed last summer by Professor Anderson of Glasgow for purifying the Clyde. It should be mentioned, that we owe the discovery of the powder known as M'Dougall's to Sir Andrew Smith, the late Director-General of the Medical Department of the Army. But while the credit which belongs to previous inventions must not be overlooked, it is well that the attention of French scientific men has been so prominently attracted to the subject. It is of immense importance to sanitary science, as well as to surgery, that the disinfectant properties of the products of coal-tar should be fully investigated. The efficiency of carbolic acid and its compounds seems to exceed that of any disinfectants yet discovered.—*Edin. Med. Journal, Sep.* 1859, *p.* 263.

160.—REPORTS ON COUP-DE-SOLIEL IN H. M.'s 71st REGIMENT (RIGHT WING) IN CENTRAL INDIA, 1858.

By Dr. W. Simpson, Surgeon to H. M.'s 71st Regt.

[The average strength of the wing during the months of May, June, and July, 1858, was 417 non-commissioned officers, rank and file; 89 cases of sun-stroke occurred, of which 13 died suddenly, 13 afterwards in hospital. This was attributable to long marches performed on consecutive nights.]

The following symptoms were invariably present: an intensely hot, dry skin, which lasted till even after death in those that died within 12 or 16 hours; a sense of constriction of the chest and laboured breathing, with a feeling of a heavy weight just below the ensiform cartilage. Great prostration of strength, accompanied very frequently with inability to answer questions without weeping (the strongest and most robust were not exempt from these symptoms); a tumultuous action of the heart, with strong pulsation of the carotids; pulse varied much, but was never full and hard; headache referred more particularly to the summit of the head, conjunctivæ injected, pupils acted to the stimulus of light, unless during convulsions, or during the stage of coma, when they were fixed and contracted. In several instances, however, they became suddenly dilated for a few minutes, after being fixed and contracted to a point. Countenance generally pale at the commencement, but in those cases of a severe character, or that had a fatal termination, it assumed a leaden hue; the urine was never entirely suppressed, but it passed off involuntarily drop by drop; bowels were generally costive, though several cases occurred the bowels being quite natural in every respect. There was also a great desire to sleep, so much so that if not checked at first, it passed into coma, which almost invariably terminated in death. Loud moaning during the stage of coma was almost invariably present. Death either occurred from convulsions, most frequently of an epileptic character, or from coma. The symptoms, however, varied according to the severity of the attack, and the constitution or temperament of the patient. Death sometimes occurred almost instantaneously. One man just before arriving in camp at Banda, (the regiment did not arrive on the encamping ground this day, 12th May, till after noon, and the thermometer was 120° Fah. in the tents,) after a march of over 20 miles, fell down in the ranks, uttered a shriek, and expired in a few minutes. A man at Koonch, before the engagement with the rebels took place, fell asleep for a short time during a halt that occurred (the sun being fully two hours above the horizon), and on being roused up answered questions with difficulty, and he had lost the use of his limbs. Others again were seized with maniacal symptoms: a man at Calpee made a rush at the Hospital Beestie and tore the mussock from him, and on remonstrating with him I remarked he was quite deranged in his mind. After getting some water to drink (which he swallowed in a painfully ravenous manner), and having a quantity poured over his head and body from a height, he became quiet, and shed tears abundantly. Others again smiled and laughed (unnaturally) at one time, and at another became excited and alarmed, if spoken to, and any attempt at deglutition brought on convulsions. In short, some of the cases presented symptoms of apoplexy, some of epilepsy, mania, and hysteria. The disease under consideration, when of a severe character, more closely resembles apoplexy or epilepsy (which often merges into apoplexy) than any other disease I am acquainted with. In many of the cases that came under my care, genuine epileptic convulsions ensued, lasting from 5 to 10 minutes, with intervals of variable duration of perfect consciousness and rest. During the stage of coma, the pupils were fixed and contracted, and the conjunctivæ injected, and there was loud moaning till a few minutes previous to dissolution taking place, which last symptom, however, I have never heard in pure cases of apoplexy that have come under my observation.

Post-mortem Appearances.—Time and opportunity did not allow of *post-mortem* examinations being made to any extent on the line of march, during the late campaign, when fatal cases of sunstroke were of common occurrence, and those that were performed elucidated little or nothing regarding the nature of the disease; the only abnormal appearances were an excess of venous blood to a greater or less extent in the brain, and congestion of the lungs and liver. This want of balance of the circulation, taken in conjunction with the symptoms during life, induces me to believe that sunstroke is attributable to the functions of all the organs that free the blood from those matters that are injurious to the system being either entirely or partially suspended, such as the lungs, liver, kidneys, and skin. The blood is not perfectly oxygenized in the lungs, the bowels as a rule are confined, and the healthy action of the liver is interrupted (this last statement is, I think, proved by the fact that those men who had severe attacks of sunstroke have since suffered from derangement of the liver). The kidneys evidently do not perform properly their functions: the urine is not suppressed, but it is passed involuntarily drop by drop, and in diminished quantity. The skin is pungently hot, dry, and harsh (every man seized with sunstroke, and who could answer questions, informed me that he had not perspired for a greater or less extent of time, sometimes not for days, previous to being attacked, and that he had enjoyed good health as long as he perspired, but that on the perspiration being checked, he felt dull and listless, and unable to take much exertion without making a great effort). No doubt this state of the body is attributable to the nervous system being overstimulated by exposure to the sun, more particularly when the thermometer ranges from 110° to 120° Fah. (which was too often the case in the tents occupied by the men during the day in May, June, and July), together with extreme exhaustion from long-continued exertion and want of rest.

Treatment.—The chief point in the treatment, in my opinion, is to rouse the nervous energy, which is most effectually accomplished by pouring cold water from a height over the head and nape of neck, and dashing the same in the face and over the chest, and this should be persevered in as long as there is any tendency to sleep or to coma; at the same time the patient should be roused up by speaking to him, and if necessary, by shaking him. As soon as he can swallow, brandy or wine and ammonia should be given in liberal quantities, and frequently repeated; and calomel and croton oil administered, to act on the liver, and move the bowels. The most distressing symptoms a man labouring under this disease complains of are—a sense of suffocation, and a dead weight at the *scrobiculus cordis*. I have found that rubbing for some time turpentine over the chest and stomach affords the most effectual relief. These remedies, together with stimulant enemata and mustard cataplasms to the legs, are more likely to prove successful than any I am aware of. The after-treatment consists of nourishment and stimulants, such as arrowroot, beef-tea, wine or brandy and ammonia, together with cold applications to the head, blisters to nape of neck, and acting on the liver and bowels. Bleeding, either locally or generally, is inadmissible, more particularly if there is exhaustion from previous exertion. No patient can be considered out of danger till the skin becomes cool and moist.

I would beg to offer a few suggestions as to the best mode of preventing so fatal a disease. When, during a campaign in the hot season, Europeans are exposed to the sun, encountering much exertion, and at the same time are deprived of sleep during the coolest hours, whilst they have to sleep and partake of their meals during the hottest hours (thereby retarding digestion, and producing a tendency of blood to the head), cases of sunstroke no doubt must occur; still great improvements might be introduced as a means of prevention. The dress as at present constituted is defective in many respects: it is imperatively necessary to protect, not only the head from the rays of an Indian sun during the hot weather, but also the vertebral column and body generally; the dress as worn by the soldiers accomplishes neither of these ends. The forage-cap is not sufficiently thick, the crown of it rests on the top of the head, it affords no means of ventilation, and the cap-cover is little better than useless, as the men generally turn the loose curtain of it either inside or outside the cap, in order to (as they have often informed me) obtain more air, and enable them to feel cooler. The coat is made of a very thin material, and affords no protection either to the spine or body; and soldiers will not, generally speaking, wear flannel if they can avoid it. I would recommend that the head-dress consist of one piece, affording cover for the nape of the neck, and shade for the eyes; it should be made of strong, close basket-work, and well padded externally, and covered with the same material and colour as the present dress introduced by Lord Clyde (during the hottest months of the year, and when not in presence of an enemy, a white puggry might be worn over this cap to reflect the heat); it should be of sufficient height to allow of a vacant space between the top of the head and the crown of the cap; and, lastly, it should be furnished with means to afford ventilation. From personal experience, I am convinced that it is the best sort of head-dress, but it should be made sufficiently strong to permit of its being made use of as a support for the head when the owner of it is in the recumbent position, for during the halts that take place in the night on the line of march, as well as on many other occasions, men lie down and often fall asleep, their heads being lower than their bodies (and I am induced to think that this position of the body frequently induced head symptoms during the late campaign). If a cap of the kind I have attempted to describe is worn, an excellent pillow or support for the head is constantly at hand. I would recommend that the coat should be made of the same material and pattern as at present, but that it should consist of two layers, or have lining, with wadding between; it should be made very loose, not only at the neck, but everywhere, more particularly at the arms, and at the armpits. A coat of this description would afford a protection for the body from the sun, and it would not be felt too warm if made sufficiently loose to admit of ventilation.

From the difficulty (and frequently from the impossibility) that was experienced in the 71st Regiment during the late campaign in obtaining a sufficient supply of water for cooking purposes, and for drinking, in consequence of Bheesties deserting, and dilatory in performing their duties, I would recommend that every regiment or wing of a regiment in the field should be allowed a man (well-paid) to superintend the Bheestie department, and who would be held responsible that an ample supply of water was always at hand, on the line of march, in the field while engaged with an enemy, or in camp. Quarter masters and officers have too much to engage their attention, and are at the same time frequently too much exhausted, to ensure a sufficient supply of water being always at hand; and the want of this most necessary article is severely felt by the men, and too often produces fatal consequences.

No regiment or body of men should be marched during the hot season, unless camels are attached to it in sufficient numbers to convey, not only the tents, but also the cooking utensils, that the men may get under cover, and be provided with breakfast, as early in the day as possible: if carts are used, great delay is always experienced.—*Transactions of Bombay Med. Society*, 1857, 1858, *p.* 246.

161.—ON ETHER AND CHLOROFORM.

By Dr. G. Hayward, late Professor of Surgery in the Massachusetts Medical College, Boston, U.S.A.

[The profession is scarcely prepared to receive without qualification the opinion that ether should be substituted for chloroform in all cases, to the entire exclusion of the latter. But the author observes that in no instance has there been any alarming or serious symptoms from the use of sulphuric ether, even when used to a great extent, undiluted with air. It does not produce anæsthesia quite so speedily as chloroform.]

There is no doubt in my mind that sulphuric ether should be used as an anæsthetic agent to the entire exclusion of chloroform. It is as efficacious, and I should say without hesitation, after having seen chloroform administered by others in many cases, that ether produces a more complete state of unconscious insensibility. Its effects pass off sooner, and less vomiting, nausea, and headache follow its inhalation. It is as easily administered. All that is required for its administration is a bell-shaped sponge with a cavity large enough to cover the nose and mouth. If the patient breathes it gradually, little or no irritation is produced in the larynx and air-passages, there is but little if any cough or sense of suffocation, nor a distressing or unpleasant symptom of any kind.

There may be some persons to whom the odour of ether is offensive and irritating, but they are comparatively few, and even they can be brought under its influence without any very great annoyance.

The quantity of sulphuric ether required to produce anæsthesia depends very much on the manner in which it is administered. If the patient is made to inhale it rapidly, and the atmospheric air is to a great extent excluded, a small amount will be sufficient. From four to eight ounces may be regarded as the average quantity. It is rare to meet with a case in which less than four ounces will be used; and in protracted operations, in which it is desirable to keep up the state of insensibility for a length of time, I have often given more than eight ounces. The ether should at first be poured on the concave part of the sponge; one or two ounces will be enough for this purpose. When the inhalation is going on, it is better to pour the ether on the outside of the sponge, so as to avoid the necessity of removing it from the face. From half an ounce to an ounce should be used at a time in this way, till anæsthesia is produced. When this takes place, the patient is wholly unconscious, and has no control over the voluntary muscles. He is unable to raise his eyelids when told to do so, and gives no indication of hearing or consciousness, if spoken to in a loud tone. The pulse usually becomes slower than the ordinary standard, though at the beginning of the inhalation it is quicker.

It is, I am confident, a perfectly safe anæsthetic agent. I have not been able to find any well-attested case of death from its inhalation. There may have been such, but they have never come to my knowledge, though I have taken unwearied pains to obtain information on this point.

It has been said, that this may be attributed to the fact that ether is not extensively used, but that if it were, there would probably have been as many fatal cases in proportion from it, as from the inhalation of chloroform. But this statement is not strictly correct; for though ether is not employed as an anæsthetic agent to any extent, if at all, in Great Britain or many parts of Europe, it is used in Lyons, Naples, and is almost the only one that is administered in the principal hospitals of the United States of America, where its now familiar properties were first discovered.

I have given it in several hundred cases, and witnessed its exhibition by others in as many more. I have administered it to infants not three weeks old, and to persons more than threescore years and ten, and have never in a single instance seen an alarming or distressing effect produced by it. On the first introduction of ether into surgical practice, it was not thought safe to allow persons to inhale it in whom there was reason to believe there was any disease of the heart or lungs, or who had any tendency to an affection of the brain and nervous system. But for some years past I have been in the habit of administering it to individuals of this description, and have as yet had no cause to regret it. In such cases I have thought it prudent to have the vapour of the ether inhaled more slowly, so that it may be more diluted with atmospheric air than under ordinary circumstances; of course, the patient could not be brought as soon under its influence as when taken in the ordinary way.

The state of the system which is produced by the inhalation of ether is that of narcotism, similar precisely to what is induced by drinking immoderately wine or other alcoholic liquors. It is a state of intoxication more transient and less dangerous than that from alcohol. Its effects pass off sooner, because the vapour of the ether begins to escape from the lungs as soon as the patient ceases to inhale it; while alcohol taken into the stomach is carried into the circulation, and mixes with the blood, and in this way acts longer, if not more powerfully on the brain, though its narcotic effect is not so soon produced. It is possible that life might be destroyed by the inhalation of ether, if it be continued uninterruptedly for a great length of time and a great quantity inhaled. Fatal congestion of the brain might thus be produced, as sometimes happens when alcoholic liquor has been taken to excess. But no person of ordinary prudence would administer it in this way.

Long before the occurrence of such a result, symptoms of an unequivocal character would indicate the approaching danger.

When death follows the inhalation of chloroform, on the other hand, there is no merciful premonition. The late Dr. Snow, whose experience on the subject was perhaps greater than that of any other person, though that "*sudden palsy of the heart* is the cause of sudden death from chloroform." In death by asphyxia, the heart beats for some minutes after breathing has ceased; "whereas in some cases of death by chloroform, the breathing has been proved to go on up to the time the pulse stopped, and after it."

With the hope that those who may have occasion to employ any anæsthetic agent will at least make a fair trial of *rectified sulphuric ether*, I respectfully submit these remarks to my professional brethren.—*Brit. and For. Med.-Chir. Review, Oct.* 1859, *p.* 484.

162.—ON THE INTERNAL EMPLOYMENT OF MEDICINES IN VAPOUR.

By Dr. J. Birkbeck Nevins, Lecturer on Materia Medica in the Liverpool Royal Infirmary School of Medicine.

[Many diseases of great importance, though perhaps constitutional in their commencement, and treated best by constitutional remedies, are in their termination strictly local, and no longer amenable to the class of remedies before employed. Such are cases of chronic bronchitis, chronic affections of the chordæ vocales, the eustachian tubes, the frontal sinuses, and the passage of the nose. These cases require local remedies, and by proper management local remedies can be used. Thus, impalpable astringent powders may be inhaled in chronic bronchitis, or remedial vapours passed through the nose by expiration through that organ, or driven into the eustachian tubes, or frontal sinuses by forced efforts at expiration, when the mouth and nostrils are closed. The writer next gives a few interesting cases illustrative of the principal above laid down.]

A patient had suffered from complete loss of voice for above a year, not being able to speak above a whisper; the affection was evidently purely local, probably dependent upon a thickened condition of the chordæ vocales. She had no pain or any constitutional symptoms, and had long since given up treatment. She used the mercurial cigarette, to be described hereafter, for a month, and perfectly recovered. A patient in the last stage of phthisis suffered from pain in the larynx, and utter inability to sleep for days and nights together, from the incessant cough and expectoration. Other treatment had been without avail; and I then made him inhale the vapour of strong nitric acid poured into a saucer, and placed near his mouth. He soon experienced relief from it; the pain abated, and the cough ceased to such an extent that he obtained some hours of refreshing sleep. He continued to adopt this means of relief at intervals until his death.

The trouble and annoyance of strong nitric acid in the neighbourhood of a sick bed are, however, so great, that the inhalation of the nitrous acid fumes obtained by the combustion of nitrate of potash is far preferable, and is easily accomplished. A young lady, who suffered much distress from the cough in a rather advanced stage of phthisis, and could only lie on one side, found far more relief from the inhalation of the fumes arising from brown touch paper burning in the bottom of a breakfast cup, and held near to her mouth or far from it as her own comfort dictated, than she did from the employment of cough medicines, the local application of the solution of nitrate of silver, or any other means which she had employed.

In a very chronic case of offensive discharge from the nostrils, with a sense of uneasiness in the frontal sinuses, the patient was quite cured in about a month by the use of the mercurial cigarettes. He held his nose after taking a mouthful of the smoke into his mouth, and then forced it into his nostrils, in the manner sometimes practised by accomplished smokers.

Another patient, who suffered from polypus in the nose, and had been operated upon in London by Mr. Ferguson, and subsequently in this town by myself, is now able to keep the disposition to form fresh polypi in check, by smoking the cigarette, and expelling the smoke through his nose, when he feels uneasiness which warns him that he has to fear a recurrence of the disease.

In the treatment of the form of deafness which is dependent upon an obstructed Eustachian tube, I have increasingly numerous cases in which the smoke forced into the tympanum from the throat gradually restores the sense of hearing. The circumstance which first led me to adopt this method was hearing a deaf patient on one occasion remark that, when he was sneezing the day before, he heard perfectly; the violent effort appeared for the moment to have dilated the Eustachian tube, and hearing was the result. I have at present under my care a patient who has been deaf for seven years, and he has benefited more by this method of treatment than by any other. In this case, however, simple brown touch paper made into cigars appears to be of more service than the mercurial cigarettes.

Such is an outline of the case likely to be benefited by this mode of treatment, and the various methods in which it may be employed. It offers a reasonable appearance of advantage in what are often very intractable diseases; and in bringing it more prominently before the profession, it is with the hope that it may prove a useful addition, in however small a degree, to the remedial agents at present in our possession. Modifications of the method itself, and the employment of other agents capable of being converted into the form of vapour will, no doubt, suggest themselves to the experienced practitioner, if he is satisfied by the results of trial that the principle itself is a beneficial one.

I use, for making the mercurial cigarettes, fifteen grains of nitrate of mercury, fifteen minims of strong nitric acid, and six drachms, or as much as may be sufficient, of water.

Dissolve the nitrate in the nitric acid diluted with the water, and aided by a gentle heat (such as the top of an oven), and soak in the solution thick white blotting paper (eight inches by six). Divide it into eight slips, which are to be rolled round a quill or pencil into cigarettes before they are quite dry, and gummed along the edge. If the paper is quite dry before it is rolled, it becomes brittle, and breaks in the folding.—*Brit. Med. Journal, Sept.* 24, 1859, *p.* 769.

163.—*Remarkable Effects produced by adding Sherry Wine to the Blood.*—Dr. William Addison describes these at length, as follows: At first there is no disturbance in the liquor sanguinis, or plasma, owing to the addition; but afterwards the fluid is seen by the microscope to contain multitudes of molecular particles, which, as Dr. Addison thinks, have come out of the red corpuscles. The corpuscles not only throw off these molecules, but also long threads or tails are projected by them into the fluid. Sometimes as many as five of these tails are seen issuing out of, and remaining attached to, a single corpuscle. They all terminate in a knob at the extremity, and wave about in a very extraordinary manner. Many of them grow thicker: from being at first a delicate filament, they swell out to a considerable thickness, and then breaking away from the corpuscle, they continue a kind of wriggling movement in the fluid. Others remain attached to the corpuscles, and attain a very great length. At the same time, from the numerous molecules issuing from the corpuscles, the liquor sanguinis becomes troubled or disordered, as just described. Dr. Addison describes the corpuscles as undergoing various internal changes before the appearance of the tails, and he remarks that on the addition of the wine to the blood all disposition in the corpuscles to adhere in rolls is removed. Sherry wine alone will produce all the effects described: but the best manner of repeating the experiment is as follows:—Dissolve two grains of common table salt and one grain of carbonate of soda in half an ounce of water. Take a slip of glass and receive on it a "very small" drop of blood; then place, by means of a pipette, a small drop of the saline solution close to, but not touching, the blood, and add to this double the quantity of sherry wine. Let fall a thin piece of glass upon the fluids, and they will mingle in various proportions. To observe the effect of the fluids upon the corpuscles of the blood, the edges of the mixture, and not the middle of it, must be looked at. The fullest effects take place in half an hour. Dr. Addison appeals to this experiment to show that the corpuscles of blood very probably during life throw off morbid matters into the liquor sanguinis, and thus become a source of disorder or distemperature to the fluids of the blood. He argues that symptoms of fever arise from disorder of the corpuscles. Miasms in the air, he says, affect the corpuscles of the blood; a contagious virus is generated, and this is excreted from the corpuscles into the fluid. Thus he seeks to account for the sequence between fever and inflammation: fever appears when the corpuscles of the blood are diseased, inflammation when the fluid or plasma is disordered.—*Brit. and For. Med,-Chir, Review, Oct.* 1859, *p.* 521.

164.—*On the Treatment of Aneurism by Compression.* By S. A. CUSACK, Esq., Surgeon to Steevens' Hospital, Dublin.—[Doubtless manual pressure is the best means of compressing the femoral artery in cases of popliteal aneurism—but it cannot be used successfully on account of the immense labour required to keep it up for any length of time. The nearer we can imitate this, the more successful shall we be in our treatment. Mr. Cusack recommends the use of conical weights suspended from a simple frame overarching the limb. The rounded point of the weight can be made to press accurately on the artery, and by means of additional discs of metal fitting on to the top of the original weight, the pressure may be increased to any required extent. About five pounds and a half will partially obstruct the arterial flow; eight, or eight and a half completely. The author concludes his subject by the following rules to be observed:]

1. The patient should be subjected to the usual diminution of diet, and sedative treatment; abstinence from drink being particularly enjoined, as recommended by Dr. Bellingham.

2. The patient having been put on a moderately soft mattress, and a pillow placed under the knee, a weight of about five and a half pounds should be applied every alternate hour, during the day, to the artery at the groin, and withdrawn at night. The lesser end of the leaden weight which I use is rounded off to the size of a hemisphere, of one inch in diameter; and the integuments are protected by one or more layers of chamois leather.

3. After the lapse of six or eight days, or sooner if the collateral circulation has been fairly established, the weight should be increased to eight and a half pounds, so as to imitate the ligature, and completely obstruct the main artery, care being taken that it is not left on more than an hour or an hour and a half at a time. In this way an immediate cure may sometimes be effected by the formation of a coagulum; or, if not, more slowly by the deposit of fibrine.—*Dublin Quarterly Journal, Nov.* 1859, *p.* 340.

165.—ON THE PATHOLOGY OF OVARIAN DROPSY.

By Dr. J. Y. SIMPSON, Professor of Medicine and Midwifery in the University of Edinburgh.

The ovary, like all other organs of the body, is liable to various forms of disease, and to become the seat of different kinds of morbid growth. But the form of disease or degeneration to which it is above all others prone, is that commonly spoken of as dropsy of the ovary, consisting of a hypertrophy of the organ from the development in it of a number of large cysts or sacs filled with serous fluid. The ovary consists normally of an aggregation of many minute cysts—the Graafian vesicles, which are the essential elements of the organ, inasmuch as in them the ova are formed. These cysts are imbedded in a fibrous stroma, serving to support and separate the different sacs, and to permit of the ramification of vessels and nerves in their exterior. Now, it is a well-known and often exemplified law in general pathology, that when any organ becomes the seat of a new or morbid growth, this new or morbid growth most readily takes on a form of development leading to the formation of a tissue akin to the normal anatomical structure of the organ in which it has its seat. It is in accordance with this great law that the ovary is so pre-eminently prone to become the seat of cystic degenerations. But we meet with various forms of cystic degeneration or disease in the ovaries and their neighbourhood. Within the external margin of the broad ligament, where the peritoneal layers pass downwards and backwards from the fimbriated extremity of the Fallopian tube to meet and invest the ovary, there lies a fibrous-looking, fan-shaped structure, imbedded in the folds of the peritoneum, and known as the organ or body of Rosenmüller—a body of but little importance in the adult, and rarely made a subject of anatomical or pathological observation. In the fœtus, however, it is relatively much larger than in the adult, and is an object of correspondingly greater importance. I have sometimes had occasion already to refer to some of the analogies between the different segments of the male and female organs of generation, and to point out the unity of structure that pervades the two sets of organs; and I may here state that the body of Rosenmüller is in all probability, as pointed out by Kobelt, who has named the organ in question the Pro-ovarium, the analogue in the female of the male epididymus. They each represent, at least in their respective sex, the remains of an organ of great size, and apparently of great importance in fœtal life—the Wolffian body. But what I want more particularly to say at present with regard to the pro-ovarium is this, that being the remains of an organ of tubular structure, the tubuli do not all become entirely obliterated and degenerated into mere fibrous cords, for some of them retain their original character till far on in adult life, and occasionally a secretion of serous fluid takes place into these cæca or tubes, distending and enlarging them, and so producing one of the forms of cystic degeneration so often met with in this locality. The form I allude to consists of cysts between the layers of the broad ligaments, or elongated pediculated cysts attached to the fimbriated extremity of the Fallopian tubes, or their neighbourhood. It is not, however, of this form of cystic disease that I am now to speak, nor of that form of dropsy, which depends on partial obliteration or occlusion of the Fallopian tube and its distension by the secretion of fluid into it between two obliterated points. We put these out of the category of cases of ovarian dropsy, seeing that they have not their seat in the ovary itself, but only in the organs nearest it, and pass on to the consideration of the various forms of cystic disease to which the ovary itself is liable.

1. *Unilocular Dropsy of the Ovary.*—The simplest form of ovarian dropsy is that where there is one single large cyst developed in one or other ovary, with very thin walls, and filled with a simple serous fluid. This is the form of the disease which we most desire to meet with in practice, for it is that which is most amenable to treatment, and in regard to which we may always most confidently hope for a favourable termination. Unfortunately, however, the unilocular ovarian dropsy is a very rare type of the malady; for far more frequently we find it presenting itself, as in the case of all the hospital patients you have had occasion to see, in the form of

2. *Multilocular Dropsy of the Ovary.*—In this form of tumour we have not one single cyst, but a vast numbar of cysts of different sizes, usually, however, with one or two of these greatly predominating in size over the others. Multilocular ovarian tumours may be found of every possible size; and it is among this class that we find those rarer cases from time to time occurring in which the tumour is recorded to have attained to almost fabulous dimensions. At first, however, they are of small size, and the diminutive cysts of which they are made up are all pretty nearly of the same dimensions. The great subsequent bulk of the tumour is usually produced by the excessive development of a few of the peripheral sacs, one or two of which usually come to be much more distended and prominent than any of the

others. It is a matter of very great practical importance to remark and remember, in regard to the growth of multilocular ovarian tumours, at what part of the mass it is that the greatest enlargement of the several cysts generally takes place. Fortunately for the prospects of successful treatment, the cyst or cysts which take on the greatest and most rapid growth are, as I have said, those which are placed towards the surface of the tumour, and at its upper and anterior surface. The largest cysts thus, as a general rule, fortunately come to lie, as development proceeds, high up in the abdominal cavity, and closely applied to the internal aspect of the anterior abdominal walls, through which they can most readily be reached by the trocar and canula, and thus most readily evacuated and obliterated. This development of the cysts in the superior and anterior aspect of the periphery of the tumour takes place in accordance with a general pathological law—viz., that the extension of a morbid growth—especially if it contains fluid—goes on most actively in the direction where it meets with least resistance to its increase from the normal anatomical structures of the body. The firm floor of the pelvis presents an unyielding obstacle to the growth of the tumour in a downward direction, so that the cysts at the lower part of the tumour, having no room to become enlarged and extended, remain, in general, comparatively small and undeveloped; while the cavity of the abdomen, filled only with the soft and mobile viscera, and closed only in front by the distensible abdominal muscular wall, affords free space for its enlargement upwards and forwards. In this direction, accordingly, the development of a dropsical or multilocular ovarian tumour chiefly occurs, and partly by the breaking down of the septa between cysts originally distinct, but more by the secretion into the cysts of a quantity of serous fluid, or gelatinous matter, the cystic mass often finally ends in the formation of a single large prominent cyst, or of two or three prominent and predominating cysts, which then become more accessible for tapping. Sometimes you will find a few cysts of smaller size lying higher up even than this large and prominent sac. For occasionally, when cysts are at first divided by very thick dissepiments, these may resist the process of atrophy which occurs in the great majority of them, and leads to their destruction, and the fusion of contiguous cells; and then the cysts thus surrounded by a thick wall may remain even in the very summit of the tumour, projecting into the cavity of the large cyst, and appearing like a new tumour growing on the interior of its wall. But fortunately for the practitioner, I again repeat, it is not the cysts which are situated lowest down in the pelvis that grow the quickest, and enlarge the most, but those which are situated above, and stretch up behind the thin abdominal parietes, through which they may readily be reached by the point of the trocar.—*Med. Times and Gazette, Oct.* 29, 1859, *p.* 423.

166.—*Peculiar Effects of Phosphorus.*—At a session of the Cercle de la Presse Scientifique, in Paris, the Abbé Moignot directed attention to two facts, novel, and fit to figure in the pathogenesis of a poison, already charged with so many mischievous properties. Females, being *enceinte*, breathing air filled with phosphoric emanations in the establishments where matches are made, are sure to abort; and this result is so common and well-known, that, in localities where the manufacture of matches engages a large number of workmen, the women profit by it to rid themselves of the product of conception. The abbé made this statement on the authority of a pious ecclesiastic, who guaranteed its authenticity. In men submitted to the same conditions, phosphorus vapours induce, after a little while, a vehement excitation of the generative functions. It is now left to the profession to verify the truth of these statements, full of interest, and susceptible, possibly, of being made useful, not only in prophylaxis, but also in therapeutics. Mons. Chevallier has commenced an investigation of the subject, which will be afforded the public as soon as any reliable results have been obtained.—*Jour. de Chim. Méd.—American Med. Monthly, June,* 1859, *p.* 459.

167.—*Removal of Rings from Swollen Fingers.* By E. **Garraway**, Esq., Faversham.—[The following method of removing a ring from a swollen finger is not so generally known as it deserves.]

A reel of cotton is wound evenly round, beginning on the extremity of the finger, and bringing each coil into close apposition with the preceding, until the ring is reached. A needle is then threaded with the cotton, and passed under the ring, and the thread is carefully unwound from the finger. The ring follows each coil as it is successively unrolled, and by almost imperceptible degrees is brought over the knuckle and removed. Care must be taken that the cotton is wound on evenly, particularly over the protuberant and swollen knuckle, or an entanglement will occur in the unwinding. A small curved needle will pass more readily under the ring than a straight one. The process requires time, care, faith, and patience; but the reward is ample in the gratitude of the suffering patient, the signet of whose marriage bond has been saved from destruction.—*British Medical Journal, July 9*, 1859, *p.* 541.

168.—*Iodide of Potassium in Hydrocephalus.* By Dr. **Carson**, Coleraine, Ireland.—[The fourth volume of the *Retrospect* contains a very interesting paper by Dr. Roeser on the exhibition of iodide of potassium in the last stage of acute hydrocephalus.]

When I saw this paper in 1841, I had a case of hydrocephalus on my hands. The patient was, as far as I could judge, completely hopeless. All the ordinary treatment, such as leeching, cold applications, extensive blistering, mercurial action, &c., had been adopted without the slightest benefit. The child lay insensible for a considerable time, with dilated pupils, squinting eyes, and frequent convulsions; he was in the last stage of the disease, and apparently near his end. I gave him two grains of the iodide of potassium every second hour, and he made a perfect recovery. I have had another opportunity, during this present month, of testing the value of this plan of treatment. Miss ———, aged five, after a slight term of gastric derangement, was suddenly seized with an attack on her brain. The acute stage was not violent, and passed speedily over. But the effusion progressed steadily in spite of mercurial action, blisters, &c. She was incapable of being raised, and passed urine and fæces without being conscious of it. The pupils were as wide as they could be, but there was neither squinting nor convulsions. There was not the least sign of improvement at the end of twelve hours after the mercurial action was established. I then put her on the two grains of iodide of potassium every second hour, and stopped all other treatment. On the second day the sensibility had so far returned that she could be annoyed by rubbing the blistered surface on her head, and she was aware of the calls of nature. On the third day she cried when moved on the pillow, and at the end of the fourth day she was perfectly sensible, and the pupils had returned to their original condition. She progressed rapidly after that, and is now as well as ever.—*Med. Times and Gazette, March* 5, 1859, *p.* 245.

169.—*The Dietetic Treatment of Defective Lactation.* By Dr. C. H. F. **Routh**, Physician to the Samaritan Hospital for Women and Children.—It has been long known that one of the most effective methods of increasing the flow of milk in all animals is to give them an abundance of food; and it is almost exclusively this plan of treatment which has been followed out by practitioners of the day in increasing the flow of milk in the human female. Simon had proved already by a special experiment made upon a woman in very poor circumstances (and in whom he examined the milk at fifteen periods, commencing with the second day after delivery), that in proportion as he gave her good food, so in proportion did the quantity of solid matter in the milk increase, albeit the quantity of liquid matter was not so much affected. Hence it is that practitioners seem to have so much faith in this method of increasing the quantity and quality of milk, that they rarely adopt any other. The quantity of food given is far more closely investigated than the quality. Here, again, much that I have before said on the subject of milch cows or goats feeding, in former papers, applies equally to a suckling woman. It is the same common office to be performed in both cases. There is, however, this difference: The plan to be adopted, with a woman, is to produce milk not only sufficient in quantity, but rich in quality, and in this respect, therefore, the disadvantage of a diet too exclusively vegetable is to be guarded against. Such food generally makes the milk thin and serous; and it is only when it is rich in leguminous plants, and in the higher cereatia or flesh, *i. e.* when a due quantity of nitrogenous matter enters into the vegetable aliments taken, that both the quantity and quality of milk supplied are good.

Experience among suckling women proves the efficacy of analogous alimentation. First among the grains are lentil powder, or the so-called revalenta; but pea-soup and bean-soup have also a marked effect in improving the flow and richness of milk. The lentil and bean, however, is preferable to peas, where they are as easily procurable. Besides the better taste, the first is slightly aperient, and the latter does not produce flatus either to mother or child, which peas are very apt to provoke.

There is a remedy that has some notoriety among women in the present day, viz., whiting-soup, which is generally believed to provoke a flow of milk. I believe this is true, but that the effects are somewhat exaggerated. Still it is a good change; fish, and particularly those varieties which are rich in phosphorus, I allude to oysters and crabs, are very efficacious. Of course these last must be sparingly taken at first, and particularly during the first months of suckling, because they do not always agree with the infant, producing urticaria and roseola; but where this effect is not produced, these kinds of food afford a ready means of supplying the phosphates, which are so beneficial to both mother and child. As far as my own experience, however, goes, I give very much the preference to Conger-eel soup. It is not generally known, but is I am told the case, that it forms the basis of all rich and nourishing soups; and for this reason our French brethren, who have so much taste for "potages," import them in immense quantities from this country, where they abound. As a soup, it is peculiarly nourishing, and very readily improves both the appetite and the strength. Like lentil powder, the stomach will often retain it when it will reject all other kind of food. Mr. Jones, of Jersey, speaks highly of it, and gives a case in which, after all other means had failed, it checked vomiting after chloroform. Its comparatively cheap price also renders it very easily obtainable by the poor. Crabs as a remedy for increasing the flow of milk, is of very old date. It is recommended by the author of "Gynæciorum" (p. 634, A). He also recommends for the same object, a blueish coloured fish (glauciscus), taken in its juice, and a variety of smelts (smarides), taken with fennel sauce and boiled in milk.—*Med. Times and Gazette, May* 21, 1859, *p.* 520.

A SYNOPSIS,

CONTAINING A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THE FOLLOWING PAGES: SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE LAST HALF-YEAR. (ARRANGED ALPHABETICALLY.)

AFFECTIONS OF THE SYSTEM GENERALLY.

Diphtheria.—Dr. Bristowe entirely disapproves of the application to the diseased surface of strong caustics and escharotics, and prefers the employment in all cases, of mild detergent gargles, or of warm milk, and such like bland and soothing fluids. For the throat affection is merely a local manifestation of a constitutional disease, and rarely kills except by involving organs, such as the trachea, and deeper tissues of the neck, which are beyond the region of the influence of such agents. Moreover, if applied to the surface of a thick false membrane, they are not likely to be of any use, and the removal of this membrane, even if possible, would only be followed by increased inflammation, and reproduction of the membrane. (Dr. J. S. Bristowe, p. 21.)

The following plan of treatment was found very successful during a severe epidemic of this disease last year, at an asylum for orphans at Croydon. There were fifty cases with only one death. ℞. Solutionis chlorinii ℥ss; syrupi simplici ℥ss; aquæ destillatæ ad ℥vj. M. Fiat gargarisma sæpe utendum. ℞. Solutionis chlorinii gtt. iv; syrupi aurantii ʒj; aquæ destillatæ ad ℥ss. M. Fiat haustus 2ndâ quâque horâ sumendus. The dose was increased according to age. Calomel was given in doses of one grain and upwards, according to age. The diet, too, consisted of concentrated jellies, strong beef-tea, wine, &c. After the third day, quinine was added to the chlorine solution. The same mode of treatment was adopted in adult cases; except, that, instead of calomel, the hydr. c. cretâ was given. (Mr. G. Bottomley, p. 20.)

The plan I have invariably adopted, regardless of sex, age, or incubation of disease, has been to give an active emetic of antimonial wine, from half an ounce to an ounce, according to age; to freely cauterize the throat with solid nitrate of silver; to have a mustard poultice applied from ear to ear; the feet and legs plunged in a hot bath; and the patient confined to bed. After the emetic action has ceased, from three to five grains of calomel with five of compound extract of colocynth were given (or, for a child, two grains of calomel with two grains of compound antimonial powder); and, four hours afterwards, the following mixture:—℞. Quinæ disulph. ʒss; potassæ chloratis ʒj; acidi hydrochlorici diluti ℥ss; aquæ ℥viij. M. Fiat mist. cujus sumatur pars sexta 4tis horis. A gargle of chlorine solution must be frequently used to sponge out the fauces; it may be prepared by impregnating water as much as can be borne with the protoxide of chlorine. The diet should be at first farinaceous, and afterwards consist of strong broths and jellies. Sherry whey may be given alternately with quinine, which latter is of the greatest use, and must be given in large doses. (Mr. J. C. S. Jennings, p. 25.)

Having first freely cauterized the false membranes with lunar caustic, inject every hour against the fauces a solution of common salt, the strength of the solution being such as not to create nausea. The tincture of iodine will also be of use as a topical application. The author places great reliance on this treatment. (M. Roche, p. 25.)

It is of the greatest importance to arrest the local inflammation, and to that and nothing answers so well as painting the fauces over with a very strong solution of nitrate of silver, (15 grains to the drachm). Later in the treatment a weaker solution may be used, or Bretonneau's application, one part of hydrochloric acid to three of honey. But where the inflammation is more sthenic, inhalation of steam and soothing applications are desirable. Of all the medicines which present themselves for our choice, the tincture of the sesquichloride of iron is the one upon which I chiefly rely. (Mr. T. H. Smith, p. 26.)

Rheumatism, *Acute*.—The two chief indications in the treatment of rheumatic fever are, 1. To prevent the formation of an undue amount of lithic acid, or to favour its conversion into urea; 2. To facilitate the elimination of the fibrine present in excess in the blood, the true causative agent of the local affections peculiar to the disease. Now, alkalies not only act as solvents for lithic acid, but also dissolve fibrine. In addition to this, their neutral salts, especially those of potash, are diuretic. Alkalies, therefore, appear to unite more completely than any other known remedy, all the properties requisite in the treatment of an uncomplicated attack of rheumatic fever. Lemon juice, though in some cases acting beautifully, is uncertain, and leaves the latter of the two indications unfulfilled. The same may be said of colchicum. (Dr. G. Whitley, p. 34.)

In many cases of rheumatism great relief will be obtained by the use of narcotic injections, to relieve the pains when severe. Of course the usual constitutional treatment must be employed. In one case a man could not move his arm after acute rheumatism, on account of pain in the shoulder. The pain was removed by a single injection. (Mr. C. Hunter, p. 375.)

Scarlatina.—Chlorate of potash must not be given in scarlatina, with the idea that in chlorine something like a specific has been found for the disease—if so given it will fall into disrepute. It is a very valuable remedy for meeting particular indications in the treatment of disease, by arresting the ulcerative inflammation of the fauces, and by its arterialising properties, supporting the restorative powers of nature, when aided by other appropriate treatment. It may be combined with carbonate of ammonia, with the best effects. (Dr. Fountain, p. 396.)

Sequelæ of Measles and Scarlatina.—M. Scoutetten, of Metz, has devised the following method to prevent the unfortunate sequelæ so frequently supervening after an attack of measles or scarlatina, as well as to prevent the necessity of confinement to the sick chamber for several weeks after convalescence: "As soon as convalescence commences, that is to say, when the skin is no longer red with the eruption, he rubs over the whole body, slightly warmed oil of sweet almonds or olive oil, and puts the patient in bed again, for two hours. The next day he gives him a tepid bath for an hour, then places him in bed, and if the skin is very dry, a new friction with the oil is made. These two frictions and one bath are usually enough to remove all danger. Still, in severe cases, it is well, to avoid any risk, to repeat the means indicated from time to time, until the skin regains its suppleness. These precautions taken, convalescents may be permitted to go out without fear of bad results." (p. 33.)

AFFECTIONS OF THE NERVOUS SYSTEM.

Coma.—Applied externally to the chest and limbs, and gently rubbed in, the external application of mucuna pruriens may be of use in comatose states from chloroform, narcotics, or other causes. (Mr. J. Rhodes, p. 379.)

Cerebral Excitement—Mania.—In cases of violent cerebral excitement, delirium tremens, and mania, everything points to the necessity of some sure, speedy, and active mode of allaying excitement and procuring sleep—yet the stomach is often highly irritable, or in such a state that it will not absorb medicines, or the patient even refuses to swallow at all. Of all cases, perhaps this is the one in which the value of the hypodermic injection of morphia is most clearly seen. Inject one-third to half a grain of acetate of morphia beneath the subcutaneous cellular tissue of any part of the body, and in a few minutes sleep will be procured. (Mr. C. Hunter, p. 371.)

HYPODERMIC INJECTION.—In inserting the point of the syringe, the part must first be rendered tense, and then, if the movement be *quick and steady*, little or no pain is caused. The tissue injected is the loose reticular tissue beneath the panniculus adiposus, as less pain is caused than if the injection is inserted just beneath the skin, and absorption is more rapid. To produce a constitutional effect, localization is not necessary. A good site to select is the inner part of the arm, as the skin is here thin and easily perforated. The dose administered must never be more than half the ordinary stomachic dose for males, nor more than a third for females, the object being to produce a certain effect with as small a quantity as possible. Men bear narcotics much better than women. The fluid used should be made of that strength that three or four turns of the piston shall be an ordinary injecting dose. (Mr. C. Hunter, p. 44.)

HYDROCEPHALUS.—In some cases great amelioration of the symptoms will follow the free use of croton oil as a counter-irritant. The liniment (croton oil, half a drachm; turpentine liniment, half an ounce) must be rubbed, about every four hours, over the entire head, until a plentiful crop of pustules make their appearance. This plan of treatment was first suggested to the author by a case in which the cephalic symptoms supervened on the cure of eczema of the scalp, with profuse discharge. (Dr. J. Watson, p. 42.)

NEURALGIA.—In cases of superficial neuralgia, especially facial, immediate and considerable relief will generally be obtained from the following local anodyne application: two parts of sp. of wine, or eau-de-cologne, one of chloroform, and one of tincture of aconite; the finger covered with a piece of lint or soft thick linen, is dipped in the mixture and rubbed on the part for a few minutes. (Dr. Gueneau de Mussy, *Med. Times and Gazette, April* 2, 1859, p. 350.)

POISONING.—In most cases of poisoning (unless the poison act with extreme rapidity) the loss of temperature of the body, if not the immediate cause of death, greatly favours it. Out of 33 poisons experimented on, 27 killed chiefly by diminishing the temperature. From this fact we may learn how necessary it is to the successful treatment of these cases that we should take means to prevent loss of temperature of the body. It is extremely difficult to restore temperature when it is once lost. It is quite a wrong idea, with narcotic poisons especially, to rely upon antidotes, as we know but little concerning the antidotes of most of them. Certainly, coffee, ammonia, and alcohol, have great power, and are very useful, in cases of poisoning by opium, and must be given as adjuvants to the other means employed. Some poisons stop the heart's action almost at once. If an animal be poisoned by tobacco, and the heart's action has ceased, it may be re-established by injecting blood into the carotids, towards the centre of the circulation, so that it passes into the coronary arteries. This has never been tried in the human subject. (Dr. Brown-Sequard, *Dublin Hospital Gazette*, *Aug.* 15, 1859, p. 245.)

Narcotic Poisoning.—In cases of narcotic poisoning, emetics freqently cannot be swallowed, and the stomach-pump is not procurable; but in *cold affusion* we have a remedy always at hand, and one that in nearly every case, in which recovery is possible by any means, is promptly effectual. (Dr. R. Jackson, p. 43.)

Poisoning by Strychnine.—A case of poisoning by strychnine is related by Dr. Bennett, of Sydney, in which apparently iodine conduced towards recovery. He explains its action by its forming an insoluble hydriodate of strychnine. (Dr. Bennett, *Lancet*, *Oct.* 29, 1859, p. 434.)

PARALYSIS.—In cerebral paralysis, the excitability of the muscles in the paralyzed limb is often, but not always, increased. In lead palsy and traumatic paralysis, the muscular excitability is almost lost. In hysterical rheumatic and spontaneous paralysis, the paralyzed muscles respond normally to the electric current. (Dr. Althaus, *Dublin Quarterly Journal, Nov.* 1859, p. 424.)

Reflex Paraplegia.—Dr. Brown-Séquard remarks, that if strychnia be administered in the reflex form of paralysis, it will be advantageous. This is well illustrated in a case of paraplegia produced by exposure to cold and wet. But given in cases of paraplegia consequent on congestion or actual inflammation of the cord, strychnia will only aggravate the affection. (Dr. W. Moore, p. 40.)

Sun-Stroke.—Probably sun-stroke is attributable to the functions of all the organs that free the blood from those matters that are injurious to the system being entirely or partially suspended, viz., the lungs, liver, kidneys, and skin. The blood is imperfectly oxydized, the bowels are confined, the liver torpid, the secretion of urine much diminished, and the skin hot and dry—the patient usually not having perspired for some days previous to the attack. No doubt this state of body is attributable to complete exhaustion following over-stimulation of the nervous system. *In the treatment* rouse the patient as much as possible—administer brandy, wine, or ammonia liberally—dash cold water from a height over the head and nape of the neck. Calomel and croton oil should be administered, to act on the liver and move the bowels. Stimulant enemata and mustard cataplasms sometimes prove useful. The after-treatment must consist of nourishment and stimulants, together with cold applications to the head, blisters to the nape of the neck, and acting on the liver and bowels. The head-dress at present worn in India affords no efficient means of ventilation—the crown rests on the top of the head. It should be of thicker material, and made of strong close basket-work, well padded, and should afford cover for the nape of the neck, and shade for the eyes, and be sufficiently strong to permit of its use as a support for the head, when the owner is in the recumbent position. (Dr. W. Simpson, 71st Regt., p. 401.)

Tetanus—Poisoning by Strychnia.—Wourali poison is a direct sedative to the muscular system, causing complete relaxation of fibre. It is a direct antidote to strychnine. It does not appear to have been used in poisoning cases in the human subject, but Dr. Harley has succeeded in saving the lives of animals to which strychnine had been administered in poisonous doses. M. Vella, of Turin, has successfully used woorara in cases of tetanus at the French Military Hospital during the late war. In one case, which proved successful, two grains of woorara were dissolved in nine drachms of water, and compresses moistened with the solution were applied to the wound, the strength being gradually increased to fifteen grains in fourteen drachms of water, and the compresses renewed every third or fifth hour. This mode of treatment requires and deserves extensive trial. (M. Vella, Dr. G. Harley, p. 42.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

Bleeding Piles.—One of the simplest and surest means for temporarily checking this kind of hemorrhage, is the application of perchloride of iron dissolved in glycerine, and applied on a piece of lint. (Prof. Simpson, *Med. Times and Gazette, Jan.* 22, 1859, p. 79.)

Heart Disease.—Most cases of disease of the organs of circulation admit of division into two great classes, with different symptoms and causes, and requiring different treatment; the whole depends on whether the disease is at one or the other side of that great barrier, the mitral valve. To the one class belong diseases of the aortic valves; to the other, mitral lesions and obstructive diseases of the lungs, especially old-standing bronchitis with wheezing respiration. Now, when death occurs *immediately* in the former class, it is by syncope; in the latter, by apnœa; in the latter there is congestion of all parts behind the mitral valve; the lungs, the liver, and kidneys are congested, respiration is difficult, there is an icteroid hue of the skin, the urine is scanty, and the engorged venous capillaries unload themselves, causing œdema of the cellular tissue of the lower part of the body. In the case of diseased aortic valves, however, this result is for a long time successfully opposed by the perfect closure of the mitral valve, after a time it may yield, and then the cases are practically the same; still it follows, that, barring the risk of sudden syncope, the danger is much more remote in disease of the aortic than in that of the mitral valves. As regards the principles of treatment, it will be found that in disease of the aortic valves, a tonic, and often stimulating place of treatment, is required in order to counteract the tendency to yielding of the ventricular wall. The sulphate of zinc, among medicinal agents (which, however, must only constitute a small part of our tonic plan of treatment), is very useful; but reliance may especially be placed on senega in these cases; it may be given along with hyoscyamus and nitric ether, and though an empirical remedy, the benefit derived from this medicine in palpitation from diseased aortic valves is "invariably great." On the other hand, in the mitral class of cases, we must direct our efforts to unload the congested vessels. The portal system must be relieved by free cathartics, after which a free secretion of urine may often be established. Free secretion from the bronchial mucous membrane is also of much advantage. Digitalis, squill, and calomel, a grain of each in a pill, is a formula in very frequent use by Dr. Addison. (Dr. G. H. Barlow, p. 47.)

Internal Hemorrhage.—The following styptic recommended by Dr. Warren of New York, will be found scarcely ever to fail in cases of hæmoptysis or uterine hemorrhages. Sulphuric acid ℨ v., spt. turpentine, alcohol, āā ℨ ii. Mix the turpentine slowly with the acid, add the alcohol and keep in a stoppered phial. The dose is 40 drops, and it may be given rubbed up with sugar. (p. 107.)

Popliteal Aneurism.—An interesting case of popliteal aneurism successfully treated by flexion of the knee-joint, was brought before the Royal Medico-Chirurgical Society by Mr. Ernest Hart. The tumour was globular, of the size of a small apple, situated at the lower and outer part of the popliteal space; it had a full beat but was not very near the surface. It was found, on very complete flexion of the knee-joint, that the pulsation in the tumour nearly ceased. After a week's preliminary rest, the leg was bandaged from the foot to the knee, (not covering the tumour) the leg was thoroughly flexed upon the thigh and retained in that position by the application of a stout roller, very little pain was caused, and on the morning of the third day considerable solidification had occurred. On the fifth day the tumour was hard and solid, and neither pulsation nor thrill could be detected, and the case rapidly progressed to the most perfect recovery. (Mr. E. Hart, p. 100.)

Mr. Cusack, of Steevens' Hospital, Dublin, applies compression by means of conical weights, which, suspended to a frame arching over the limb, may be exactly applied to the artery; five and a half to seven and a half or eight pounds, are generally required to arrest the pulse of the femoral artery completely. At first the circulation must only be partially arrested, and the weight must be applied every alternate hour. After the lapse of six or eight days the weight may be increased till the circulation is completely arrested, the pressure not being continued, however, more than an hour and a half at a time. This mode of applying pressure appears the best hitherto suggested, as it approaches the nearest to manual pressure. It is easily applied, and has proved very satisfactory in its results. (Mr. S. A. Cusack, p. 410.)

Styptic.—*Solid Perchloride of Iron.*—The perchloride of iron is manufactured in a solid form by Messrs. Hopkins and Williams, of New Cavendish street, in which state it is particularly manageable as a styptic. Another, and perhaps superior way of using it is to apply, by means of a spun-glass brush, a small quantity of the thick brown fluid, into which the solid perchloride kept in a bottle always deliquesces. It is particularly useful in such cases as excision of the tonsils, bleeding from the deeper-seated gums, &c. No inflammatory action follows the use of this drug. (Mr. J. Z. Lawrence, p. 398.)

Wounds of the Palmar Arch.—Compress the wound in the vessel by a firm roller, lay the fingers over this in a flexed position, and maintain them there, flex the hand on the forearm and the forearm on the upper arm, and render movement of the entire extremity impossible by the application of a roller from wrist to shoulder. The happiest results will follow this mode of treatment. (Mr. O. Pemberton, p. 104.)

Before having recourse to more serious measures, it is well to try the effect of flexion of the forearm upon the arm, placing a small pad of lint upon the wound, with very slight pressure. In country practice, where appliances are not at hand, this may be of much temporary, or even permanent use. (Mr. H. C. Johnson, p. 106.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

Acute Pulmonary Diseases.—*Veratria* given in very minute doses, and frequently, (five millegrammes, .077 of a grain troy, six or seven times a-day) does not produce the usual vomiting, but nausea and the other depressing effects are present. It will be found of great use in acute pulmonary affections, but must be avoided where there is any disorganization of the lung. Its action varies rather in different individuals. It may be given in a pill, with a little opium. (M. Ghiglia and M. Aran, p. 56.)

Asthma.—There is a remarkable tendency to habitude in asthma; that is, the disease having once assumed a particular form or peculiarity, has a great tendency to retain this. From this fact we may deduce a practical rule of considerable importance. When the asthmatic is going on well, leave well alone; keep him as he is; do not try experiments with him. If, on the other hand, he is going on ill, if his case has got into a rut, give it a shake, make some change, *any* change, no matter whether the object is very definite, or the therapeutics very rational, in the hope that, by breaking the existing habit, the patient's condition may be improved. It is an hazardous thing to make any change in the surroundings of an asthmatic whose symptoms are quiescent. (Dr. Salter, *British Med. Jour. Aug.* 13, 1859.)

The remedial action of *strong coffee* in relieving an asthmatic paroxysm is well known—but there are two or three practical hints with regard to its administration worth bearing in mind. It cannot be given too strong, and an excessive bulk is thereby avoided which oppressively distends the stomach, and its effects are less rapid. It is best given without sugar or milk, and very hot. It should always be given on an empty stomach, as, if given after a meal, it retards digestion and thereby favors an attack. (Dr. Salter, *Edin. Med. Jour., June* 1859, *p.* 1112.)

Croup.—The advantage of repeated vomiting to aid in the detachment of the false membrane of croup is admitted by most physicians. Some use antimony, others prefer ipecacuanha, the action of which is less depressing. The sulphate of copper in addition to its emetic action possesses a very remarkable property of acting locally, and this peculiarity makes it superior to both tartar emetic and ipecacuanha. When a solution of this salt is employed, the secreting surfaces are so modified, that no more false membranes are formed, or if they are formed, they no longer present the plasticity which renders them so adherent to adjoining parts. The dose should be repeated frequently till vomiting is induced, and the solution tolerably concentrated. (Dr. Missoux, p. 55.)

Foreign Bodies in Trachea.—*Tracheotomy.*—The opening required is large, in fact, sometimes as large as it can be made, if in a child of four or five years of age. There is no increase of danger or difficulty in making a large opening instead of a small one. It is little or no use employing forceps of any kind for the purpose of extracting the offending body, it is preferable to await the return of cough, which in the act of expiration, will inevitably carry the foreign body with the current of air through the larger and nearer orifice, in preference to the smaller and more remote one. (Mr. F. C. Skey, p. 366.)

PHTHISIS.—It is found that the administration of *ozonized oils* has a remarkable tendency to reduce the frequency of the pulse. It was administered to fourteen patients. In two no such effect was observed, but in the larger proportion of the remainder the effect was very considerable, in some cases to the amount of twenty beats. Oils may be ozonized by exposure for a considerable time to the direct rays of the sun, after previous saturation with oxygen gas. (Dr. T. Thompson, p. 392.)

M. Beau observes that it is extremely rare that a case of phthisis is found amongst workers in lead, and accordingly recommends a trial of this mineral in cases of threatened phthisis. He administers from two to sixteen grains of carbonate each day in pill, suspending the use of it as soon as the patient appears to be sufficiently impregnated. The patient must be supported by nourishing food, wine, tonics, and causing him to observe all the rules of a rational hygiene. (M. Beau, p. 55.)

SYPHILITIC PNEUMONIA.—There is an inflammatory consolidation of the lung which owes its origin to the poison of syphilis. An interesting case is recorded, in which it was associated with a papular eruption, and enlargement of one of the testicles. The treatment consisted of blisterings all over the chest, five grain doses of iodide of potassium, and four grains mercury with chalk, and conium, twice a week. Subsequently the iodide of mercury with chalk, were given, till the gums become tender. (Dr. O'Connor, p. 54.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

ASCARIDES.—Use as a simple injection of water, containing five, ten, fifteen, or twenty drops of sulphuric ether, according to the age of the individual, and repeated more or less frequently as necessary. The author states that this agent has a double advantage. It destroys the larvæ and allays the spasmodic and nervous symptoms produced by the animals. (Dr. Compĕrat, p. 120.)

ASCITES FROM CHRONIC PERITONITIS.—This disease is especially liable to come on in young females. There is distinct fluctuation, emaciation, generally diarrhœa, and frequently hectic fever; in some cases tubercular disease of the lungs may be detected. In fact the relations of the affection are decidedly with the strumous diathesis, and the treatment must be accordingly. Cod-liver oil must be given internally, and must form a sort of basis for the treatment; at the same time syrup of iodide of iron may be advantageously administered, if there is no diarrhœa, and the iodide of mercury ointment rubbed over the abdomen. Keep your patients as much as possible in the open air, and administer a nourishing but unstimulating diet. If there is diarrhœa or abdominal pain, opiates with or without astringents must be given. In two cases out of 36 tapping was resorted to; in one it materially assisted the cure, in the other it gave no permanent relief. (Dr. Paley, p 57.)

CONSTIPATION IN CHILDREN.—The chief cause of constipation in nurslings is the insufficiency of sugar in the breast-milk. Speedy and full relief may generally be given by ordering some sweet sugar water to be taken every day besides the breast. (Dr. Jacobi, p. 322.)

DEFECTIVE ASSIMILATION IN INFANTS.—This disease, which is the most frequent and fatal of all infantile disorders, is almost always the result of want of breast-milk, and the use of injudicious food. Fatty acids, and already artificially digested animal and occasionally vegetable substances, and especially breast-milk, must be supplied. It is a very good plan to mix human and cow's milk. Simple juice of meat is very useful. The remedies of use are phosphate of soda, producing an emulsion with fats, thus allowing of their assimilation; chloride of potassium to dissolve carbonate of lime; phosphate of lime, to enable the blood to take up more carbonic acid, and thus hold in solution more carbonate of lime; (these substances severally strengthening muscular and bony tissue. (Nitrate of silver and sulphate of copper are the best remedies for the diarrhœa. Wine is also required, even in large quantities.—(Dr. Routh, p. 57.)

Dyspepsia.—*Saccharated Lime.*—The ordinary lime-water in use is far too weak a preparation to develope to advantage the therapeutic properties of lime. Now lime combines with sugar, forming a soluble saccharate of lime, the solution of which is sufficiently strong to act as a valuable tonic and antacid. For practical use, the following is the best mode of preparation: Slake 8 ounces of quick-lime; rub up with it 5 ounces of white sugar; add 1 pint of water; stir for some time, till the hard stiff masses which the sugar and lime are liable to run into are as much as possible dissolved; then filter. The product should be perfectly clear, and of only a slightly yellowish tint. A solution made in this way will contain 18 grains of lime in every ounce by weight, and altogether about 106 grains of solid matter to the ounce. This may be given in doses of from 20 or 30 to 60 minims or more, in a glass of water, two or three times a day. This solution is a powerful antacid, and probably the best we have, since it is stronger and pleasanter than magnesia, and instead of weakening digestion like the alkalies, one of its most important uses is as a tonic of the alimentary system in cases of obstinate dyspepsia. As such, its action is much more powerful than that of the vegetable stomachic tonics. It is particularly serviceable in gouty constitutions. It is not in the least constipating like chalk, but affords a very valuable means of overcoming gradually that chronic constipation which is so frequent an accompaniment of dyspepsia. (Dr. J. Cleland, p. 393.)

Fissure of the Anus.—It is unnecessary to divide the entire sphincter, as several examples have lately occurred which have been effectually cured by the division of *a few* of the muscular fibres of the sphincter at the situation of the fissure. (Mr. H. Hancock, p. 113.)

Fistula and Sinus.—Many or most cases of fistula in ano and sinus from other causes may be cured by injection with a small quantity of a very strong solution of iodine, and this mode of treatment, though not superseding the use of the knife, should be tried before having recourse to the latter. The solutions used may be ʒ ss., ʒ i., or ʒ ij. to the ounce of spirit of wine, with a little sulphuric ether as a quick solvent of the iodine, or if it is not wished to keep so large a quantity ready prepared, from gr. iiiss. to gr. xv. may be dissolved in ʒ i. of sulphuric ether, just when a case requires the treatment. A very fine gold nozzle may be fitted to Dr. Alex. Wood's hypodermic injection syringe (Young, Prince's Street, Edinbro', will supply this), and you have everything complete. Supposing it to be a case of fistula in ano, you would proceed as follows: First clear out the fistula by injecting with a little plain tepid water, then, having dilated the rectum with Weiss's female dilator, and inserted a little cotton wool into the rectum to absorb any superfluous injection and so preserve the rectal mucous membrane, insert the nozzle of the syringe previously charged with the stronger injection into the fistula a little way, and inject about 30 minims into the part. A suppository containing half a grain of morphia may then be introduced, and the operation is complete. The author describes it as an operation which in his hands has rarely failed. (Dr. T. Skinner, p. 114.)

Double Fistula in Ano.—Avoid multiple divisions through the sphincter ani, as there is no doubt but that if there are two or more divisions of the sphincter muscle, subsequent union does not admit of such an amount of control over its functions as when only one is made. In some cases, the skin between the two fistulæ may be divided, and then this common cavity connected with the gut by a single division of the sphincter. (Prof. Fergusson, p. 113.)

Hernia, *Radical Cure of.*—Mr. Redfern Davies, of Birmingham, has operated on 40 cases (not including the more recent ones) with success in thirty-seven. He recommends as an improvement in Wützer's instrument, that, by means of a screw, the blades be made to expand at the end towards the abdomen, so that the funnel-shaped canal may be accurately filled by a plug whose sides are inclined towards its own. The transverse diameter of the instrument is also made much greater than the antero-posterior (as recommended by Mr. Spencer Wells), whereby the ring is converted into a mere chink, thus affording additional security against descent of the gut. (Mr. R. Davies, p. 107.)

JAUNDICE.—The first principle to be borne steadily in mind in all cases, whatever their cause, is, to promote in every way the functions of those organs compensatory by which elimination of bile is effected, using warm and vapour baths, saline purgatives, and the various kinds of diuretics. In employing blisters, cantharides is inapplicable, from its action on the kidneys.

In jaundice from acute congestion of the liver, leeches, cupping, fomentations, &c., over the region of the liver, and saline purgatives to unload the engorged portal system, are the curative measures most likely to be followed by relief. When the congestion is primary, due to spirit drinking, and such as may go on to inflammation of the adhesive character, mercury pushed to slight specific action is indicated; but in cases of closure of the ducts, mercury can do no good, here we can only carry out the principle of elimination by other channels. (Dr. S. H. Ward, p. 67.)

PROLAPSUS ANI AND HEMORRHOIDS.—*The Ecraseur.*—The ecraseur is not so frequently used in these cases as it deserves. Two very interesting cases are related in which a speedy and safe cure was effected, and that after the nitric acid plan had failed. (Mr. R. Davies, p. 111.)

STRICTURE OF THE RECTUM.—*New Instrument.*—A very ingenious and useful instrument for dilatation of strictures of the rectum will be found figured and described at p. 145. It is intended to obviate the great difficulty experienced in the treatment of these cases, from the irritable anus and sphincter ani muscle being dilated as much as the rectum above. When introduced, the instrument is in very small compass, but when the stricture is reached, two parallel blades, situated quite at the upper end of the instrument, are made to expand by means of a screw in the handle. Thus the stricture is dilated, whilst the sphincter is not stretched in the least. Instruments on the same principle might be employed with advantage in strictures of other parts besides the rectum. (Mr. A. Todd, p. 118.)

ULCEROUS STOMATITIS.—During the Crimean war, ulcerous stomatitis, amongst the young soldiers, assumed an "endemo-epidemic" form. The most rapidly efficacious method of treatment was found to consist in the exhibition of chlorate of potash, preceded or not by an emetic. If, after some little time, no benefit followed this treatment, the dry chloride of lime was substituted for the chlorate of potash. In most instances, however, a speedy cure resulted from the use of the chlorate alone. (M. Bergeron, p. 58.)

AFFECTIONS OF THE BONES AND JOINTS, &c.

ACUTE PERIOSTITIS.—The chief difficulty in diagnosis lies in its great resemblance to acute rheumatism. In both there is high inflammatory fever, with swelling of the limb and great pain, increased by pressure; but the point of difference is here—that in periostitis little or no pain is caused by pressure, unless it be made over, or in the course of, the affected bone. The swelling is not limited to the larger joints, to the ankle or to the knee, but occupies a wider range, and is œdematous in character. The conclusion in favour of periostitis will be much strengthened if it be found that the attack of inflammation succeeded an injury. In the treatment, do not rely on local depletion, with calomel and opium; make an early and free incision down to the inflamed periosteum, not waiting even for evidence of matter, for the mischief is then done, the periosteum is already separated from the bone, and the bone will die. (Mr. T. B. Curling, p. 95.)

DISEASE OF THE TARSUS.—*Conservative Operations.*—Disease of the tarsus in the vast majority of cases commences in the bones, and affects the articulations secondarily. The following is an epitome of the resources of conservative surgery in disease of these parts. If the os calcis is the primary seat of disease, (and this bone is the most frequently affected of all,) a T-shaped incision having been made, gouge away the diseased osseous structures. This may generally be done with success, for however extensively the cancellous structure of the bone is involved, provided an external sound shell exists, the removed bone in most cases will be replaced by fibroid tissue, which, in time, to a great extent ossifies. If the whole bone is involved, and the astragalus partially, the former must be entirely removed, and the latter gouged as far as necessary. Disease commencing in the astragalus rarely long continues confined to it, and it is the result of experience that gouging operations, even if performed early, are rarely of much benefit; excision ought, as a rule, to be practised in preference. If the ankle-joint and calcaneum have become implicated, in addition to removal of the astragalus, the diseased parts must be gouged from the under surface of the malleolar arch and upper surface of the calcaneum, and the two brought together. A strong and even movable foot will result. When the scaphoid or cruciform bones are primarily diseased, (the middle of the three cruciform is so most frequently) the great tarsal synovial membrane becomes implicated, and Chopart's operation may be required, though sometimes partial removal, and gouging similar in principle to the operations already mentioned, may suffice. The cuboid is seldom primarily diseased. Infants and very young children frequently recover from caries of the tarsal bones with abscess by proper constitutional and local treatment. so that operations on them should not be hastily performed. (Mr. J. Erichson, p. 84.)

Painful Cicatrix after Amputation.—This is not so much induced by the nerve or its bulb being implicated with the cicatrix, as by the adhesion and connection of the cicatrix by firm, unyielding, cartilaginous structure to the periosteum or bone. Separate the cicatrix from the periosteum by a subcutaneous incision, and prevent reunion by from day to day moving the skin backwards and forwards. It is no use excising the cicatrix; in the end matters are only made worse. (Mr. H. Hancock, p. 96.)

Pirogoff's Operation.—In this operation, as performed by Pirogoff, the articular surface of the tibia is left untouched. Dr. Eben Watson, of Glasgow, considers that the unfortunate issue of some of Pirogoff's cases may be attributed to his following this plan of operating, and recommends a modification of the operation, by which the articular surface is removed—thus the inflammation is rendered simply adhesive, otherwise the cartilage must inflame and suppurate, and be partly absorbed, partly discharged, before osseous union can take place between the tibia and calcaneum. The mode of operating recommended by Dr. Watson will be found detailed at p. 87.

Resection of the Ankle-Joint.—Syme's and Pirogoff's operations or modifications of them, are frequently performed in diseases of the ankle-joint, when, by resection of the joint the otherwise healthy foot might be saved. These former operations should never be performed unless there is so large an amount of disease as to preclude all hope of preserving a good and useful foot. Mr. Hancock at the Charing Cross Hospital has now performed this operation four times, three times successfully, and once unsuccesfully owing to pulmonary affection. The success of the operation depends upon leaving the anterior and posterior tibial arteries intact, and not opening the sheaths of the tendons; the only parts cut through are, the external and internal lateral ligaments, and the bones. Neither the extensor nor flexor tendons, the anterior nor posterior tibial arteries are injured, consequently, there are no vessels to tie; when successful, the patients are able to walk and run about with scarcely any perceptible limp. (Mr. H. Hancock, p. 81.)

Suppuration of the Knee-Joint.—*Free Incision.*—When suppuration has taken place in the knee-joint, it is absolutely necessary that an incision should be made. You will commit a fatal error to leave a patient unrelieved of abscess in the interior of the joint, under the impression that you are likely to add to the mischief already existing by opening so large a joint as that of the knee. The incision must be free, and the cyst allowed by its inherent faculty of contraction to force out its own contents. It is desirable to leave the opening patent. and not to bring the edges of the wound together by strapping and bandaging, although this practice may sometimes succeed. (Mr. J. Adams, p. 92.)

SURGICAL FEVER.—*Veratrum Viride.*—The veratrum viride has been very extensively used lately in America, and its effects are thus described by practitioners of that country: The administration of a concentrated tincture reduces the pulse and keeps it reduced with a certainty, and to a degree, which can be effected by no other drug. Dr. Barker and others have published cases where they have thus brought down the febrile pulse in a few hours, from 140 beats to 80, 60, or less in the minute, and kept it at will at this lower standard. It thus is a powerful arterial sedative; but it has this further important action, that it is a powerful depurant, stimulating the action of the skin, kidneys, and secretory functions generally. It thus may be substituted for colchicum in the treatment of acute gout and rheumatism; most probably it depends for its therapeutical effects upon the common principle of the genus, veratrin, if so, we may fulfil the same therapeutic indications with the species which is in all our European pharmacopeias, the veratrum album. (Dr, J. Y. Simpson, p. 98.)

AFFECTIONS OF THE URINARY ORGANS.

ALBUMINOUS ANASARCA.—Tannin is useful in all cases where it is required to arrest hemorrhages, to give tone to the organism, or to remedy morbid secretions. Tannin given in doses of ʒss. to ʒi per diem will be found to cure anasarca or œdema developed passively and occurring simultaneously with albuminous urine; its curative action is soon manifested in return of appetite, more normal state of urine, &c. (Dr. P. Garnier, p. 76.)

CATHETERISM.—*Precaution.*—When the bladder is greatly distended do not draw off the whole of the urine at once, especially if in a debilitated person, for it has occasionally happened that a fatal syncope has occurred, or depression from which the patient has never rallied. After removing thirty or forty ounces, withdraw the remainder in the course of half an hour or an hour. (Mr. H. Thompson, p. 143.)

DIABETES.—Some interesting cases of diabetes are related. The following is the treatment pursued in the first case: ℞. Acid. sulph. dilut. ℥iij.; liq. gent. compos. ʒiiss. M. cap. coch. parv. ter die ex aquæ cyatho. ℞. Aloes. barb., ext., saponis, āā gr. xxiv.; pulv. ipecac. gr. iv.; ext. nucis vom. gr. iij. M. et div. in pil. xij. cap. j. vel plures h. s. ut opus f. A diet of bran cake, meat, and a free use of vegetables was enjoined. Dr. Camplin's bran cake is composed of ground wheat, bran, eggs, butter, and milk. It contains scarcely any starch. "It at once checks the formation of sugar and arrests the whole train of morbid actions." The formula for its preparation will be found detailed at p. 425, *Retrospect*, vol. xxxv. (Dr. J. M. Camplin, p. 73.)

DIURESIS.—The constant drain on the system deteriorates the health, so that this affection sometimes becomes very serious; frequently it ends in phthisis, or may first pass on to mellituria. Tonics, as iron and quinine, must be freely given, along with opium or Dover's powder, and the health maintained in every way possible. Belladonna may be given with advantage, in small doses, as in irritability of the bladder, with a view of controlling the irritability of the kidneys. (Dr. Willshire, p. 75.)

DIURETIC.—The "Erodium Cicutarium" or "Stork's-bill," an indigenous plant growing in sandy places near the sea side, possesses great diuretic properties, and may be advantangeously employed in many cases of dropsy. The mode of preparation is to infuse an ounce of the dried plant (every part of it) in three pints of water, stewing it in an oven, until two pints remain. The dose for an adult is four or five fluid ounces, three times a day; probably more may be needed in some cases. (Mr. I. Byerley, p. 284.)

HYDROCELE.—Mr. Pollard, at St. George's Hospital, adopts with success the treatment of hydrocele recommended by Dr. Simpson, (see last vol. p. 277) viz., the introduction of metallic wires through the sac, leaving them there for a few days; they completely drain it of fluid, and excite sufficient inflammation of an adhesive character to ensure the obliteration of the sac. (p. 148.)

IRRITABLE BLADDER.—An interesting case of irritable bladder is related, which had existed nearly a year and a half, and, as usual, was chiefly noticeable in the night, two or three times the natural amount of urine being passed, pale and insipid, but otherwise normal. Many other remedies had been tried without benefit. Extract of belladonna was then given, at first in doses of the twelfth of a grain gradually increased to the third of a grain, and ultimately a grain and a half was given during the day. The specific symptoms were then quite established—there were nausea and dilated pupils. The irritability of the bladder was almost entirely subdued, and now, six months after, the patient remains perfectly free from any recurrence of her distressing complaint. The whole course of administration occupied rather more than six weeks. This is a disease which is chiefly met with amongst the upper classes of society, and seem to be in great measure dependent on luxurious and enervating habits. (Mr. H. Behrend, p. 147.)

LITHOTRITY.—Cases should be rejected if there be—1. Manifest disease of the kidney. 2. The urethra so contracted as not to admit with facility a lithotrite of ample size. 3. The bladder so intolerant as to be incapable of retaining its urinous contents for three or four hours; and on the other hand, a bladder of low nervous susceptibility. 4. Much enlargement of the prostate gland. Moreover, in performing the operation, the following cautions should be observed: The quantity of water injected should not exceed four or five ounces. No attempt should be made to open the instrument till it has been pushed thoroughly home into the bladder. In the act of separating the blades, pass the lower blade downwards towards the bottom of the bladder, that the upper blade may not be painfully pressed against the neck. The instrument should retain the mesial line throughout the entire operation, there being neither necessity for, nor advantage in, directing the instrument to the right or left. At the first operation do as little as possible, breaking the stone only once. No advantage is gained by an abstemious diet. (Mr. F. C. Skey, p. 123.)

Lithotrity and Removal of the Fragments at the Time.—A "favourite method" of Mr. Fergusson's for expediting the cure in cases of lithotrity is to remove, by means of a light instrument, as many of the fragments as can be got away, at the time of operation. The stone must be broken by a larger and stronger instrument. There is sometimes a little bleeding and difficulty at the meatus. Fragments too large to be voided spontaneously, may thus be removed. (Mr. W. Fergusson, p. 126.)

PENIS, *Amputation of the.*—To obviate the difficulty in keeping the urethra open after amputation of the penis, immediately after the operation introduce a director into the urethra, and by the aid of a bistoury slit up the urethra and skin covering it, up to the extent of about two-thirds of an inch. Now place a single suture on each side of the slit, uniting the mucous membrane to the skin. Perfect patency is thus given to the orifice, which is of a long oval form; and after cicatrization is complete, there remains a free opening into the urinary canal. The same plan may be adopted with equal success in the treatment of obstinate stricture at the glans penis. Mr. T. P. Teale, p. 148.)

RETENTION OF URINE.—Two interesting cases of retention of urine are related, in which the obstruction (probably stricture, aggravated by temporary spasm of the urethral muscles) was overcome by passing down to the part, and pressing against it for a short time, a small piece of potassa fusa inserted into the end of a wax bougie: the wax was well moulded over all but the extreme point of the caustic. A small gum-elastic catheter without the stilette may be substituted for the bougie. In both cases the passage of a catheter had been found impracticable. (Mr. L. Parker, p. 127.)

STRICTURE OF THE URETHRA.—*Dilatation.*—Generally the improvement in the stream of urine is commensurate with the mechanical progress, yet occasionally cases occur in which there is an absence of improvement in the power of micturition, although dilatation has been carried on successfully. This may be owing to one of two causes. The bladder may expel the urine with its wonted power, but that fluid, coming in contact with the irritable portion of the canal, causes it to contract forcibly. Or it may be owing to loss of power in the bladder. The former requires for its cure *complete* dilatation of the urethra; the latter frequent emptying of the bladder artificially; and both, the greatest attention, to the general health. In fact, the loss of power in the bladder is often but a symptom of constitutional debility. (Mr. H. Smith, p. 145.)

Treatment by Caustics.—Of course, simple dilatation, in the great majority of cases, will succeed in giving satisfactory and permanent relief. If this fails, we have two resources, viz., the division of the stricture by some cutting instrument, or the application of caustic. External division is an operation justifiable only in the most urgent cases, and especially when anterior to the bulb should, whenever possible, be superseded by internal division. In intractable cases, as a general rule, the use of the potassa fusa will be attended with the most beneficial results, rendering it unnecessary to resort to the knife. The cases in which potassa fusa is most applicable are—1. Strictures having a cartilaginous hardness, impermeable as well as permeable. 2. Strictures which bleed more or less freely on the introduction of the bougie. 3. Irritable strictures. If the stricture is impermeable, commence with a piece of caustic, the size of a pin's head, inserted into a hole in the point of a soft bougie, and prevented by a little lard from acting before it reaches the stricture. Press this gently against the stricture for a minute or two, and then withdraw. If the stricture is permeable, pass the bougie several times slowly, backwards and forwards over the whole surface of the stricture. This must be repeated as necessary. For a long hard gristly stricture the quantity of the caustic must be increased. (Mr. R. Wade, p. 136.)

Internal Urethrotomy.—The treatment of strictures in which dilatation has been proved to be only a slight palliative, by internal division, is now but rarely practised. Yet, though by no means taking entirely the place of external division, it may with the greatest advantage be employed in a large number of cases in which incisions are required. It is a much simpler, easier, and less severe operation than external division. No instrument must be employed capable of making a deep incision, for, where a deep incision is required, external urethrotomy should be practised in preference. The cutting instrument used must be drawn from behind forwards, thus the incision is not jagged, as it would be if made during the passage of the instrument from before backwards. The limits of the stricture being first accurately defined, the whole of the contracted part must be divided. After division, the borders of the incision must be maintained apart by catheterism, and healing by first intention thus be prevented. Internal division is almost devoid of risk when anterior to the scrotum, and it is an almost invariable rule that the non-dilatability of strictures increases with their proximity to the external orifice of the urethra. Thus the indications for internal division and its freedom from danger happily coexist in relative proportions. (p. 135.)

The best instrument for ordinary use is that designed some fifteen years ago, by Civiale, of Paris. The shaft is almost equal in size to an ordinary No. 3, the bulb to No. 5. By means of the bulb the extent of the narrowing can be accurately told. When the instrument is used, the bulb having been passed about half an inch beyond the obstruction, the cutting side must be directed downwards, and directly in the middle line; the blade is made to project to the required extent, by means of an apparatus in the handle, which accurately controls it, and the instrument is firmly pressed on the floor of the urethra and slowly and steadily drawn outwards, about an inch r an inch and a half, so as fairly to divide the obstructing portion. For smaller strictures than Civiale's instrument is applicable to, Mr. Thompson has invented a smaller instrument on essentially the same plan. Woodcuts of both will be found at pp. 164, 165. (Mr. H. Thompson, p. 132.)

AFFECTIONS OF THE SKIN, &c.

CARBUNCLE.—*Incisions.*—The great point to be observed in making incisions in anthrax or carbuncles, is to make them sufficiently deep to go through he inflamed skin and areolar tissue to the healthy parts beneath. If properly done, the flaps made by a crucial incision will be quite loose when taken hold of by the forceps, and will curl up and leave a widely gaping wound. (Mr. M. H. Collis, p. 150.)

CONTRACTION FROM BURNS.—*Simple Extension.*—Mr. Holmes Coote, at St. Bartholomew's Hospital, recently tried simple extension, perseveringly carried out, in one of these cases. By suitable appliances, the head and chin were kept extended, with the effect of bringing back the lower lips and jaw to their natural position. The effect of extension is to cause absorption of the adventitious material present in the cicatrix, and thus permit the latter not only to become soft and extended, but permanently to remain so. (Mr. H. Coote, p. 98.)

ERYSIPELAS.—*Pigmentum Album.*—Paint the part over pretty thickly with common white paint, going a little beyond the edge of the inflammation. "I have never yet met with a case of this nature where it has not done immense good." The light, shining skin soon becomes wrinkled and shrunken, and after repeating the application once or twice the inflammation very rarely extends. This same application may be used with advantage in other forms of cutaneous affection. (Mr. A. Freer, p. 149.)

GANGRENOUS SORES.—*Disinfecting Powder.*—Mix and rub together 100 parts of plaster of Paris and from 1 to 5 parts of coal-tar; they form a grey powder with a bituminous odour. This powder may either be used as it is or made to cohere into a sort of ointment by olive oil, when it may be spread on lint and used as a dressing. It is found that this powder absorbs the pus and other morbid products engendered on the surface of sores, and moreover destroys all offensive odour. (MM. Corne and Demeaux, p. 151.)

LUPUS SUPERFICIALIS.—The treatment adopted at the Charing Cross Hospital, as exemplified in a case reported under Dr. Willshire's care, consists in the internal administration of arsenic, dulcamara, elm bark, and of cod-liver oil, the latter being likewise used as a local application. This treatment is described as having produced "a really wonderful effect." The redness is diminishing and slowly disappearing. By some, this form of lupus is considered essentially scrofulous. (Dr. Willshire, p. 152.)

SCABIES.—*Glycerine Ointment.*—The ointment made according to the following formula, is recommended by M. Bourguignon as having been successful in his hands. One general friction, not preceded by soap ablutions, is sufficient. Yelks of two eggs; essence of lavender, lemon, and mint, of each seventy-five drops; essence of cloves and cinnamon, of each 120 drops; gum tragacanth, half a drachm; well pounded sulphur, twenty-six drachms; glycerine, thirty-two drachms. Total weight, nearly eleven ounces. Mix the essences with the yelks of egg, add the gum tragacanth, make a good mucilage, and then add very gradually the glycerine and sulphur. Glycerine may also be advantageously substituted for axunge in the preparation of the well-known Helmerich ointment. A formula for its preparation will be found at p. 156.

SLOUGHING BEDSORES.—These do not depend entirely on pressure, for they often come on where there is no pressure. It can be ascertained by experiments on animals, that injury of the cord is followed by sloughing, in parts not exposed to pressure. The real cause is irritation of the cord; sloughing of this nature may be prevented, or if it have occurred, may be checked, sometimes with great rapidity, by alternately applying hot and cold applications; apply sometimes eight or ten times a day morsels of ice to the parts threatened or affected, then apply a very hot poultice. Thus, a change in the circulation of a part, and consequently a change in its nutrition, is produced. (Dr. Brown-Sequard, p. 152.)

AFFECTIONS OF THE EYE AND EAR.

CANCEROUS ULCER NEAR THE INNER CANTHUS.—An irregular slowly-spreading cancerous ulcer close to the inner canthus of the eye was successfully extirpated by the sulphate of zinc paste, as recommended by Dr. Simpson. A few grains of the sulphate having been heated to drive off the water of crystalization, were powdered and made into a thick tenacious paste, by the aid of a little glycerine. A little of this was applied over the ulcer and its hard edges, and the whole covered with dry lint. The unhealthy surface and hard edges were destroyed, and the wound cicatrized. In these cases it is found by experience that extirpation by the knife does not answer, the scirrhous hardness and obstinate ulceration returning. (Dr. W. Mackenzie, p. 161.)

CORNEAL FISTULA.—A case lately occurred to Mr. Dixon at the Moorfields Ophthalmic Hospital, in which a minute corneal fistula occurred in the cicatrix, left after extraction of cataract. At first, cauterization with a probe coated with nitrate of silver was tried alone, but without success. Subsequently a counterpuncture was made at the lower part of the cornea, and the cauterization at the same time repeated. Whilst the cornea was thus flaccid, the old fistula completely and firmly healed. (Mr. Dixon, p. 159.)

GLAUCOMA.—The following operation proposed and performed by Mr. Hancock in a case of acute glaucoma possesses several advantages over Graefe's: Introduce a Beer's cataract knife at the junction between the cornea and the sclerotic, the blade being inclined downwards, the point proceeding inwards and backwards. The place of puncture is at the commencement, on the outer side, of the lower semi-circumference of the cornea. The point of the knife having traversed obliquely the layers of the cornea, must be pushed backwards toward the interior of the globe, thus dividing the ciliary ligament in a portion of its extent. On the removal of the knife, a quantity of discoloured fluid will escape. The advantages are: that by the situation and oblique direction of the incision, a free drainage of the fluid is provided for. The iris is very slightly wounded. The pupil is preserved of its original size, and in its normal situation. The operation is very simple, and is performed remarkably quickly. (Mr. H. Hancock, p. 177.)

Iridectomy.—Graefe's operation of excision of a portion of iris in glaucoma is not so much employed as it ought to be. Many cases are allowed to drift into hopeless blindness, which by it might be restored to useful vision. The mode of performing the operation has been previously described. The immediate effect is to lessen the tension of the globe: the wound heals in a couple of days, during which the aqueous humour drains away, but when union is quite complete, and the integrity of the anterior chamber is restored, the globe still continues naturally soft. The most complete results are obtained in acute cases, where the improvement of sight is very rapid and striking, and the cessation of pain equally so, when the operation has been early performed. An ordinary Beer's extraction knife is a safer instrument for incising the cornea than the lance-shaped knife of Jaeger. The fact that removal of a portion of iris does lessen permanently the excessive tension of the globe, is undeniable, though the modus operandi is still unexplained. (Mr. J. W. Hulke, p. 173.)

NÆVUS OF THE EYELIDS.—The simplest plan of treating these is, by drawing a little floss silk through them, steeped in perchloride of iron. The following inflammation will be slight. If this fails, severer remedies may be resorted to. (p. 161.)

NEW OPHTHALMOSCOPE.—A new ophthalmoscope, the invention of M. Graefe and his assistants, is now in use at the Ophthalmic Hospital, Moorfields. It is fitted with adjusting tubes, rests for the patient's head, &c. It is rather a cumbrous affair, and requires to be fixed to the table, or elsewhere, before use. The advantage of it is, that the proper focus having once been adjusted, a dozen observers may look through in succession, and all see exactly the same part of the retina without any trouble. Its cost as at present made is about five guineas. (M.Graefe, p. 179.)

Night Blindness in Scurvy.—Night blindness in cases of scurvy on board ship or elsewhere is more frequent than is generally supposed. The affection appears simultaneously with scurvy, and disappears suddenly whenever a better diet is obtained, as fruit, fresh meat, and vegetables. It probably depends on an altered state of the retina, the scorbutic blood not nourishing, and blunting the nervous pulp. (Dr. A. Bryson, p. 171.)

Obstructed Lachrymal Passages.—Mr. Hulme, at the Central London Ophthalmic Hospital generally treats these cases, when the nasal duct is the part obstructed, by gradual dilatation, by successive sizes of probes, made of virgin silver, that they may more readily bend. In the first place the canaliculus is slit up, not quite to the nasal sac however, and then the probes are introduced, a small one at first, which is bent over the edge of the lid and cut off. At the end of three or four days this may be removed, and a larger one introduced. (Mr. E. C. Hulme, p. 157.)

Sclerotitis and Iritis.—Give a quarter of a grain of hydrochlorate of morphia every third hour, fomenting the eye frequently with warm water—the majority of cases yield to this very simple treatment, especially those in which intense pain is a prominent symptom; but a certain number of cases remain, in which the old plan of treatment is the better to pursue. (Mr. J. Z. Lawrence, p. 173.)

Soft Cataract.—It is well known that when soft cataract is treated in the ordinary way, by being broken up with a view to solution, it is often dissolved very slowly, and not without ultimate injury to the visual powers of the eye. But by the operation of linear extraction, or, in other words, extraction through a small section, the pieces may be successfully removed at once. The incision of the cornea is made by means of a triangular-shaped knife, sharp at the point, keen on both edges, and about two and a half or three lines broad at the base. It should enter the cornea near its outer margin, and pass horizontally in front of the iris, until the whole length of the cutting edges has entered the anterior chamber; it is then withdrawn. The capsule must then be broken up, and the fragments brought into the anterior chamber. Most of these will come out with the gush of aqueous humour. Others may, if necessary, be removed by a small silver scoop; but there is no objection to a few small fragments being left; they will rapidly be absorbed on the resecretion of the aqueous humour. (Mr. B. Bell and Dr. P. Watson, p 168.)

Strumous Corneitis.—In children, what is called strumous corneitis, is in most cases the result of a congenital syphilitic taint, and the remedies directed against it should be chosen accordingly. Mercurials and iodides should be given, at the same time supporting the system by tonics and a liberal diet. The mild mercurial ointment should be rubbed in behind the ears every night at bedtime. Ptyalism must never be induced. If the intolerance of light be great, the occasional employment of blisters behind the ears may do good. (Mr. J. Hutchinson, p. 163.)

Weak Vision in the Aged.—In the case of aged persons whose sight is becoming enfeebled, and requires the aid of convex glasses, great advantage is derived, supposing no nervous lesion exists, from painting every evening the eyelids and brow with laudanum, and allowing this to remain all night. (Prof. Nascar, p. 177.)

MIDWIFERY, AND THE DISEASES OF WOMEN.

Acute Peritonitis after Abortion.—*Chloroform.*—Chloroform, when given in full doses, either by inhalation or swallowing, depresses and brings down the rate of the pulse from 90 or 80 to 70, 60, or less. This may be taken advantage of in some instances of disease, especially acute serous inflammations. Dr. Simpson relates a very interesting case of acute peritonitis after abortion: the pulse was weak and scarcely perceptible, ranging above 150, and there was little hope of the patient surviving. Chloroform was given to relieve the intense pain, when the pulse was found to sink down to 100, or less, and became stronger and steadier; and as long as the action of the chloroform continued, the pulse continued thus greatly lowered in rate and improved in power. The patient was kept for sixty consecutive hours under its influence, and the great abdominal tenderness and tympanitis were then almost entirely reduced, the patient being in a much more satisfactory state. The pulse never rose again to any very high rate, and she recovered nicely. (Prof. Simpson, p. 180.)

CAOUTCHOUC AIR-BAGS.—At p. 240 will be found some illustrations of M. Gariel's ingenious application of caoutchouc air-bags. One bag may be used either as a pessary in displacement of the uterus, as a plug in uterine hemorrhage, or as a dilator of the os uteri. On the same principle M. Gariel treats strictures of the urethra, œsophagus, cervix uteri, &c., and similarly the nasal fossæ might be plugged in epistaxis. In all cases the bags are introduced collapsed, and when introduced, are inflated either by a bag insufflator or syringe. (p. 240.)

DEFECTIVE LACTATION.—*Dietetic Treatment.*—Experience proves that much may be effected by proper diet; the food should be "analogous." First among the grains are lentil powder, or the so-called revalenta; but pea-soup and bean-soup have also a marked effect in improving the flow and richness of milk. The lentil and bean, however, are preferable to peas, when they are as easily procurable. Shell-fish, as oysters, are peculiar in containing much phosphorus. If, on trial, they do not produce urticaria or roseola in the child, they will on the above account be advantageous, as the phosphates are beneficial to both mother and child. (Dr. C. H. F. Routh, p. 310.)

FIBROUS TUMOUR OF THE UTERUS.—In the case of fibrous tumour developed in the uterine stroma, the best treatment, if any treatment is necessary, is to cut down upon it, making a deep incision through the mucus membrane of the uterine cavity, through any intervening tissue, down to the tumour. We thus allow the tumour to enucleate itself, and to transform itself into a fibrous polypus, permitting at some after period an easy removal. We moreover partially destroy the congeries of enlarged and congested vessels enveloping the tumour, and, knowing the marked tendency which these tumours have to degenerate when disturbed, we may hope that some subacute inflammation may take place, resulting in a suppurate disorganization. (Dr. A. K. Gardner, p. 213.)

HEMORRHAGE FROM CARCINOMA UTERI.—In cauliflower excrescence of the uterus, the hemorrhage is occasionally most violent and alarming. Simply plugging the vagina does not suffice to check it, and the use of some powerful styptic means becomes necessary. Of these the simplest and surest is the perchloride of iron dissolved in glycerine. It may be applied by a sponge or piece of lint. Tannin also is very useful; it may be applied in the form of a medicated pessary; it rapidly coagulates the effused blood, and thus prevents the further flow. (Prof. Simpson, p. 234.)

OVARIAN TUMOUR.—*Injection of Iodine.*—In most of the cases recorded in which iodine has been injected into ovarian cysts, some four, six, or eight ounces of the strong tincture have been used, and many of the subsequent ill effects were owing to the rapid absorption of so large a dose of alcohol. It is better to use an aqueous solution, sufficiently strong to cauterize the secreting lining membrane of the cyst. In a case successfully treated by the author, a scruple of iodine and half a drachm of iodide of potassium were dissolved in an ounce of water. The pain caused by this became very severe in the course of an hour and a half. Opium was given freely, and next day the pain had abated. Wine and quinine were administered for some weeks after the operation. (Mr. S. Wells, p. 226.)

OVARIOTOMY.—Never proceed to operate without first making an exploratory incision. Never operate in cases in which there is co-existing disease of some important organ. By thus selecting cases, the mortality after this operation will be materially lessened. It is remarkable that frequently the most successful cases are those in the most advanced stage of disease, whilst the most fatal are often in an early stage of development, and the general health comparatively little injured. In securing the peduncle, the ligatures formerly used are almost or quite superseded by the use of hare-lip pins or clamps, the suppuration within the abdominal cavity incident to the former being avoided by the use of the latter. It is probable, however, "that the écraseur may completely do away with the ligature and the clamp." Cotton wool is the most comfortable covering for the wound. Complete excision of the cyst is preferable to injection of iodine, which, although proved to be occasionally successful in those rare cases when the cyst is unilocular, yet its effects even in these cases are uncertain, and sometimes deadly. (Mr. T. S. Wells, p. 220.)

If a proper clamp be carefully applied, as recommended by Mr. Hutchinson, no vessels require tying, and the sloughing pedicle is properly secured externally. During the operation two assistants should carefully cover the wound with warm wet flannels, so as to prevent the extrusion of the intestines, and especially the admission of air into the peritoneal cavity; the air in the room should be warm, not less than 66° to 70° Fah., and the same should be maintained for some days after the operation. Be careful lest the cut edges of the peritoneum be caught by the sutures or pins employed in bringing the edges of the wound together; the first suture should be applied close to the pedicle, and others at every half inch upwards. Opium and wine should be administered to the patient an hour before the operation, this is a point of great importance; if the pure opium of commerce made into pills direct from the mass be used, and given by the mouth, most patients will be able to take opium who would not otherwise bear it. Adhesions do not offer any objection to the operation, indeed, it is doubtful if the peritoneum is not so thoroughly altered from its normal character, as to be less prone to inflammation on this very account. Adhesions should be torn through, never cut. (Mr. I. Brown, p. 228.)

PELVIC ABCESS.—The exploring needle is never used to more advantage than when employed for the exploration of pelvic abscesses, when they happen to be unusually difficult or doubtful in their diagnosis. The best exploring needle is a long slender thread-like trocar, with a wire stylet passing through it, and this instrument may with safety be passed into the most important organs, and the most malignant tumours. Inflammatory pelvic tumours feel fixed and immovable to a degree seen in the case of no other morbid growth, and more particularly when occurring in the broad ligament, —their most common seat—and lying close to the ilium, they feel so hard and adherent that they might almost be mistaken for an osseous tumour. (Prof. Simpson, p. 205.)

PHLEGMASIA DOLENS.—In the treatment of this affection, which depends essentially on a toxæmic state of the blood; the first and most important indication of general treatment is, depuration of blood by exciting the various organs of elimination to increased activity, by their appropriate stimulants. An emetic, or a mercurial purgative combined with ipecacuanha or antimony, may be given at first, but not so as to produce debilitating effects. Perhaps small and repeated doses of alkaline salts, are, as a general rule, the safest and most efficient remedies to fulfill this indication. Tonics and stimulants are rather required than any antiphlogistic measures, and of these iron and quinine are the best. The best local treatment is, to wrap the limb up in cotton wool, and then encase the whole in oil silk, to prevent the escape of the insensible perspiration; the limb should be slightly raised in bed. Towards the end of the case a flannel bandage neatly applied from the toes upwards will greatly favour resorption. (Prof. Simpson, p. 198.)

PLACENTA PRÆVIA.—*Air Pessary.*—An interesting case is related by Mr. Jardine Murray, of Brighton, in which the os uteri being dilated sufficiently to admit two fingers, and syncope impending from the hemorrhage, a flattened caoutchouc air-pessary was introduced between the wall of the uterus and the presenting surface of the placenta. The pessary being then inflated by means of the attached syringe, it acted admirably as a direct plug and dilator of the os uteri. Whenever from time to time the trickling of blood recurred, it was effectually checked by further dilatation of the pessary, by a few strokes of the syringe. In two hours the os was sufficiently dilated to admit the hand, and though turning was necessary, as the shoulder presented, the case was soon brought to a favourable conclusion. (Mr. J. J. Murray, p. 196.)

PREGNANCY.—*Keisteine in the Urine.*—If a little rennet be added to the urine of a pregnant woman, it considerably accelerates the deposition of kiesteine, and thus renders this more eligible as an aid to our diagnosis of doubtful pregnancy. If when the deposit is well formed we add to, say half an ounce of the turbid urine (taking the lower portion) a few drops of strong solution of ammonia, and boil for a minute or two, and we obtain a semi-mucous, almost tremulous mass, our opinion as to the presence of kiesteine is strengthened. By a mode of preparation described at page 187, the juice of the rennet may be always kept ready at hand for use. (Dr. J. B. Hicks, p. 187.)

PREMATURE LABOUR, *Induction of.*—The following is the mode of procedure by Prof. Braun, of Vienna: Soften in hot water the end of a well-oiled catgut bougie, a foot long and from two to three lines thick, and pass it along the index finger with a twisting movement into the uterine cavity, until only a portion, equal to two fingers' breadth, remains in the vagina. The bougie so passed always excites pains in from six to twenty hours, does no injury to the membranes, and is to be removed only just before the discharge of the waters, or the birth of the child. (Professor Braun, p. 186.)

PRURIGO OF THE VULVA.—This may be relieved, and generally cured, by the assiduous and persevering application of a solution of borax, five or ten grains to the ounce of water, or in a little infusion of tobacco. An ointment of the iodide of lead, or of bismuth and morphia is very useful. Chloroform applied locally in the form of vapour, is one of the most certain means which can be employed. These remedies should be alternated. In the more obstinate cases astringents will be found of much use: alum, or aluminated iron, or tannin. Of course the general health of the patient must be attended to. Arsenic, aqua potassa, and other alterative medicines of that class may be required. (Professor Simpson, p. 214.)

PSEUDOCYESIS.—*Diagnosis.*—There is a form of spurious pregnancy, in which there is a firm unyielding swelling of the abdomen, often supposed to be due to the enlargement of a gravid uterus, but which is in reality due to a tympanitic state of the bowels and a peculiarly tonic condition of the abdominal muscles; and the abdominal walls are so firm and tense, and resist the pressure of the hand so effectually as to render an adequate examination utterly impossible. In such cases as this give chloroform; under its influence, if deep enough, the abdominal muscles will become perfectly relaxed, and on pressing on the abdomen the walls will give way before your hand, and sink backwards until you can feel the spinal column quite distinctly. This curious affection is probably owing to some affection of the diaphragm, which is thrown into a state of contraction, and pushes the bowels downwards in the abdominal cavity. (Professor Simpson, p. 180.)

SPONGE TENTS, *To Make.*—Compression of the sponge is best executed by means of a copying machine or in default of this, by laying it between pieces of board upon which heavy weights are laid. If desired to be at hand at all times it must have been kept for weeks or months under the compressing power. When wanted to be used as a tent, the compression should be effected by winding some thread or thin cord around a piece of clean, well-moistened sponge, removing the thread after the sponge has become thoroughly dried. Or, the sponge may be soaked in a solution of gum arabic before winding, and the tent afterwards smoothened into a proper shape by means of a knife. The winding is facilitated by transfixing the sponge by an awl which is afterwards removed. The tent should always be prepared with mucilage, and not with water, when intended to be used for dilating the canal of the cervix uteri, or any other part where moisture might induce premature expansion, i. e. before it can be properly inserted; also when it is desirable to avoid rapid dilatation, which sometimes causes considerable pain and uneasiness. When the tent can be introduced with facility, the mucilage may be dispensed with; and when it is to be introduced into any internal part it should be transfixed by a needle and thread for its withdrawal. (Dr. Batchelder, p. 285.)

URETHRAL CARUNCLES.—If these florid painful growths be removed by the knife alone, even if the piece of mucous membrane upon which they are seated be also removed, a permanent cure is seldom effected. The use of caustics is not attended with better results. The actual cautery alone will destroy these growths effectually. An iron of proper size and shape, adequately heated, may be employed, or the requisite degree of heat may be applied through the galvano-caustic wire. The latter method is especially useful when the caruncles extend up the urethra higher than the orifice, because you can introduce and apply the wire, before heating it by the transmission of the galvanic current. Apply immediately afterwards cold water and cloths soaked in it; and subsequently treat the ulcerated surface, after the slough separates, with very frequent applications of black wash, zinc lotion, or other surgical applications. Though this is the best mode of treating these growths, yet even this is not absolutely a certain method of cure, as cases now and then occur in which the growth will return in spite of all. Round the larger projecting tumour a number of small painful red spots of mucous membrane generally exist, and the removal of these is essential to the radical cure. The best local application to relieve the irritation and pain is an ointment made up of two drachms of the dilute hydrocyanic acid of the pharmacopœia to an ounce of lard. A bit of this ointment about the size of a pea, applied to the part three or four times a day, often relieves the pain more effectually than any quantity of opium administered internally, or than any other form of local anodyne. (Prof. Simpson, p. 209.)

UTERINE EXCITEMENT.—Bromide of potassium has a decidedly sedative effect upon the generative system. Dr. Simpson recommends its use in spurious pregnancy. Some time since Dr. Locock advocated its use in cases of epilepsy in females apparently having a monthly return. (Prof. Simpson, p. 181.)

VAGINITIS, *with Superficial Inflammation of the Cervix Uteri.*—Introduce every morning, with the assistance of the speculum, a good-sized pledget of cotton wool, well smeared over with tannin ointment, into the vagina, bringing the pledget in contact with the cervix. By means of a thread attached to it, this may be withdrawn by the patient, either in the evening or next morning, and the parts having been well washed out with cold water or weak alum water, a fresh pledget may be introduced. By a little practice patients soon learn to apply the cotton wool for themselves. (M. Foucher, p. 214.)

VOMITING OF PREGNANCY.—*Oxalate of Cerium.*—Cerium has a peculiar sedative and tonic action on the stomach, resembling in some degree the action of the salts of silver and bismuth. It is of all remedies the most generally useful in cases of obstinate vomiting from pregnancy. It does not invariably nor certainly act thus in these cases, but it is more certain than any of the remedies previously in use. The oxalate is the salt most readily procured. It may be given in doses of one or two grains three times a day, or oftener. (Prof. Simpson, p. 185.)

MISCELLANEA.

ANÆSTHESIA.—*Rectified Sulphuric Ether.*—Rectified sulphuric ether should be used as an anæsthetic agent, "to the entire exclusion of chloroform." This opinion is founded on the safety of its administration. In no case has death been known to result from its employment, although in very extensive use in the hospitals of America. The effects of sulphuric ether pass off sooner, and less vomiting, nausea, and headache follow its administration. A bell-shaped sponge, with a concavity large enough to admit the nose and mouth, is all that is required in its administration. The greatest disadvantage is the large amount required, viz., from four to eight ounces, which is the average quantity—and anæsthesia is not so speedily induced as by chloroform. (Dr. G. Hayward, p. 303.)

Medicated Vapours.—*Local employment of.*—Many diseases of the greatest importance, though perhaps constitutional in their commencement, and best treated by constitutional remedies, are in their termination strictly local, and no longer amenable to the class of remedies before employed. Such are cases of chronic bronchitis, chronic affections of the chordæ vocales, the eustachian tubes, and passages of the nose. In these cases local remedies are required, and are only not used on account of the great difficulty in getting at the parts; an impalpable powder of alum, gum, and morphia, may be inhaled or forced into or through these parts with considerable facility and much benefit. In one case of chronic bronchitis, the expectoration was reduced from a pint and a half to half a pint daily, with corresponding benefit to the patient. In many cases of this class much benefit is derived from the smoking of mercurial cigarettes, (a formula for which will be found at p. 305.) This principle may be further developed, probably with much success, in the treatment of hitherto intractable diseases. (Dr. J. B. Nevins, p. 305.)

Snake Bites.—A decoction or broth of the leaves near the root of the common male fern, polypodium filix mas, has been used as a secret remedy in Australia, as a specific for snake bites. Though the experiments hitherto made cannot be said to be satisfactory, it might be tried by medical men in the army, if nothing better is at hand. Probably a tincture would be more powerful. (Mr. Underwood, p. 278.)

Swollen Finger, *To Remove a Ring from.*—Wind a reel of cotton evenly round, beginning at the extremity of the finger, and bringing each coil into close apposition with the preceding, until the ring is reached; having then threaded a needle with the cotton, and passed it under the ring, carefully unwind the thread from the finger, the ring follows each coil as it is successively unrolled, and by almost imperceptible degrees is brought over the knuckle and removed. (Mr. E. Garraway, p. 310.)

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EDITED BY

W. BRAITHWAITE,

LECTURER ON OBSTETRIC MEDICINE AT THE LEEDS SCHOOL OF MEDICINE, ETC.

PART XLI.......JULY......1860.

UNIFORM AMERICAN EDITION.

NEW YORK:
W. A. TOWNSEND AND COMPANY.
1860.

C. A. ALVORD, PRINTER, 15 VANDEWATER STREET, NEW YORK.

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PRACTICAL MEDICINE.

&c., &c.

DISEASES AFFECTING THE SYSTEM GENERALLY.

Art. I.—ON THE FORMS OF CONTINUED FEVER.

By Alexander Tweedie, M.D., F.R.S., Physician to the London Fever Hospital, &c.

[It is almost needless to say, that up to within a comparatively recent period, all continual fevers were included in one class, under the generic term *typhus*. At the present time, however, careful observation of the course, symptoms, and pathology of continued fevers, lead us to divide them into several distinct forms. Much has been written upon this subject, but physicians who have had most opportunities of judging on the subject, agree now in this view of the question. The observations of Dr. Tweedie on this subject are particularly valuable, from his position at the head of the London Fever Hospital, and the great attention which he has for many years paid to febrile disease. We extract the following from the Lumleian Lectures, delivered by him, session 1858–9. We must premise, that he divides continued fevers into four classes: 1. Typhus; 2. Enteric, or Intestinal Fever, (a better term than typhoid); 3. Relapsing Fever; 4. Febricula. The term typhus is restricted to one form, characterized by early prostration, more or less prominent affection of the nervous system, but more especially by the invariable absence of any specific lesion.]

1. *Typhus Fever* originates in circumstances tending to impair the essential or vital properties of the blood, more especially overcrowding, defective ventilation, insufficient nourishment, and hence its prevalence in times of scarcity and famine. Its accession is marked by no special symptoms, but such as occur in many acute diseases—chilliness alternating with heat of skin; quickened pulse, succeeded by muscular prostration; more or less sensorial disturbance; and between the fifth and eighth day a peculiar morbillous-like eruption, not fading on pressure, and persistent; the duration of the fever being about fourteen, and seldom exceeding twenty-one, days. In fatal cases there is no specific lesion, congestion of the internal organs being the only change observed. If there be other lesions they are superadded or accidental.

2. *Enteric or Intestinal Fever.*—This form (known also by the term *typhoid*) is endemic; and supposed to be produced by emenations from organic matter. Its mode of invasion, slow and insidious, differs little from that of the other forms, except that there is almost invariably diarrhœa from the commencement, followed by gurgling in the right iliac fossa, and tympanitic distension. It is characterized, moreover, by an eruption of rose-coloured spots, visible about the eighth day,—often later,—first on the anterior aspect of the trunk, rarely on the face or extremities, coming out in successive crops, and fading or entirely disappearing on pressure; occasionally epistaxis, little comparative diminution of strength, and sometimes sensorial disturbance. This form is often protracted, seldom terminating before the third week, often lasting much longer. After death there is invariably alteration in the solitary and agminated glands of the ileum, which are enlarged, and more or less extensively ulcerated, according to the duration of the fever, with enlargement and softening of the corresponding mesenteric glands, and increased volume and softening of the spleen.

3. *Relapsing Fever.*—In this peculiar form the invasion is sudden, marked by irregular chills or prolonged shivering, succeeded by hot skin, severe

pain in the head and limbs, epigastric tenderness, sometimes vomiting (often accompanied with jaundice), and hepatic and splenic congestion. But the chief peculiarity consists in an abrupt cessation of symptoms from the fifth to the seventh day, so that the patient is apparently restored to convalescence. A few days afterwards, however, generally about fourteen days from the beginning of the attack, there is a well-marked relapse, or recurrence of the symptoms, which, after a profuse sweat, about the third day again disappear, leaving the patient exhausted, though soon recruited by a prolonged and invigorating sleep. This form is rarely fatal, and leaves no special lesion. It appears to be connected with famine and destitution, and to spread by contagion.

4. *Febricula.*—This is a mild variety of fever, characterized by its short duration and the mildness of the symptoms. It seldom lasts more than a few days, sometimes terminating within twenty-four hours, when it constitutes what has been called *ephemera*, or one-day fever. Its cessation is generally preceded by a critical sweat. It is seldom if ever fatal, unless from the supervention of acute local disease, which of itself may destroy life.

Now let me remark that these are not arbitrary but practical distinctions, recognised in our hospitals as well as in private practice, and occurring either sporadically, or as epidemic visitations. The records of the London Fever Hospital afford the most satisfactory mode of showing the comparative prevalence of each of these forms, in and around this metropolis.

ON ENTERIC FEVER.—[This form of fever is distinguished by its invariable anatomical character, specific lesion of the intestinal glands, and corresponding glands of the mesentery. It has been described under various names, as typhoid fever, abdominal typhus, enteromesenteric fever, gastric fever, ileo-typhus, and enteric fever. The last is to be preferred as being simple, and expressing the prominent destructive feature of the disease. Enteric fever seldom occurs after forty.]

It commences slowly and insidiously, so that the patient is often able to pursue his ordinary avocations, complaining only of undefined indisposition, such as irregular chills, loss of appetite, more or less headache, thirst, pain of limbs, and a degree of langour and lassitude for which he cannot account. In the course of a day or two, these symptoms increase, the pulse quickens, there is less inclination for exertion, the tongue becomes furred, the bowels disposed to be relaxed, sometimes with abdominal pain, the countenance altered in expression, the face at times flushed, and at length, from increasing weakness, the patient seeks the couch, or prefers to keep to bed entirely.

Towards night, there is more or less aggravation of the symptoms, accompanied with restlessness, inducing constant change of posture in the vain hope of finding relief; the sleep is disturbed and unrefreshing, the thirst more urgent, and the heat of the skin more pungent. Sometimes there is vertigo, or singing in the ears, or occasionally slight epistaxis.

Such are the symptoms of the early stage of enteric fever; and though to an experienced observer, they are sufficient to indicate the disease, there may be reasonable doubt as to its nature, until its characteristic eruption appears. This is seldom visible before the second week (generally from the eighth to the twelfth day) after the commencement of the symptoms, when, on close examination, a few small circular rose-coloured spots may be discovered, chiefly on the anterior and posterior aspect of the trunk, seldom on the face or extremities. As the characters of this specific eruption will be presently pointed out, I shall only observe that each spot is perfectly distinct, fades or entirely disappears on pressure, and after remaining visible for three or four days disappear, fresh spots coming out every two or three days, and undergoing a like process of eruption and decline. In this way successive crops of these spots appear until the conclusion of the disease. This characteristic eruption, however, as we shall presently see, is occasionally absent.

If the abdomen be examined, it will be found more or less distended and resonant; a sensation of gurgling in the right iliac fossa, often accompanied with tenderness, being generally perceptible.

The diarrhœa so commonly observed, as one of the early symptoms varies in degree; sometimes it is moderate, sometimes profuse and exhausting, and in some cases accompanied with blood.

If the case be mild and uncomplicated, these symptoms continue without marked variation till towards the middle or end of the third week, when a gradual abatement of the more prominent, especially of the diarrhœa, portends the approach of convalescence. The change, however, is very gradual. The pulse becomes slower; the alvine discharges less frequent and more consistent; the tongue more clean; the thirst abates; the heat of skin and restlessness disappear; the sleep is tranquil and refreshing; there is gradual improvement in strength; while the appetite for food returns, and is often so keen as to require great vigilance to prevent its too early indulgence, and consequent risk of convalescence.

But the progress of enteric fever does not always run so smoothly. On the contrary, when the stage of convalescence is daily watched for with great anxiety, all the symptoms may continue unabated, or some of them become even aggravated. Thus the headache, which usually passes away towards the end of the second week, may persist, and eventually may be followed by delirium, or by somnolence, gradually passing into coma, more or less profound; the diarrhœa may increase, or a sudden discharge of blood from the bowels may arise, which, if it do not destroy life, adds much to the danger: or, again, the intestinal ulceration may extend, inducing progressive emaciation and weakness, from which it may be difficult or impossible to restore the patient. Not unfrequently the progress of the fever is interfered with by some form of pulmonary complication. This may be open and easily detected; but in many cases, more especially when there has been early and severe disturbances of the nervous system, it assumes a latent form, and is recognised only by careful auscultation, which may reveal either pleurisy, bronchitis, or pneumonia, more or less diffused. Sometimes, and indeed not uncommonly in severe cases, there may be both cerebral and pulmonary disease co-existing; so that the fever may be said in its progress to have involved the most important internal organs. Under such extensive complications the issue is seldom doubtful.

In proportion to the intensity of the local complication, the general or febrile symptoms increase; indeed, a low form of secondary or symptomatic fever may be said to supervene. The pulse rises in frequency, and is weak and compressible; the tongue becomes dry, brown, shrivelled, and often fissured; the teeth and lips covered with brown or black incrustation; the emaciation progressive; the weakness day by day more marked; the evacuations passed unconsciously; the sacrum, hips, and other parts subjected to pressure, becoming inflamed and passing rapidly into gangrene; or pus may be deposited in different parts, more commonly in the joints.

It is unnecessary to observe that the ultimate issue, under such complications, must depend on the circumstances presented by each case: the degree or intensity of the local complication, and the stamina or power of the patient to contend against such a formidable disease. He may linger from day to day and eventually recover, or death may result from gradual exhaustion, seldom before the fifth or sixth week. Many cases are protracted to a much later period; indeed, there is scarcely a limit to the duration of complicated enteric fever. Or, lastly, the ulcerative process may gradually destroy in succession the coats of the intestine, and at length perforate the peritoneal covering; and thus the patient may be speedily cut off. Dr. Jenner notes that when death is the result of the general disease, the fatal termination always occurs before the thirteenth day. Local lession sufficient to account for death is always found after that date,—proving the natural duration of the general disease to be about four weeks.

On the Pathology of the Ulcerated Bowel of Enteric or Typhoid Fever.—[The first morbid change observed in the intestine, in enteric fever, is slight swelling of the mucous membrane covering Peyer's patches; to this rapidly succeeds the deposit of typhous matter, which soon presents the appearance of a deep yellow or brown sloughy patch; this is in a short time detached, leaving a cavity or ulcer on the inner surface of the intestine, known by the name of typhous ulcer. The following summary by Rokanitsky gives a clear view of the progressive changes of this intestinal lesion:—]

1. We find the lower third of the small intestine to be the seat of the lesion, the number and size of the ulcers increasing as they advance towards the cæcal valve.

2. The form of the typhous ulcer, when it corresponds to the infiltration and detachment of a larger Peyerian patch, is elliptical; it is round when it corresponds to a solitary follicle, or a rounded patch, or to the partial detachment of a glandular plexus; it may also be irregular or sinous, when corresponding to a partial detachment.

3. The size or circumference of the ulcer varies from that of a hemp-seed or pea to that of a half-crown.

4. The patches are placed opposite to the insertion of the mesentery, their long diameter being always parallel to the longitudinal axis of the intestine. The typhous ulcer never forms a zone; at least Rokitansky has only seen this occur once in many hundred cases.

5. The base of the ulcer is formed by a delicate layer of submucous tissue, which covers the muscular coat, a well-defined fringe of mucous membrane forming the margin.

In regard to the progress of typhous ulcers, there can be no question that the typhous deposit or infiltration affecting Peyer's patches may undergo the process of resolution. This happens in very mild cases in which the duration of the fever is short. There can be as little question that when ulceration has taken place, the ulcers cicatrize or heal.

This reparative process may be seen, in its different stages, in examining the intestine in cases in which death has unexpectedly occurred, either from some intercurrent local affection in the advanced stage of the fever, or during convalescence. When such an opportunity offers, some ulcers are observed to be superficial and not extensive, others are deep and spreading, while in one or more patches (generally near the cæcum) the ulcers appear smooth and polished, and covered with a thin transparent pellicle, continuous with the submucous tissue around the ulceration—an indication that the process of healing or cicatrization is going on.

For this important result, however, two conditions are essential; the one, the termination of the local process—that is, of the deposition of typhous matter, and the complete extinction of the typhous dyscrasia; the other, that the powers of the patient are adequate to withstand such a formidable local affection.

When such favourable circumstances concur, the following, according to Rokitansky, is the mode by which the process of cicatrization is effected. The fringe of mucous membrane which lies upon the base of the ulcer, gradually connects itself from without inwards, with the cellular tissue that invests the base, and, uniting with it, becomes paler and thinner. At the same time, the cellular layer becomes whiter and more dense, and finally converted into a serous lamina, the circumference of which is dovetailed between the muscular and mucous coats. The margin of mucous membrane is beveled off in such a manner that the union is imperceptible, while the line of union, as well as the mucous membrane, is so thinned down, that, at last, their villi appear to have been transferred to the serous lamina. The edges finally unite at one or more spots, and coalesce. Instead of the typhous ulcer, we find, in proportion as the above process is effected, a slight depression on the inner surface of the intestine, due to the thinning of the mucous membrane, and the connexion with a thin cellular layer of denser structure; or we find a spot at which the mucous membrane is more firmly attached and less movable, in the middle of which, by oblique light, we may often discover a smooth remainder of the serous lamina, of the size of a millet-seed; or we may discover a spot at which the mucous membrane is more tense, void of plicæ or folds, smooth, less vascular than the surrounding portion, and particularly less villous. Such cicatrices have occasionally been observed thirty years after the attack of fever. It is singular, and characteristic of the typhous ulcer and its cicatrix, that it never induces diminution of the calibre of the intestine.

In examining these ulcers from the cæcum upwards, we find the healing process in different stages. In some it has just begun; in others, it is going on partially; in others, again, it has been completed: so that the same course is apparently followed in the healing as in the development of this lesion, the cicatrization being either farthest advanced or completed in the patches nearest the cæcum, and in a proportionably less advanced state as we trace the patches upwards towards the jejunum.

Perforation.—In the destructive progress of the typhous ulcer, the mucous, areolar, and muscular coats may be successively laid bare, so that the floor of the ulcer is formed by the thin transparent peritoneum alone. In some cases this membrane is perforated, so that the intestinal contents escape into the peritoneal cavity, giving rise to acute peritonitis.

The perforation is always found in the centre of an ulcerated patch, and most commonly in the lower third of the ileum; generally it is very small, not larger, perhaps, than a pin's head, unless there be a sloughing ulcer, in which case the aperture is large. There may be a single perforation, or there may be two or more.

These perforations happen. as I have already remarked, not in the most severe cases of typhoid fever, but more commonly when the disease is of moderate severity—when the ulceration is proceeding slowly, but surely, as in the cases of atonic ulcers described by Rokitansky.

As to the period of the fever when this lesion usually takes place, it seldom happens before the middle period—towards the end of the second week; but it may occur at any time between the second and the eighth or ninth week.

I have known it to take place more than once, when the patient was supposed to be well advanced in convalescence, walking about and enjoying food. The possibility of such an accident renders it imperative on the medical attendant—fearless of the imputation of sordid motives—to watch with anxious care the period of convalescence in this dangerous disease.

When the convalescence is protracted, indicated by the general and popular phrase, that the patient does not get on—does not mend—does not gain strength—be assured that Nature has not yet finished her work of cicatrization, and that an atonic ulcer has not healed, and is consequently pursuing a non-reparative course, which may end in erosion or perforation of the bowel, and the almost certain death of the patient. There may be even appearances of such improvement in the patient's general condition as to throw the most cautious and experienced practitioner off his guard. The nutritive powers may be fairly active, the patient may be gaining flesh, the alvine evacuations natural, and even formed. But with all these advantageous circumstances, I have known intestinal perforation take place, and destroy life in a few hours.

These, I admit, are exceptional cases, the perforation, in the great majority, happening in the earlier or more acute stages of the fever.

It is not easy to come to a certain conclusion as to the comparative frequency of this lesion. Louis met with it in eight out of fifty-five cases. In the last ten years there have been in the Fever Hospital, as far as I can trace, twenty examples of perforation out of the total admissions (1820). Out of these, *fifteen* were males, and *five* were females. The age of the youngest was eleven; of the oldest, forty-five. The preponderance in males is striking. In my private practice, extending over more than thirty years, I have met with it eight times..

On Typhus Fever.—[Typhus is distinguished pathologicaly from enteric fever by the entire absence of any specific local affection (unless the mulberry rash can be so called). The importance of being able to distinguish between these two forms of fever cannot be over-estimated, as they differ in their symptoms, duration, and in many respects in their treatment. In short,] Typhus fever is pre-eminently the type of a blood disease—the fever poison acting primarily on the blood, and secondarily on some organs or parts of the body, though after death no trace of structural change can be detected; differing in this respect, from enteric fever, in which, as we have seen, there are invariably well-marked special lesions.

It appears, besides, that the two fevers differ in their etiology. Typhus often prevails epidemically, from unknown causes, and is moreover the almost invariable accompaniment of famine and destitution, more especially in over-crowded and ill-ventilated dwellings; and when once engendered, from whatever cause it may have been induced, it spreads by contagion regardless of age, sex, or local circumstances.

Enteric fever, on the other hand, appears to have less of an epidemic, more of a local character; and though its causes are most obscure, it has been supposed to have an apparent connexion with ill-constructed dwellings and malarious exhalations.

[On the first onset of the disease, or during the period of incubation, there are various symptoms, as]

Irregular chills or shivering, nausea, disrelish of food, thirst, giddiness, perhaps slight pain in the head, somewhat accelerated pulse, with languor and muscular aching—these are felt for days perhaps before the fever can be said, in popular language, to be *formed.* As the disease progresses, these sensations become intensified; there is a visible change in the countenance, the patient is agitated and restless, while the symptoms of general disturbance alluded to are more pronounced, and recur in paroxysms of uncertain duration, but chiefly towards evening, or in the night, which is spent in wakefulness, or disturbed sleep.

But these symptoms are common to other forms of fever, and even to acute diseases in general, so that it is by their progress and further development that we are enabled to form our judgment as to the precise nature of the disease they indicate.

1. There is the peculiar distinctive eruption, known by the term *typhus rash;* or, from its resemblance to the efflorescence of measles, it has been called *mulberry* or *morbillous.* When this eruption is present, it indicates, at once, to the experienced eye, this form of fever.

A comparison of this rash peculiar to typhus, with the rose-spotted eruption of enteric fever in the living body, will enable you at once to recognise the one from the other.

Let me draw your attention more particularly to this rash.

The typhus eruption appears from the third to the seventh day of the fever, the spots of which it is composed varying in size from a point to three or four lines in diameter, having an irregular outline; sometimes they are few in number, but more commonly they are numerous, the larger spots being formed by the coalescence of smaller ones. At first they have a dusky pinkish hue, fading or partially disappearing on pressure, and when the finger is passed over, they give the sensation of being slightly elevated. After a day or two they assume somewhat of a brick-dust colour, scarcely fading on pressure, the tint gradually becoming more and more dark, and not disappearing when even firmly pressed. Another distinctive peculiarity is, that each patch, or cluster of spots, remains from its first appearance till the termination of the fever, differing from the rash of enteric fever, in which, as I have already pointed out, the spots not only disappear on pressure, but last for a few days only, and are succeeded by a fresh eruption, so long as the fever continues.

In some cases the typhus rash, though well developed, is less prominently marked. Many of the individual spots are pale, undefined, and run into each other, giving the portions of the skin covered by the rash a mottled appearance. There is, however, considerable modification in the colour, as well as copiousness, of this rash, according as the fever is more or less severe, or in some respects, according to the colour and texture of the patient's skin. It will generally be found that the abundance, and especially the deep colour, of this eruption, are in proportion to the severity of the symptoms, so that it would appear that the colour has an obvious bearing on the gravity of the fever.

In regard to the regions of the body on which this eruption appears, when it is abundant, it covers both the trunk and the limbs, coming out first on the anterior aspect of the trunk, and spreading to the extremities, being rarely, however, seen on the face. When less abundant, it is limited to the chest and abdomen, and, as a general rule, it seems to reach its maximum within the first three or four days after it first becomes visible.

It may be inquired, is this distinctive rash always present in typhus? It is occasionally absent. And here statistical facts are valuable in determining the relative proportion of cases, in which, though looked for, it was not found. Of one hundred and fifty-two cases, of which accurate notes were taken and recorded in the journals of the Fever Hospital by Dr. Jenner, this eruption was present in one hundred and thirty-six, and absent in sixteen. It has been generally observed, too, that when the rash is absent, the fever is mild and of short duration, unless some intercurrent or secondary affection alter the character of the disease. Of the sixteen cases in which it was absent, thirteen were between the ages of four and fifteen, and the remaining three between sixteen and twenty-two. It also appeared, that of the whole number (one hundred and fifty-two), seventy-six (exactly a half) were more than twenty-two years old, and twenty-one between the ages of sixteen and twenty-two inclusive. The eruption was, therefore, present in every individual more than twenty-two years of age; it was absent in three, whose ages varied between sixteen and twenty-two; and absent also in thirteen (or about a fourth) of those fifteen years of age and under, and of these thirteen, there was only one whose age exceeded thirteen.

From these details given by Dr. Jenner, he infers, that of one hundred individuals of all ages with typhus fever, we may expect to find the rash absent in twenty-five (or one-fourth) of those under puberty; from fourteen (or about one-seventh) of those under manhood; but from none above twenty-two years of age.

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In the Edinburgh epidemic (1838–9) the mortality of cases with scanty eruption was one in eight; where the rash was abundant it was one in four; the duration of the fever being also between one and three days longer in the one than in the other.

As it is important to keep in view the distinguishing characters of the typhus eruption, let me restate them:

1. It consists of a congeries of spots, which cohere in the form of patches on the trunk and limbs (seldom on the face), of a brownish or mulberry hue, giving a diffused or mottled appearance to those parts of the skin on which it comes out; fading, but not disappearing, on pressure; and differing in those respects from the bright rose spots of enteric fever, which are isolated or distinct, and entirely vanish on firm pressure. 2. The typhous eruption is permanent or persistent—that is, it continues from its first appearance until the fever terminates; differing also in this respect from the rose-spotted rash peculiar to enteric fever, which, after running through successive changes, disappears in a few days, and is succeeded by a fresh eruption of spots, which go through the same changes, crop after crop, until the cessation of the fever. 3. Though this eruption may be discovered by careful examination in a large proportion of cases, it is occasionally absent—its absence, however, being rarely observed in adults, more commonly in children; and it will be found that its presence or absence has an important

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1. It consists of a congeries of spots, which cohere in the form of patches on the trunk and limbs (seldom on the face), of a brownish or mulberry hue, giving a diffused or mottled appearance to those parts of the skin on which it comes out; fading, but not disappearing, on pressure; and differing in those respects from the bright rose spots of enteric fever, which are isolated or distinct, and entirely vanish on firm pressure. 2. The typhous eruption is permanent or persistent—that is, it continues from its first appearance until the fever terminates; differing also in this respect from the rose-spotted rash peculiar to enteric fever, which, after running through successive changes, disappears in a few days, and is succeeded by a fresh eruption of spots, which go through the same changes, crop after crop, until the cessation of the fever. 3. Though this eruption may be discovered by careful examination in a large proportion of cases, it is occasionally absent—its absence, however, being rarely observed in adults, more commonly in children; and it will be found that its presence or absence has an important

bearing on the character of the fever—its absence more especially indicating a mild disease.

2. *Cerebral Symptoms.*—In the early stage of typhus, *Headache* is one of the most constant symptoms, indeed rarely absent. It may be so slight as not to attract the patient's notice; but sometimes, for the first week or ten days, it is severe and persistent, after which it generally abates gradually, and finally disappears towards the end of the second week; or, should it continue longer, it comes on at intervals only—in the evening, perhaps, or during an exacerbation of the fever.

Delirium.—The intelligence is seldom disturbed in the milder cases. In the more severe, there is transient confusion, soon lapsing into delirium, perceptible at first only on awaking, or in the night, but becoming more constant as the fever advances. The character of the delirium varies; it is sometimes noisy and violent (typhomania), more often low and muttering, with tendency to somnolence; in other cases, it partakes of the character of delirium tremens—the form usually observed in spirit-drinkers, or in persons harrassed with care.

This symptom (delirium) is rarely noticed before the seventh or eighth day. It may appear, however, earlier or later; but at whatever period it comes on, it continues until the termination of the fever.

[There is prostration of the whole muscular system; the facial muscles lose their tone, and give a peculiar expression to the face. There are various affections of the organs of sense, as deafness, imperfection of vision, loss of taste.]

Tongue.—In the beginning of the disease, the tongue is covered with thin white mucus, which in its progress becomes more or less brown, thick, and tenacious. In the more grave cases, this incrustation is still darker, becoming at length almost black or fuliginous, while the tongue itself is shrivelled and fissured, and sometimes coated with blood.

The teeth and lips are incrusted with a similar exudation of dry, offensive mucus, or with bloody sordes, which evidently issue from the mucous surfaces,—a kind of local hemorrhage, in fact,—as blood-discs may be discovered on placing a portion of this sordes under the microscope.

With this dry, parched state of the tongue and fauces, there is often some difficulty in thrusting forward the tongue, or in swallowing, especially when the first efforts are made.

Secondary Affections.—The secondary affections that occasionally arise in typhus often become more important than the primary fever, and not unfrequently determine the issue of the disease. They are, however, incidental only, and in no other way are they to be regarded in connection with the primary or antecedent fever. They are moreover, due rather to local congestion than to inflammation, thus differing from the secondary affections in enteric fever, in which, if the views of Rokitansky and other pathologists of the German school be adopted, there is a deposit of typhous matter in organs—the lungs, intestinal and mesenteric glands, more especially.

[From the earliest times it has been a common opinion, that the period of the subsidence of a fever is accompanied or preceded by a crisis. This, however, is not the case with enteric fever: in typhus fever, on the other hand, a crisis is frequently observed.]

A change in the aspect of a case of typhus, favourable or otherwise, is sometimes preceded by sweating more or less profuse and persistent, sometimes by copious deposit of lithates in the urine, by spontaneous diarrhœa, some form of cutaneous eruption, or occasionally by the formation of an abscess. And this theory receives support from comparison with other forms of fever—the periodic and relapsing, for example,—in which there is an abrupt termination of the febrile disturbance after a profuse and apparently salutary sweat.

When we take into consideration, however, the large proportion of cases in which the convalescence slowly, and, as it were, imperceptibly proceed, without any evident critical event, we cannot resist the conclusion, that, as a general rule, the fever leaves the body without any marked evacuation, and that when a crisis does occur, it is to be regarded as an exceptional, though favourable circumstance.

In examination of the bodies of persons who have died from typhus fever, without secondary or intercurrent local disease, in a considerable proportion, after the most careful search, no trace of disease in any organ or tissue can be discovered. In other instances, the only deviation from the normal state is congestion of internal parts, more especially of the mucuos surfaces of organs.

Even when secondary affections have arisen and had an important influence on the severity and progress of the fever, or proved even the more immediate cause of death, they do not always leave corresponding effects on the organs or parts affected; so that it is more than probable that their function rather than their structure has been implicated in the febrile conflict.

There are no special lesions that can be said to be peculiar to this form of fever. The most common, and perhaps the most constant changes are due to congestion of organs, more especially of their mucous surfaces; but that even these changes are not an essential or invariable element of typhus is proved by the fact, that in a large number of bodies, after the most careful examination, no trace of disease in any organ or tissue can be discovered. When secondary affections arise in its progress, as already stated, they are to be regarded as incidental, having, nevertheless, always an important bearing on the progress and result of the fever, and as being not unfrequently the immediate cause of death.

Changes in the Blood.—The blood, which has from remote ages been supposed to undergo marked changes in its physical properties in fevers, has been, in recent times, subjected to chemical analysis. These investigations show that several of its constituent principles become altered, either primarily or secondarily.

I need scarcely mention that healthy blood contains, besides water, four essential constituents—*corpuscles*, *fibrine*, *albumen*, and *certain saline matters.* The fibrine, albumen, salts, and water, constitute the *liquor sanguinis*—a transparent colourless fluid, in which the corpuscles, red and white, are suspended and carried along in the living vessels.

Again, when blood is drawn from the body, as in ordinary venesection, and allowed to remain at rest, it separates into crassamentum and serum.

The *crassamentum* consists of fibrine, in which the red and white corpuscles are blended together with a small quantity of serous fluid.

The *serum* differs from the liquor sanguinis in one respect only—that it does not contain fibrine; it contains, however, albumen, which gives it the property of coagulating by heat; and if a degree of heat sufficient to decompose the animal matter be applied, the residuum becomes converted into earthy and alkaline salts.

In regard to the proportional or mean amount of these constituents, Becquerel and Rodier found that in 1000 parts of healthy blood there are of

Water	791·1
Fibrine	2·2
Corpuscles	127·2
Albumen	70·5
Fatty matters	1·6
Extractive matters, salts, and loss	7·4
	1000·0

So much for the constituents of *healthy* blood, the outline of which now given may be useful for comparison with the changes which it has been observed to undergo in fevers. But it is to be understood that the observations to be made have reference to the blood in both forms of fever, (enteric and typhus); the difference in either being trivial.

The blood, in fever contains a smaller amount of fibrine than in health, and is often deficient also in albumen. Hence, when blood-letting has been resorted to in the treatment, and more especially when performed in the advanced stage, it often coagulates imperfectly, and crassamentum being soft and diffluent, and rarely exhibiting the buffy coat. It is likewise more liable to putrefy than healthy blood, or than blood abstracted in other diseases. This deficiency in the fibrine of the blood in fevers, when uncomplicated, tends to distinguish it from that in acute inflammations, in which there is an increased amount of fibrine, in proportion to the intensity of the disease.

This gradual defibrination of the blood in fevers cannot be ascribed either to bloodletting (if practised in their treatment) or to other antiphlogistic measures, since in other acute diseases those circumstances (bloodletting and other depletives) do not appear to be followed by diminution—nay, rather by augmentation—of the fibrine. It has, moreover, been ascertained, that if a local inflammation spring up in the progress of fever, the amount of fibrine becomes increased, though it is still less than in ordinary inflammation, the fibrine condition apparently influencing the quantitative amount of this constituent, when an intercurrent of local inflammation has arisen. Besides, as soon as the period of convalescence arrives, the fibrine begins to increase, and before the prescribed nourishment could be so elaborated as to contribute to this change, and which appears to proceed with advancing convalescence.

With this diminution of fibrine, it has been shown by the experiments of Andral and Gavarret, that in fevers a considerable increase in the corpuscles takes place, forming another point of contrast with the blood in primary inflammations, in which there is a slow but certain diminution in the corpuscles.

It may, therefore, be concluded—first that in continued fevers, the fibrine gradually decreases in proportion to the duration of the fever, never exceeding in amount the normal standard, but being much more frequently below it; and, second, that the corpuscles become increased in amount.

It appears, too, by these researches, that the residue of the serum and solid constituents generally, are little, if at all, diminished in amount, notwithstanding the statements made by some writers that, according to the duration and intensity of the fever, the salts of the blood, especially chloride of sodium, become decreased, a notable increase of water at the same time taking place.

On this doctrine,—the supposed origin of fevers in morbid changes in the blood,—I may observe, that when we reflect that the blood, which was aptly termed by Borden *fluid flesh*, has the same proximate principles as the solids of the body, that it is organized, and apparently endowed with vitality—the doctrine that it is liable to disease rests on a more certain foundation than mere vague hypothesis. Whether the fever poison acts primarily on the blood, producing changes in its sensible properties, or whether, as some are inclined to believe, the changes are secondary, it is scarcely worth while to inquire. Indeed, we have no data to guide us in this inquiry, except the statements of Dr. Stevens, who affirms that he and other practitioners in the West Indies observed, that in persons exposed to the fever-producing malaria, the blood underwent marked changes, even before the symptoms became developed.

Again, I am inclined to believe, that the different forms of fever arise from distinct poisons, however induced; that the true typhus poison acts on the blood alone; that enteric fever is produced by a poison, *sui generis*, which has a compound action on the blood and on the solids, inducing special lesions, with which, as we have seen, this type of fever is invariably associated; and that it is more than probable that the relapsing fever, to be next described, has also its origin in a distinct poison, though from its comparative infrequency, little or nothing, as yet, has been satisfactorily made out as to its etiology. But I know no department of the pathology of fevers more important and more worthy of further elucidation than their connexion with morbid states of the blood.

Diagnosis.—The importance of forming a correct diagnosis between the two forms of fever—enteric and typhus—cannot be over-estimated, the one differing from the other in so many different prominent circumstances, and more especially, as we shall presently show, in their treatment.

In order to arrive at a correct conclusion as to their differential diagnosis, it will be necessary to institute a comparison of the individual symptoms of each form.

1. It has been pointed out, that in typhus fever the accessions of the symptoms is more sudden than in enteric fever, in which it is generally gradual. slow, and insidious.

2. The heat of skin is more constant and more marked in the early stage of typhus; during the exacerbations, it is often pungent and accompanied by flushing. In enteric fever the heat of skin is moderate, or it may be entirely absent.

3. The brain symptoms, especially delirium, intellectual dullness, and stupor, are more strongly and early developed in typhus. In enteric fever, the cerebral symptoms appear later—usually not until the first week—and slowly and gradually increase in severity.

4. In typhus, the bowels are generally confined, so as to require the occasional administration of aperients. There is seldom diarrhœa, and if the bowels act more frequently than usual, the evacuations are watery. There is, besides, no gurgling on pressing the region of the cæcum, and rarely meteorism. In enteric fever, the evacuations, on the other hand, are generally frequent and watery, and accompanied with griping. The belly is tense, generally inflated, and a gurgling noise may often be detected by pressure over the right iliac fossa.

5. Epistaxis is rare in typhus; not uncommon in enteric fever.

6. Intestinal hemorrhage is very rare, almost unknown, in typhus; frequent in enteric fever.

7. Emaciation is much less marked in typhus than in enteric fever; in the latter when protracted, it is often extreme.

8. The cutaneous rash is quite dissimilar in the two diseases. In typhus it is dark and mottled, often extensively diffused over the skin, unchanged by pressure, and persists throughout the disease. In enteric fever, the rose spots are bright, isolated, slightly raised, fade or entirely disappear on pressure, and are found chiefly on the chest and abdomen. They last for a few days, and are succeeded by a fresh crop, which goes through a similar course until the termination of the fever.

9. The anatomical characters (or leisons) are distinctive. In typhus, Peyer's patches and corresponding mesenteric glands are in their natural condition. The vessels and sinuses of the brain are more generally gorged with dark blood. The lungs exhibit more frequently evidence of congestion, especially of the posterior and inferior portions, in typhus than in enteric fever.

In enteric fever, the alteration in Peyer's patches and corresponding mesenteric glands form the great distinguishing characteristics of the disease. The spleen is more often enlarged and softened. Ulceration of the pharynx and œsophagus, and softening of the mucous membrane of the stomach, are more common than in typhus.

10. The duration of the two forms may also serve as diagnostic marks. In typhus it is generally less protracted, terminating in recovery or death within the second week, much more frequently than in enteric fever. We have stated that the average duration of typhus does not exceed fourteen or fifteen days; and if prolonged beyond that period, it is generally owing to some secondary affection or complication. In enteric fever, the average duration is longer,—from twenty to thirty days, and extending, as has been shown, in some instances, to forty, fifty, or sixty days, or even beyond.

In children there is more difficulty in determining the diagnosis of the various forms of febrile diseases than in adults. Let me mention a few of the more prominent.

1. Disorders of the gastric system attended with fever are very common, especially among children of the poor, and often simulate enteric fever; or, perhaps, it more frequently happens that the latter is mistaken, and treated as an acute gastric affection by irritating purgatives. If, however, the symptoms that are so peculiar to enteric fever be kept in view, the distinction is less

difficult. For example, in enteric fever, the invasion is not sudden, nor can the symptoms be traced to dietetic mismanagement; the prostration is more marked, the child is more restless and agitated, and often delirious in the night; the stools are frequent and watery; the belly tender and inflated; the splenic region dull; and, above all, the characteristic rose spots, if present, and the prolonged duration of the symptoms, are sufficient to indicate that the child is suffering from enteric fever.

2. This form of fever (enteric) may be distinguished from infantile gastric remittent by the irregularity of the paroxysms in the latter; by the absence of rose spots; by the large, often solid evacuations, requiring active aperients for their dislodgement; by the occasional admixture of undigested aliment in the secretions expelled from the bowels; and by decided improvement resulting from a course of treatment that would certainly aggravate, if it did not hopelessly augment, enteric fever.

It is right, however, that I should mention that some physicians of the present day disbelieve in the existence of this infantile gastric remittent fever, without affection of Peyer's patches, which they state to be a constant element in this common infantile disorder. I cannot adopt this conclusion—at least, I am unwilling to do so—until there be better evidence by accumulated observations of the appearances in the intestine, when this form of gastric fever proves fatal, which, I may observe, rarely happens—a circumstance to be regarded as another distinctive character, and showing clearly, to my mind, the non-identity of the two diseases.

3. Another disease of childhood, which may be difficult to distinguish from enteric fever, is meningitis, and more especially that form which occurs in strumous children—tubercular meningitis. In this affection, the symptoms from the very commencement indicate severe cerebral disease—violent pain in the head, flushing, aversion to light, and as the disease advances, drowsiness, delirium, dilated pupils, and, finally, deep coma, sometimes alternating with convulsions. There may be sickness, and perhaps vomiting, but the bowels are generally confined. The rose spots, too, so characteristic of enteric fever, are absent.

If we compare these symptoms with those of enteric fever, it will be found that the cerebral symptoms are much less prominent in the latter, at all events, in the earlier period; that there is seldom sickness or vomiting; almost invariably diarrhœa; meteorism; often rose spots; dry, fissured tongue; and, when the fever is protracted, exudation of sordes on the lips and teeth; progressive and often extreme emaciation; sloughing of parts subjected to pressure; and death by gradual exhaustion, rarely, however, preceded by convulsions. The duration of the two diseases is also dissimilar. Meningitis terminates either in recovery or death within the third week, while enteric fever may be protracted beyond the thirtieth or fortieth day.

There is less difficulty in distinguishing cases of infantile typhus. The absence of gastric symptoms, the less pronounced cerebral affection, the less pungent heat of skin, the morbillious eruption, if present, the propagation of the disease by contagion, and its comparatively short and definite duration, are sufficient to distinguish typhus fever from other acute disorders of infancy and childhood.—*Lancet, Jan. 7, Feb.* 4, *March* 17, 31, *and April* 14, 1860, *pp.* 3, 108, 265, 313, 365.

2.—ON ZYMOSIS.

(Being an abstract of a paper read by Dr. Richardson, before the Epidemiological Society.)

In commencing, the author first considered the process of fermentation as out of the body. He dwelt on the different views which have been held as to the nature of this process, explaining that they divided themselves into three groups—the vital, the physical, and the chemical. In regard to zymosis or ferment *in* the body, the view generally held was linked with the idea of the vital character of the fermentation process. Having at some length sustained these positions, the author passed on to notice the objections which might be urged against the vital hypothesis, and then presented a reading of the subject which, resting on a basis purely chemical, explained much that was obscure and conflicting. He did not oppose the idea of zymosis; on the contrary, he gave to it a direct affirmative; but he argued in reference to three points:

1st. That the virus exciting the fermentation was simply an albuminous product.

2nd. That the virus was not by its *presence* the cause of the symptoms, but that the symptoms of the epidemics or zymotics were due to the presence of new chemical products resulting from the new chemical changes.

3rd. That the reappearance in some cases of the virus as an excrete was a necessary result, but that the origin of the virus was purely local.

Dr. Richardson next passed on to show that if the views he had advanced were tenable, they would explain many points which otherwise are all in confusion, would link certain diseases to the zymotic which are not at this moment ranged in that list, and would yet, at the same time, reduce the verbal list of zymotics to a few units.

In the first place he, urged that the theory of zymosis, as he explained it, disclosed the reason of the analogy which exists between diseases acknowledged to be communicable and certain other diseases which are not considered communicable, such as dissecting-room cholera. In the next place, it explained the reason why a small, equally with a large, quantity of poison introduced into the body excites the same intensity of effect. Thirdly, the theory suggested an alliance between diseases arising from the absorption of poisons and diseases produced by changes occurring in the body spontaneously, such as rheumatic fever and tetanus. Fourthly, it explained why the majority of zymotic diseases got well of themselves. If the poisons of these diseases were vital, and increased or multiplied in the body, the reproduction would last so long as the material for the continuance of the reproduction were present; but if the process were simply chemical, as he supposed, there was a direct reason why the diseases ran a limited course, inasmuch as chemical changes, having no independent continuance when their causes are removed, cease necessarily after a time, together with the symptoms excited by them during the period of their operation. Lastly, on the theory suggested by the author, an immense field was opened for direct experimental inquiry. Here scientific researches might take two directions: the one towards tracing the actions of known poisons on animals, and exciting diseases analogous to the zymotic diseases after the synthetical method; the other by ascertaining whether diseases so excited could be transferred to subjects previously unaffected.

The point which attracted most interest in the paper was that which had reference to tetanus. This disease Dr. Richardson claimed to be essentially zymotic. The theory of its production in traumatic cases is that the wound, in the process of healing, secretes a special albuminous product, which has the property of a ferment. This substance absorbed into the body excites new chemical changes, and as a product of these there is developed an alkaline or alkaloidal substance, having properties analogous to strychnine. Whether the patient shall or shall not recover depends, therefore, on the circumstance of this produced poison being or not being eliminated from the system by the excretions before the tetanic symptoms excited have progressed to a certain degree. If the symptoms are extended, or are moderate, or are moderated by medicinal means, the poison may be eliminated, and the patient may recover, or the reverse. In regard to tetanus, two experimental questions were open for solution—viz,: Will the secretion form a wound in a tetanic patient, introduced by another wound into a healthy animal, excite tetanus? Secondly. Are the excretions, say of the urine, of a tetanic patient capable, like strychnine in solution, of exciting tetanic spasm in a susceptible animal.—*Lancet, Dec.* 3, 1859, *p.* 565.

3.—ON FEVER AND INFLAMMATION.

By WILLIAM ADDISON, M.D., F.R.S., &c.

The Fluid or Plasma of the Blood.—The plasma is a very complicated fluid. Its chief constituents are water, fibrine, albumen, colourless corpuscles, volatile matter, fatty compounds, and salts. In its passage through the capillary vessels it permeates all organs and tissues; and at various points of the circulation, the parenchymatous depurating organs remove from it those substances, which, if retained in it, would injure its qualities. We find that numerous soluble compounds—substances, for instance, which have been taken as food or drink, and poisons received into the stomach—are absorbed into the plasma, whence they may be again eliminated or removed, changed or unchanged by the depurating organs—the organs of secretion.

Many of the common poisons have a local action as irritants, and also a remote or special action upon some parenchymatous organs; other poisons exhibit only the remote or special action. Cantharides has a special action as an irritant to the stomach; it also affects, remotely, through the plasma, the urinary organs. Morphia, taken by the stomach, affects specially the brain, through the plasma. Digitalis, taken by the stomach, affects the heart; strychnia the spinal marrow, through the same medium.

In experiments on animals with prussic acid, it has been found when death took place within a few minutes only, that the odour of the poison could be perceived in the cavities of the body; and again in other experiments, where preparations of antimony were injected into the blood by a vein in the thigh, the metal has been found in the coats of the stomach.

From these and numerous other facts, which it is unnecessary to particularise, we learn that poisonous substances, taken by the stomach, enter the plasma or mingle with the fluid of the blood; and thereby, the poisoning, not of all, but of some one parenchymatous organ ensues, determined by the nature of the poison, and the special qualities of the parenchymatous elements of the different organs. But, if we compare the actions of these common poisons received into the body through the stomach, with the action of aërial poisons inhaled by the lungs, important differences will be found. In the former, the symptoms do not begin with fever, nor is any contagious matter generated in the blood; whereas, both these phenomena are distinguishing features of the latter. Here, then, we have a ground for our argument, that the two parts of the blood have distinct pathological relations; for poisons taken by the stomach exhibit themselves by symptoms which proceed from some local parenchymatous organ, and they do not cause the production of a contagious matter in the blood; whereas poisons inhaled from the atmosphere by the lungs very often do so. Yet both species or classes of poisons act through the medium of the blood.

In 1832, numerous cases of cholera, then epidemic, were treated by saline injections, thrown into the blood in large quantities, not only without hurt to the patient, but in some instances with great apparent benefit. Among many others, there is a very remarkable case reported in the 'Lancet' (vol. ii., 1831 and 1832), in which five gallons of saline liquid were injected by a vein, in four days.

The first injection was to the amount of nine pounds and a half; it was thrown into the circulation in the space of eighteen minutes. A few hours afterwards an additional ten pounds of the saline fluid were injected, with four ounces of albumen. This was at one of the clock on the morning of the 29th May; at half-past seven the same morning the injection was repeated to the amount of ten pounds, with the addition of ten grains of sulphate of quinine; at half-past eleven the injection was repeated to the amount of ten pounds. On the second June, at half-past four in the morning, six pounds and a half of the saline fluid were injected, with six drops of a solution of morphia; and on the 19th of June the patient is reported to have left the hospital, well.

If we look around, we may see in all classes of society individuals of various conditions of body. We may contrast the florid complexion with the pale thin face; tissues laden with fat with the spare habit; those who luxuriate in good living with those existing on bread, potatoes, and water. Persons in each of these classes may be following their daily avocations in good health, yet the blood fluid, we know cannot be the same in all of them; nor can it be the same or have the same qualities in the same person, if he greatly alter his diet, his food, and drink. In the chemistry of the blood, there are scarcely two analyses of it alike; and the milky serum sometimes seen in blood after venesection has been referred to articles of diet, or to the recency of a meal, which has for a time changed the qualities of the plasma.

These facts support or coincide with our argument that the blood must be regarded in respect to its pathology, as consisting of two distinct parts; and of these, that the plasma, or fluid, holds a position of different or inferior rank to the red corpuscles in the functions of life. It furnishes elements of growth and reparation to its containing vessels, the common tissue; and it is the medium in which the corpuscles swim; but it varies, within certain limits, in qualities and composition, from day to day, according to the nature and abundance of the food and drink; nay, further, it appears that it may vary, in quality and composition, from poisonous substances taken into the stomach; so as to occasion symptoms of poisoning in some local parenchymatous organ, before any of the symptoms of fever appear, and without evidence of the generation of any contagious matter in the blood.

Distemperatures or Dyscrasies of the Plasma or Fluid of the Blood.—There are varieties or changes of health—moods, tempers, and feelings—dependent upon qualities of the blood, which are temporary, multifarious, and fleeting. They are cured by attention to diet and the excretions; by good living in those who have fared badly, or are half starved; by abstinence and physic in those who have abused the good things of the table. These changes in the moods and feelings of the individual, have all the character of dependence upon that part of the blood which is quickly replenished and altered by diet—namely, the plasma.

Also there are numerous forms of inflammation which are uncertain and multifarious—dependent upon qualities of blood occasioned by unwholesomeness of diet, privations, and neglect of the functions of the depurating organs—the skin, kidneys, and bowels. Sometimes the simplest wounds fester and ulcerate. Eruptions, pustules, or boils break out in the skin; or the slightest external injury provokes chronic and ulcerative forms of inflammation without fever, comprehended in the terms scrofula and scurvy. For example:—

Sailors at sea are dieted on salt provisions; and in hot climates are accustomed to tramp about the ship without shoes or stockings. Their feet and legs are consequently bitten by mosquitoes, and, when the men have been a considerable time on salt diet, and in the hot climate, the wounds ulcerate. The ulcerations become larger every day, and will resist all kinds of local treatment, so long as the diet remains unchanged. But, upon a change of diet, and cruising in a cooler latitude, the ulcers heal spontaneously.

Under analogous conditions—of unwholesome or insufficient diet or confinement—fractures in a hospital will remain only partially united; but upon the patient's removal into the country, cure is completed.

When a patient undergoes a formidable surgical operation, and makes a recovery, the blood is regarded as of normal constitution, free from any taint or distemper. On the contrary, when trifling wounds and accidents degenerate into chronic ulcerations, it is reputed unhealthy. And the unhealthiness or impurity may be limited to the plasma, or that part of the blood from which elements of repair are taken. At all events, it is a question or matter for inquiry, which part of the blood is most in fault in those cases. "A person shall have a sore upon the leg, which is granulating freely," says Mr. Hunter, "when all at once the granulations shall lose their life, and fade away. New granulations may afterwards spring up, and these shall undergo the same process, and so they would continue to go on, if some alteration in the nature of the parts be not produced." That is to say, some alteration in the plasma or fluid of the blood; differences in the nature of granulations being determined by differences in the qualities of the plasma. These examples are sufficient to shew—what indeed is well known—that forms of inflammation, suppuration and ulceration, are produced and kept up by distemperature or unhealthiness of blood, and more particularly of its plasma. And that a depurative operation is often performed on the plasma, by forms of local inflammation. may be argued from the eruption of small-pox. In scarlet fever, that a contagious matter from the blood is discharged by the vascular action in the skin, may be concluded from the properties of the particles of the exfoliating epidermis. And in gout, that offending matter is discharged from the blood by the local inflammation, is also to be concluded from the morbid material deposited in the parts inflamed.

For demonstrative evidence of forms of inflammation dependent on distemperatures or dyscrasies of the fluid of the blood, we may refer to the *chef d'œuvre* of pathological anatomy, the great work of Rokitansky, also to the lectures of Mr. Paget. Actual proof of alterations in the qualities of the plasma may often be obtained by venesection, in cases of inflammation. The speedy separation of the plasma, or sinking of the corpuscles, which occasions buffed blood, indicates a change in the relative qualities of the two parts of the blood. We have recorded the yellowish, greenish, and purplish hues, and the flocculated appearance like curdled soap in the plasma, of buffed blood before its coagulation in different cases of inflammation. And we have repeatedly observed with the microscope differences in elements of the plasma of blood, taken from within the sphere of inflammation, as compared with elements of the plasma of blood drawn exterior to it. It would seem, then, that abnormal qualities of the plasma promote forms of inflammation; and the disposition of the colourless corpuscles of blood to congregate and attach themselves to the coats of the vessels, we may suppose to indicate a way whereby disqualified or abnormal portions of the plasma are transferred to the common tissue for discharge.

In the first part of this lecture, we spoke of the severance of blood-vessels and other bonds of connection between sloughs and the subjacent texture without bleeding. In abscess and ulceration from thorns and other foreign bodies impacted in the flesh, we showed that great changes take place in blood-vessels without bleeding.

In fact, in all cases of repair, where exudations and new growth appear, the blood-vessels engaged undergo a change from fibrous to corpuscular—their component elements retrograde. This change is exemplified in granulations, the vessels of which bleed upon the slightest touch. We have a perfect conviction that, in places of inflammation, from disordered qualities of the plasma, the coats of the blood-vessels undergo the same changes; for we find that corpuscular elements of the plasma form, not only the coats of new vessels which arise during inflammation, but also supplant the fibrous elements of the coats of the older vessels, which must be opened to supply blood to the new ones; and as the plasma is the part of the blood which plays the conspicuous part in these changes, so it is not difficult to comprehend how it may be relieved of hurtful material in a place of inflammation.

"That sores give rise to very different kinds of pus," says Mr. Hunter, "is very evident to the naked eye; and that the different parts of which the blood is composed will come away in different proportions, we can make no doubt; and we find that whatever is in solution in the blood comes away more in one kind of pus than in another." The inference is, that inflammation exercises a therapeutical action in distemperature of the fluid of the blood. Hence, then, inflammation, exudations, granulations, and pus, may arise for two distinct purposes, namely: for the cure of mechanical injury; for the discharge of sloughs and foreign substances from the solid textures; and as a means of cure in distempered plasma.

In injuries to the common tissue, wounds, lacerations, contusions, and fractures, the process of repair comprises inflammation, granulation, suppuration, and ulceration; new blood-vessels are formed without bleeding, and there are discharges of pus. If physical hindrances interpose, the process is protracted and made chronic until they are removed. If they cannot be removed, the life of the person is endangered, and may be destroyed unless amputation be performed.

In distemperature or injury to the quality of the fluid of the blood, by unwholesomeness of diet, privations, or other causes, reactions of the same kind arise for its depuration. These also comprise inflammation, granulation, suppuration, and ulceration; boils, pustules, eruptions, and gout. If hindrances interpose—if the causes of the distemperature comprised in errors of diet, privations, etc., be not abated, the process is protracted and made chronic until they are removed, and in such cases, as in the sailor's before mentioned, to cure the local affections (the ulcers), the qualities of the plasma must be altered or improved by a thorough change of diet or habits of life.

In the one group—injuries to the solid texture (surgical cases)—the reactions arise for the healing of wounds, the discharge of sloughs, dead matter, and foreign bodies. There is direct evidence of what occasions the action, what it accomplishes, and why it is protracted and dangerous. In the other group—disorders of the blood (medical cases)—the influence of the reaction upon the qualities of the fluid of the blood can only be inferred from collateral events; and these, in gout, small-pox, and the other exanthematous fevers, point unequivocally to the therapeutical characters of inflammation.

In perusing such cases as Mr. Lowdell's, we cannot fail of remarking the advantage the surgeon has over the physician, in being able to go safely beyond the things which are seen. At the bottom of the fistulous and ulcerated openings in the flesh, beneath an inflamed and thickened integument, and through a thick crust of new-grown bone, he is able to assist, or rather to accomplish the task nature has long been endeavouring to perform. He lays his hand upon the hindrance which has made inflammation chronic, plucks it away, and the person is restored to health. Likewise in other severe injuries, a compound fracture or a crushed joint, the surgeon surveys the whole of the injured parts, and he has time and opportunity, before therapeutical reactions set in—before inflammation, suppuration, granulation, and ulceration have commenced—to form a judgment of the most probable course of events; and if, upon consultation, serious doubts be entertained whether or not the cure should be entrusted to the natural course, he amputates the limb.

The physician is very differently placed. The inflammations he has to deal with arise from qualities in the blood itself; and when he has protracted and dangerous forms to deal with, he cannot lay his hand upon the cause of danger and take it away.

From the facility with which reactions between the fluid of the blood and its containing vessels may be studied in mechanical injuries, has arisen the *doctrine of repair*, daily exemplified in numerous examples.

But reactions of the same kind, from changes in the quality of the blood itself, owing to the difficulties surrounding their explication, have been separated from the ordinary process of repair. Our object will be to accumulate evidence of the propriety of bringing all reactions between the plasma and the common tissues, the process of repair and inflammation, into one—the physiological—category, and to show that failure of success, in cases of inflammation of blood-origin, no more alters the nature and intent of the action, than does failure of success in necrosis of bone, and in other surgical cases. But further argument upon this matter must be reserved until we have spoken of the corpuscles of the blood.

Of the Red Corpuscles of the Blood.—The development of the red corpuscles of blood is doubtless conducted on the same principle as that of other cells. They take from the plasma, in which they swim, the materials requisite for their growth, and excrete the products that must be consequent on the act of absorption.

"The only hypothesis," says Dr. F. Simon, "regarding the preservation and growth of the blood-corpuscles, is that their increase in bulk is due to a reciprocal action between the young corpuscles and the plasma; and that, as independent organisms in circulation, every change which occurs in them must be accompanied by a simultaneous alteration in the plasma, from which they obtain their nutriment, and into which their excretions must pass." And of the excretions passed into the plasma, those of them which are injurious are withdrawn therefrom by the depurating parenchymatous organs—the secreting parts of the skin, liver, and kidneys.

Now if we treat the red corpuscles as independent organisms, swimming in the plasma, then any matter or substance, whether derived from the plasma or from the atmosphere, which is necessary to their sustenance, to their normal state and functions, may be regarded and spoken of as nutriment. That is to say, we may speak of the corpuscles of the blood as nourished by elements in the air, as well as by elements of the plasma; and, as respecting the matter they excrete or discharge, it is a part only of their excretions which is passed into the plasma; for certainly another part of their excretions is discharged into the air in the form of carbonic acid, by respiration. Let us pause for a moment to consider this matter. The leaves of a plant take in part of the materials of the nourishment of the plant from the atmosphere, and they excrete or discharge into the atmosphere elements that would be injurious to the economy of the plant if retained in it. Also the rootlets of a plant take up other materials of nourishment from the soil, and they excrete into the ground matters which they do not want. In both these examples, in the roots and leaves of the plant, the bodies which perform these offices of absorption and excretion, are microscopic cells—organisms analogous, and in many respects similar, to the blood-corpuscles.

Animals inhale the air, and discharge into it matters which are thrown out from the body by respiration; and, in their nutrition by diet, they cast their droppings or excretions upon the herbage, on which they must afterwards feed.

Unicellular animalcules, in a drop of water, derive from it elements of their nourishment, and also they must discharge into it the matter of their excretions. In a *vivarium*, water is the medium sustaining different forms of life, and the fluid is preserved in a fit and proper state, because the excretions of the animal furnish nutriment to the bodies of vegetable life, and because, on the other hand, the excretions of the vegetable furnish elements of nourishment to the animal bodies; the excretions of one department of life furnishing food and necessaries to the other department.

There is, therefore, nothing incongruous in the corpuscles of blood taking part of their nourishment from the air, and discharging into it a part of the matter of their excretions; and in their taking another part of their nutriment from the plasma, and discharging into it another part of their excretions. Nor, again, is there any incongruity in our finding that the plasma, or fluid of the blood, maintains its purity because the excretions of the corpuscles are ingredients upon which the functions of the depurating organs are exerted. Nor, finally, if with Zimmermann and Simon we should regard the fibrine of the plasma as an exuvial matter, or excretion of the corpuscles, is there any incongruity in finding it to be an essential material, used in the maintenance and repair of the common blood-distributing tissue.

As respects matter passing from the corpuscles into the plasma, we are able to show, by some remarkable experiments, which will hereafter be fully related, that molecular and tailed forms of some material substance may be seen issuing out of them and passing into the plasma, without much alteration of their form or colour; and, to any one witnessing the experiment, there can be no difficulty in concluding that the corpuscles, in their natural state, discharge matter into the plasma. It must be confessed that but little can be demonstrated of the vital and depurating processes constantly going forward in the blood of the living person. Nevertheless, in the absence of such demonstration, we are warranted, by the experiment just referred to, and by the undisputed teaching of physiology, in concluding that the corpuscles of blood are sustained in their vital and chemical qualities by the plasma and the air; also, that their excretions are passed, partly into the plasma, and partly into the air. And, these properties of the corpuscles being established, there are some broad and acknowledged facts respecting venous blood and urea which claim our attention,—*British Med. Journal, April* 30, 1859, *p.* 352.

4.—ON THE CONDITION OF THE HEART IN TYPHUS FEVER.

By Joseph Bell, M.D., Physician to the Glasgow Infirmary.

[It is to Dr. Stokes that we were first indebted for pointing out the existence of cardiac softening in typhus fever. This will be found in the chapter "On the Condition of the Heart in Typhus Fever," in his work "On Diseases of the Heart and Aorta." Dr. Bell having acknowledged this obligation to Dr. Stokes, sums up an interesting paper on the subject by the following propositions:]

1. That in numerous cases of typhus, about the fifth, sixth, or seventh day of the attack, the impulse and systolic sound of the heart become feeble and ultimately imperceptible.
2. That these symptoms indicate a morbid alteration in the structure of the muscular tissue of the heart, especially in the walls of the left ventricle.
3. That this alteration resembles the usual changes which result from congestion and inflammation of muscular structure.
4. That the nature of this pathological change requires further examination and research, because the evidences on which the doctrine of its non-inflammatory origin rest, are not conclusive; the circumstances on which Louis and Stokes have placed their reliance being not uniformly present.
5. That the beneficial influence of stimulants does not prove the non-inflammatory nature of the morbid change, because, in asthenic inflammation, a stimulating treatment is always necessary.
6. That whether or not the pathological alteration be owing to inflammation, the softening must be regarded as one of the special secondary effects of typhus.
7. That the proper treatment is to maintain the action of the heart by stimulants.
8. That in cases of cerebral and pulmonary disturbance arising in connection with the symptoms of cardiac softening, a stimulating plan of treatment is indicated.
9. That the presence or absence of the physical symptoms diagnostic of softened heart, may be relied on as affording trustworthy evidence, by which the sthenic or asthenic nature of these cerebral and pulmonary affections can be determined.

From these propositions, it follows as a *corollary*, that it is the duty of the physician to devote the strictest attention to the action of the heart, especially as regards its impulse and sounds, throughout the course of every case of typhus.—*Glasgow Med. Journal, Jan.* 1860, *p.* 409.

5.—ON THE NATURE AND TREATMENT OF GOUT.

By Alfred Baring Garrod, M.D., F.R.S., Physician to University College Hospital, &c.

[Dr. Garrod's work on this subject—the result of much patient investigation and extensive clinical research—we have ourselves read with the greatest pleasure. It was so long ago as 1847 that Dr. Garrod first discovered uric acid in the circulating fluid, since which time various papers of his have appeared in these pages. The principal results of chemical investigation of the blood, in this disease, are thus stated by the reviewer:]

That healthy blood contains the merest trace of uric acid or urea, so small as to be in general undiscoverable, except by the most minute and searching chemical examination, and not always then.

That, in gout, the blood is invariably rich in uric acid, which exists in the state of urate of soda, and can be separated from it, either in the form of the crystalline salt in acicular needles, or as rhombic crystals of uric acid.

That in acute rheumatism the blood is free from uric acid, or at least contains no more than in health.

That the serum obtained by the action of an ordinary blister yields uric acid when the blood is rich in this principle, except when the blister is applied to a surface affected with gouty inflammation.

That the perspiration seldom contains uric acid; but that, in gout, oxalate of lime may be crystalized from it, as also from the blood.

Dr. Garrod's researches into the nature of the urine in gout exhibit results somewhat at variance with our previous ideas; for, instead of showing an increased secretion of uric acid, they point to a well-marked diminution almost from the commencement of the disease; and the explanation of the discrepancy between the occasional apparent large amount of urates and the deficiency found by analysis seems to arise from the urine being deficient in quantity before an attack of the disease, and at the same time more acid, thus causing them to be precipitated, and become visible to the eye. From some hundreds of analyses, Dr. Garrod states that

"In the earlier stages of acute gout, the urine is scanty, and the uric acid, measured by the twenty-four hours' excretion, also diminished; that this acid is thrown out in much larger quantities as the disease is passing off, and that then, amounts even far above the patient's daily average may be excreted."

In the chronic stages of gout, the excretion of uric acid becomes much further decreased, the urea remaining but little affected; deposits of urates are now more rarely seen, and the urine frequently contains a little albumen. Even in the intervals between the attacks, Dr. Garrod has noticed a deficient elimination of uric acid, pointing out that the kidneys have undergone some structural change.

The great fact arrived at by the researches of Dr. Garrod is this—that in every instance gouty inflammation is accompanied by a deposition of urate of soda in a crystalline form; and he considers this a pathognomonic phenomenon, seeing that it is not found after rheumatic nor any other inflammation. Numerous examinations are related of subjects who had had gout in every degree, from the most severe and inveterate form, to those who had only suffered in a single joint; and in every one the peculiar structural change was detected.

[In the portion of the work devoted to the therapeutics of gout, the following paragraph shortly sums up the subject:]

"The diet should be very light, and chiefly amylaceous; diluents freely used, but no alcoholic stimulants allowed, unless in exceptional cases. The medicinal treatment should consist in the administration of some simple alkaline saline, combined with a moderate dose of colchicum; if necessary, purgatives may be given, selected according to the habit and condition of the patient. In the majority of cases, this will be found to be all that is necessary; but in some instances certain modifications may be essential; for example, if there be plethora, the question of the abstraction of a few ounces of blood may possibly arise; and, on the other hand, if the vital powers are at a low ebb, and great vascular and nervous depression exists, ammonia, in the form of the sesquicarbonate, may be desirable, in addition to, or as a substitute for, other salines; at the same time, colchicum should be altogether omitted, or used with the greatest caution. The only application required, in the majority of cases, is cotton wool covered lightly with oiled silk, which forms a protection to the joint; but now and then an anodyne may be advantageously used, and a small blister is occasionally of service."

In chronic forms of gout, Dr. Garrod considers that the following are the indications necessary to be fulfilled:

"First, to treat the chronic forms of gout by less heroic means than those employed in the acute disorder.

"Secondly, to render the blood pure by augmenting the various secreting functions, more especially of the kidneys and skin.

"Thirdly, to restore the power of the digestive organs, which are usually much impaired in chronic gout.

"Fourthly, to attend to the local mischief which the long-continued gouty inflammation induces in the articular structure.

"And lastly, to carefully regulate the diet, and pay proper attention to regimenal means."

He also proposes a new remedy, the *carbonate of lithia*, which he looks upon as probably destined to become a valuable remedy in gout and other affections connected with the uric acid diathesis. As many of our readers may be unacquainted with the properties of lithia salts, we may state that lithia possesses a very remarkable property—namely, that of forming the most soluble salt known of uric acid; and seeing that the insolubility of the acid and many of its salts leads to the formation of gravel and calculus, and probably to the deposition in gout, the salts of this alkali offer the most promising remedy in these affections. Dr. Garrod has already used them somewhat extensively. Unfortunately the known sources of lithia are at present not very abundant, but this difficulty may probably be soon overcome.

[The so-called "rheumatic gout," Dr. Garrod observes, has no independent existence.]—*Lancet*, *Dec.* 24, 1859, *p.* 645.

6.—*On Lime-Juice in Rheumatic Fever.* By THOMAS INMAN, M.D., Physician to the Liverpool Northern Hospital.—Some time ago, I stated the results of my experience of the value of lime-juice in acute rheumatism; since then I have had abundant corroborative proof of its utility. The last case under my care is so instructive, that it deserves a record.

Mary S., aged 27, was admitted into the Liverpool Northern Hospital, with a severe attack of the above disease, on the 10th of Sept. I ordered her half a pint of lime-juice daily. On my visit on the 23d, I found her only a trifle better; and on the 26th I found her worse than she was on admission. As this was the first failure, I had met with, instead of altering the treatment, I went into the shop to inspect the lime-juice, of which I was told the supply was fresh. and furnished by a first-rate house (the Liverpool Apothecaries' Company.) I found it colourless, slightly viscous, and, on my tasting it, it seemed to be nothing but lemonade sweetened and acidulated with tartaric or other acid. I directed the pure material to be procured, and the patient to take it as before. After leaving the hospital, I lost sight of the patient; but I called to hear of her on the 5th of October. I did not see her, but next day I received a note from the House-surgeon, stating "that the patient got well rapidly after the lime-juice had been changed, and was discharged cured in three days afterwards."

This case proves:—1. That the lime-juice procured at first-rate houses is not always to be depended upon. 2. That lime-juice does not act simply because it is an acid. 3. That lime-juice may have been undervalued, because those employing it have not had the pure material. 4. That the treatment of acute rheumatism by this drug is not equivalent to leaving the disease alone.—*British Medical Journal*, *Oct.* 29, 1859, *p.* 865.

7.—*On Acute Rheumatism.* By THOMAS INMAN, M.D.—[The patient, a man aged 34, was admitted into the Liverpool Hospital with the usual symptoms of acute rheumatism. Severe pain in the joints, excessive but not very odorous perspiration. Lime-juice, freely administered, had no marked effect. Profuse sweating continued for a fortnight, at which period twenty minims of the tincture of sesqui-chloride of iron were given, three times a-day: improvement immediately began, the sweating and pain abated, and in four days the skin was natural. The patient was soon after able to get up. After detailing the above case, the author offers the following remarks :]

This case illustrates many points :—1. It corroborates what I have before noticed, that those cases of acute rheumatism are the worst in which the perspiration is most profuse. 2. It is the first in which I have known lime-juice to fail. (I must note, however, that I had a similar case some years ago at the Northern Hospital, where I administered lime-juice for three weeks before it effected any apparent change.) 3. It shows that great improvement may occur with a diminution of the perspiration; and, consequently, that the cutaneous secretion is not an eliminative effort of nature, which we ought necessarily to encourage. 4. Coupling together the facts, that in acute rheumatism the blood is as poor in globules and as rich in fibrine as it is in consumption, Bright's disease, and anæmia; that the cases in which the fever is the worst, are those in whom there is some hereditary or acquired constitutional debility; that the intensity of the disease is augmented and the complications increased by venesection, salivation, and purgation; and that such debilitating treatment prolongs convalescence, if it do not actually prevent it; must we not conclude that the sweating in this disease is more analagous to that in consumption and ague than it is to anything else? 5. Referring to a preceding paper of mine (see Vol. xl. p. 361) upon the influence of vitality upon the secretions, may we not ask whether the well known sour smell of rheumatic sweating is not due to its rapid decomposition *after* it has become external to the skin; and consequently, that it is no more proof of the elimination of lactic acid, &c., than an ammoniacal smell from the bed-pan is a proof that the kidney and bowels are eliminating ammonia from the system? I certainly can recall instances in which the perspiration has smelt rheumatic, when the sole complaint was debility, and conversely, I have known many cases of mild rheumatic fever where the perspiration did not smell sour.—*Brit. Med. Journal, Nov.* 12, 1859, *p.* 906.

8.—ON OPIATED COLCHICUM-WINE IN RHEUMATISM.

By Dr. Eisenmann, of Würzburg.

Dr. Eisenmann first states the wide application which he gives to the word "Rheumatism," denoting by it every affection which may arise in the healthy system, independently of any specific cause, from exposure to cold. "By exposure to cold I do not understand merely the effect produced by the contact of cold water or cold and humid air with the external integument, but also that which takes place when cold and damp air penetrates into the lungs, or when cold water is taken into the stomach, the temperature of the body having been raised by exercise." This view is justified by the facts —1. That cold gives rise to the most various affections of the nervous and vascular systems; 2. That these various affections may become, by metastasis, transformed into each other; 3. That they communicate to the economy a marked predisposition to affections of the same nature, such predisposition being increased with each reproduction; 4. And that they yield to the same treatment, whether they show themselves under the form of neuroses or of vascular affections. Under the title of rheumatic inflammation, therefore, the author ranges all inflammatory affections of the heart, lungs, pleura, peritoneum, kidneys, serous membrane of the liver, &c., when these are not due to any specific cause, and treats them in the same manner as acute articular rheumatism.

The means which beyond all others he has found of efficacy in the treatment of rheumatism is a mixture of colchicum and opium, the colchicum acting far more efficaciously when so combined, and then not giving rise to the half-poisonous effects which often attend its use when given alone. Neither the one or the other substances will produce alone the advantageous effects which result from their union. Dr. Eisenmann speaks not only from his own large experience, but from that of many of his medical friends, among whom "Eisenmann's Drops" have acquired a great reputation. These consist of 12 parts of colchicum-wine, and 2 of tincture of opium, 20 drops being taken three times a day. Instead of preparing the colchicum wine with sherry as he formerly did, he now makes it according to the formula of the Prussian pharmacopœia, which directs 150 parts of the colchicum seeds to be macerated in 770 of alcohol; this preparation being always uniform in strength, and more active than the ordinary colchicum-wine. Although the above drops succeed so well in acute rheumatic affections, they are of little or no use in old and chronic cases. This induced the author to try the effect of adding minute doses of corrosive sublimate; and although his trials of this modification have been as yet too few to admit of an opinion being pronounced in respect to chronic cases, he has found the addition of great advantage in many cases of the acute form. Although in the various forms of rheumatism in which he has employed this treatment he has not had to have recourse to preliminary bleeding, he by no means denies that this may not be occasionally desirable in the robust.

In treating *acute articular rheumatism* in this way its course has usually been cut short in from the third to the fifth day, convalescence rapidly following, and no trace of heart affection persisting. When the pains have been very severe, tepid applications of a very weak solution of corrosive sublimate have been made to the joints, with the most satisfactory results. Sometimes after a rapid amelioration by means of the colchicum, when the pulse still continues irritable, and the tongue remains loaded, an emetic or purgative expedites convalescence. Among the rheumatic affections of the *mucous membranes*, which may be rapidly and durably cured by means of the opiated colchicum, without the sublimate, may be specified angina, pulmonary catarrh, and influenza, gastric fever, catarrhal diarrhœa, and catarrho-rheumatic conjunctivitis. In the case of catarrhal ophthalmia, even of a severe character, its remarkable efficacy may be watched step by step. Among affections of the *serous membranes*, pleurisy and perihepatitis stand prominently forward as amenable to this treatment. Of parenchymatous inflammations, *pneumonia* has been the only one in which the medicine has been tried, and that only in two slight cases, which recovered with rapidity. In *muscular rheumatism* of the head, loins, &c., from two to four doses have always sufficed. It is also of great efficacy in cases of *rheumatic neuralgia*, especially in facial or intercostal, in sciatica and odontalgia. But the case must be recent, or it will be of no avail. In *odontalgia* the results are truly remarkable, a single dose rapidly dissipating the pain. The distinction between the rheumatic form of odontalgia, and that which arises from carious teeth is exhibited by the different effects of the colchicum. For the relief of odontalgia arising from carious teeth, the author, after having cleaned out the cavity of the tooth, introduces into it a morsel of nitrate of silver as large as a pin's head. In about a minute the moisture of the mouth dissolves this, and the mouth is then to be gargled with cold water, and the pain disappears. He has employed this plan of relieving the pain of carious teeth for the last twenty years, and he has seldom known it fail, even after the ineffectual trial of various other measures. It causes no pain, and it retards the progress of the caries.—*Bulletin de Thèrap.*—*Med. Times and Gazette, April* 14, 1860, *p.* 376.

9.—ON EXPERIMENTAL PATHOLOGY.

By M. Claude Bernard, Member of the French Institute; Professor of General Physiology at the Academy of Sciences.

[In a previous lecture (this being an abstract of one of a course delivered at the College of France) M. Bernard showed how necessary a previous study of other sciences was, to a successful study of the complex science of medicine, and that morbid phenomena are merely modifications of those phenomena occurring in health.]

We shall now enter on the study of the symptoms peculiar to the pathological state, the agents which give rise to them, and those calculated to bring about their removal; lastly, we shall produce all the phenomena of disease by artificial means, and shall then endeavour to make them disappear.

But, before entering on this subject, it is indispensable to ascertain whether everything which occurs in a state of disease can be explained on physiological grounds; or, whether disease has the peculiar property of originating, in the living being, laws altogether new, and of which we have not the slightest idea as existing in a state of health.

If, in the case of an adult in the full enjoyment of all his faculties, all we ask ourselves what is the regulating agent, what the *primum mobile* of all physiological actions, we are constrained to reply that its seat is the nervous system. It is to the nervous system that we owe both sensibility and voluntary motion, that two-fold source of all our relations with the external world; it presides over all organic functions, and it is my intention to prove to you, that while it is the origin of all the normal phenomena of life, it is also that of all pathological action.

In proportion as we ascend in the scale of animal life, we see the nervous system acquire greater development, and at the same time we observe that diseases become more frequent, more variable in their form, and more complicated in their nature. Why should this coincidence astonish us? Are not all our organs dependent directly on the nervous system? If we take, one by one, the different systems of the animal economy, it will be easy to show that all the symptoms of the diseases to which they are liable, may be produced by direct irritation of their corresponding nerves. We can even give rise, in this way, to all the anatomical lesions by which they are characterized.

What, for instance, are the principal signs of the affections of the respiratory organs? Cough, dyspnœa, increased bronchial secretion; are not these the symptoms which most frequently proclaim their existence? Now all these phenomena can be produced at will by the direct excitation of the pneumogastric or certain other nerves; we can even call into existence the anatomical lesions incidental to pleurisy and pericarditis. The causes of these morbid changes would therefore appear to be intimately connected with the nervous system. If we now turn our attention to the digestive apparatus, we shall soon be convinced that the physiologist possesses the same power relative to it, as he does in the case of the respiratory organs. By exciting the solar plexus and its efferent branches, we can determine both diarrhœa and dysentery, together with the anatomical lesions which habitually accompany them. Acute peritonitis has even been induced with all its consequences, as evidenced on opening the animal, by the presence of pus and false membrane in the peritoneal cavity.

Thus, then, a multitude of diseases may be brought into existence by a simple modification of the elements which the animal economy originally contains, without having recourse to the introduction of any new principle; and if we were to examine the other systems of the body, results analogous in their nature would be obtained. Fever itself, that essentially medical symptom, can be excited by a mere mechanical irritation of the nervous system, and the products of inflammation, such as pus, false membranes, and plastic exudations, may, any, or all of them, be called into existence, in a similar way. In an animal, previously enfeebled, we can produce directly pleuritis with purulent deposit, by the simple division of the great sympathetic nerve, in order, however, to ensure success in this experiment, it is absolutely necessary that the state or condition of the animal's health be previously lowered.

It is, therefore, a fact, that the perverted state of the nervous system gives rise to a great variety of diseases, not only of a general, but also of a local character: deprive a muscle or a bone of its nervous supply, and you will have, as a consequence, fatty degeneration in the one case, and rickets in the other; in fact, if you tie the nerves, which enter the nutritive foramina of a bone, you will very soon see the cells of the lamellar structure increase in size, the vessels become more numerous, and all the phenomena of rickets follow in rapid succession: we can even bring about these results on *part* of a bone, without interfering with the remainder. This experiment has been successfully carried out, in the case of the lower jaw, by M. Schiff, of Berne.

But there exists in disease an immense number of other phenomena which, at first sight, it appears impossible to produce by a simple lesion of the nervous system: I allude especially to the alteration or modification of the fluids of the body, which takes place in the course of certain maladies. Now I am prepared to demonstrate that a vast number, if not all, of these morbid changes, are still to be traced to the action of the nervous system, and that they can be reproduced at pleasure by the physiologist. Among the various fluids of the body, the urine is that one the morbid changes of which have been the most carefully and completely investigated. Now you are perfectly aware, that albuminuria, polyuria, and diabetes, are invariably produced by excitation of definite points of the medulla oblongata, the peculiar form of the perverted urinary secretion being determined by the particular portion which is acted on: it is in the case of diabetes especially that the importance of this experimental fact is fully brought out. It was supposed that in diabetic patients the morbid state created entirely new conditions in the economy, which gave rise to the pathological production called sugar; it is now, however, admitted on all hands that these phenomena are fully explained by the mere exaggeration of a normal function, in virtue of which glycose is generated in every individual even in a state of health. It is therefore evident that disease, in this case, is nothing more nor less than an exaggerated natural function.

There exists, however, a certain number of pathological products and morbid manifestations which we have not yet been able to imitate by the employment of artificial means. Shall we be able, at a later period, to connect these facts with those which already fall within the range of experimental physiology? Such is the scientific problem of the day. The question is, whether we shall, one day, be able to embrace pathology in its entire extent, within the compass of biological explanations, or whether we shall, in addition to all which we can imitate or explain, for ever be compelled to recognise a *special principle*, mysterious in its nature, which we must acknowledge as a morbid or vital phenomenon?

Let us take, for example, eruptive fevers, small-pox, scarlatina, and measles; these are diseases, indeed, which it is impossible for us to produce without having recourse to a special virus. Shall we, one day, be able to realize this undertaking without the intervention of the peculiar animal poison on which they seem to depend? Must we not, first of all, solve the question whether such diseases as those we have just specified can possibly exist in animals, even in those which approach most closely to the human species? Are they not in reality the exclusive property of the human organization?

Nothing is more difficult than to produce, through the agency of the nervous system, eruptive diseases in animals, the vitality of the skin of which is essentially different from that of man. We can, nevertheless, produce ecchymosis, congestions, and glandular swellings; but it must always be borne in mind, that each particular species of animal has its own peculiar diseases, which cannot be transmittted to a neighbouring species, however closely allied they may be. Now man, in himself, presents a greater number of special diseases than all the other animals taken together.

Fortunately for physiology, such incompatibilities are rather the exception than the rule. Tubercle, cancer, and many other morbid productions, are found equally in animals and in man. Every disease which gives birth to morbid tissue is evidently a perversion of the nutritive function; now, who will venture to deny the influence which the nervous system exercises over this physiological act? We must, therefore, advance resolutely in the path which lies open before us, without allowing ourselves to be disheartened or intimidated by the difficulties of our science. But we must bear in mind that a disease is not characterized by one single symptom; it consists rather of a complete series of symptoms, standing to each other in the relation of cause and effect. It is, in fact, a morbid evolution which offers a commencement, a middle, and an end; so that a skilful and practised observer, on witnessing the first stage of a disease, can predict its probable termination. This is no doubt true; disease does not consist in an isolated symptom; it is a collection of symptoms. Now these reunions of morbid phenomena, we indubitably succeed in reproducing in animals. The functions of life are modified in various ways by a variety of different agents. Poisons determine real diseases which present an unbroken chain of symptoms, consequent on the introduction into the system of the toxic agent. Here, therefore, we find an entire class of diseases which can be produced at will.

But setting aside this question, of such vast dimensions, and to which we shall revert at a later part of our course, let us inquire whether, by mere surgical operations, by mere mechanical lesions, we can determine on the animals subjected to experiment, a certain number of morbid series. If you simultaneously remove the two kidneys of a dog, or simply tie the renal arteries, you immediately produce a general disturbance in the entire economy. The animal is powerless in expelling the excrementitial product which should pass off by this channel, and the whole system becomes gradually poisoned. At first the animal is not seriously affected; it continues to eat and digest its food for a certain lapse of time, which corresponds with the period of incubation in diseases; by-and-by it is attacked with vomiting and purging, shortly after which it dies.

What takes place in a case like this? Let as endeavour to explain it. During the first period the urea, which can no longer be eliminated by the kidneys, is expelled by the intestines. It is found, together with the salts of ammonia, in the animal's excrements, and even in the gastric juice. If this new mode of elimination could be prolonged indefinitely the animal would not become diseased—it would not die; but very soon the mucous membrane of the intestines, irritated by the constant contact with the ammonial salts, gives rise to morbid changes. On the other hand, as long as the urea is eliminated by the intestines, it does not find its way into the blood. This fact has been demonstrated experimentally by MM. Prevost and Dumas, who have not, however, succeeded in explaining it. Now, at a later period, when the mucous lining of the intestines refuses to continue this function, which is altogether foreign to it, the urea finds its way into the blood, and the animal soon expires, comatose and convulsed.

When the cessation of the urinary secretion depends on the ligature of the renal arteries, this state of things may sometimes be obviated by removing the ligatures; the self-same thing would also take place in man, if there existed an obstacle to the passage of the urine, and if it were possible to remove that obstacle: but in all cases in which the kidneys have been removed death has always supervened. The destruction of the animal has been the invariable termination of the morbid series.

Here, then, we have a disease which can be artificially produced; but there are many others, the causes of which are agents existing exterior to the body; contagious affections belong to this class. The fact has been experimentally proved in the case of peri-pneumonia of horned cattle, by the establishment of a communication between two cow-houses, the one containing healthy, the other diseased cattle. In the inclosure, which, in the first instance, contained only healthy cattle, several cases of this peculiar affection occurred in succession.—*Medical Times and Gazette, Jan.* 21, 1860. *p.* 55.

10.—ON DISEASES ARISING FROM THE VITIATED DEVELOPMENT OF CELLS.

By M. Claude Bernard, Member of the French Institute; Professor of General Physiology at the Faculty of Sciences.

We are compelled to admit the existence of three principal classes of phenomena within the living body, which, although closely connected through reciprocal influence, exist independently of each other.

Firstly, Nervous phenomena, embracing all the mechanical actions of life;

Secondly, Catalytical phenomena, embracing the various kinds of fermentation;

Thirdly, Histological Phenomena, embracing the entire results of cellular evolution, or the process of development.

The two first classes of physiological agents, viewed in connection with the origin of disease, have been examined in our previous lectures; it now remains for us to investigate the latter,—viz.: the morbid phenomena superinduced by disordered powers of evolution.

It cannot be denied that during the first period of embryonic life, the development of tissue is in no way connected with nervous influence. The nerves have not yet been called into existence, and are only created at a later period, through the agency of that mysterious force which presides over the evolutions of the being so lately called into life. The vascular system being, for similar motives, set aside, we discover, in the very first stage of existence, nothing but the primitive cell, and the medium in which it grows to maturity. But micrographists do not entirely agree in the view they adopt of the first step which commences that long series of successive evolutions, by which an entirely new living being is ultimately produced. That within an entirely amorphous medium, a living cell might spontaneously be produced, was the opinion of Schwann; but in our own days, these views appear to have been completely laid aside; modern investigators are convinced that all living organisations are invariably derived from one primitive cell, which, by its divisions, and subdivisions, give birth to all the rest; now, this primordial element originates, of course, with the animal's parents.

We meet, however, with cases in which it seems altogether impossible to deny that one or several primitive cells have been spontaneously generated within a medium which previously contained no vestige whatever of histological elements. The serum of blood, for instance, when introduced into a vessel hermetically closed, is found to contain, after a few days have elapsed, a large number of yeast-cells, which the grape-sugar dissolved in this liquid has probably produced; and yet, when carefully examined under the microscope, immediately after coagulation had taken place, it did not contain a single nucleus or cell. Similar results frequently occur for notice; and, in more cases than one, the mere process of subdivision and multiplication will not be found sufficient to account for cellular development.

In the adult the phenomena of evolution continue to exist, and although far less extensive than during fœtal life, they are almost equal in importance. Thus, in certain animals, organs are found, which momentarily disappear, and are subsequently reproduced. Hunter observed that the sparrow's testicle, reduced during winter to the smallest possible bulk, promptly returns to its primitive size during the first days of spring. Mr. Stanlius has collected all the cases of similar reproduction which have been noticed in the animal series; thus in hibernating mammals, for instance, certain parts of the body are completely atrophied during winter, their functions being suspended for a time; some nervous ganglia are even found to disappear, more especially those connected with the genital organs; but as soon as the physiological activity of these parts is awakened, they are called a second time into being. Hunter also discovered that in the pigeon a peculiar organ, no vestige of which exists in the animal's previous state, is produced during the latter part of incubation; the mucous membrane of the gizzard becomes tumefied, grows vascular, and secretes a new substance, which serves for the nourishment of the young birds when newly hatched. In the human species (as in all mammels besides), the rapid development of the mammary glands, towards the period of lactation, offers another instance bearing on the same point.

But must we consider these as the only phenomena of a similar nature which take place within the system? Do not mucous surfaces continually secrete new layers of epithelial cells? and is not the epidermis incessantly renewed on the surface of the skin? Here, then, we discover evident instances of histological development going on during all the stages of life With respect to muscular tissue, its tendency to increase in bulk under the effects of constantly increased exertion is perfectly known: but it has been hitherto admitted that the constituent fibres of a muscle may no doubt increase individually in size, but that their number invariably remains the same. A German micrographer, Mr. Budge, has lately endeavoured to prove the contrary. A frog, being reduced by abstinence to a state of emaciation, a small muscle is laid bare, and the number of its elementary fibres ascertained by direct investigation under the microscope. The animal being then properly fed, and gradually restored to a state of health, the same muscle is again examined; and, according to the ingenious observer, the number of its elementary fibres is found to have considerably increased, as well as their bulk.

Let us now consider the morbid manifestations of this power, which never suspends its action within the living body; we allude, of course, to those peculiar tissues which have been styled heteromorphous—an expression utterly condemned by German micographers; for morbid tissue is generated within the economy in strict conformity with the laws that preside over fœtal development. But as we have previously seen, both nervous influence, and catalytical agency, give rise to a variety of diseases when deviated from their proper course; thus, also, in certain given cases, the power of histological evolution may create positive disorder in the system. An immense and uninterrupted movement takes place within the organs of which the body is composed, for the purpose of supplying new tissues, in the place of those which are no longer fit to accomplish the functions devolving upon them; let this unceasing activity be diverted from its proper channel, and the production of tubercle, cancer, and all kinds of morbid deposits, will be the immediate consequence. We find here as usual, an evident connexion between the phenomena of health and disease, between physiological activity and pathological influence. The question which lies before us must evidently be viewed in this light; and such is the principal object of Virchow's labours on cellular pathology, the leading features of which it is our purpose to make known to you. But before entering into the study of this particular point, a few general notions on the subject cannot safely be dispensed with.

Some diseases, in the first place, result from total absence or considerable deficiency of normal evolution on a given point. The mucous coat of the intestinal tube affords us a fine example of incessant development. New layers of epithelium are continually being secreted, to line its inner surface; but a living medium, or blastem, is necessary to their production; and whenever this blastem itself happens to be altered in its essential properties—a modification which always occurs in inflammation—the epithelium disappears, and is no longer regenerated. Cholera also exhibits another instance of this; for it has been indisputably proved that, in this disease, the vessels which ramify on the internal surface of the intestines are completely laid bare. In his admirable researches on the intestinal mucous membrane, Professor Goodsir has established that, after each meal, when absorption has taken place, the epithelium which covers the villosities falls off, and is renewed during the interval which elapses before food is again introduced into the digestive apparatus—a remarkable instance of the rapidity with which the reproduction of tissue frequently takes place. But when, through some pathological agency, epithelium is no longer secreted, what results from its absence? No obstacle is henceforth opposed to serious exudation from the vessels; no protecting surface resists the introduction of various poisons into the economy; and, lastly, no regulating power of absorption any longer exists. In this manner innumerable diseases may be traced back to the suspended activity of normal evolution as their primary cause. The chronic inflammation of the trachea and bronchial tubes likewise destroys the vibratile epitholium, the utility of which is too well known to be expatiated on.

But we meet, at the same time, with other diseases, which arise, not from interrupted, but from perverted evolution. Cells which pursue a regular course in their development comprise three distinct elements—Firstly, an envelope, or cellular paries, the physical properties of which take a prominent share in its action; secondly, liquid contents, the importance of which is principally derived from their chemical composition; and, lastly, a nucleus, in which the powers of development appear to reside. As soon as a morbid state of nutrition supervenes, the contents of the cell are liable to alteration. Whether pigment, or fatty substances, or carcareous salts are therein deposited, morbid tissues are gradually formed, and disease is introduced into the system; and, even in similar cases, no pathological entity; no abstract principle of disease, is required to explain the fact. The deviation of physiological activity is its only cause. It is therefore evident that, in their successive phases of development, heteromorphous tissues entirely resemble the normal ones, and are subject to the same natural laws. To Muller belongs the honour of having been the first to proclaim this great principle; and he may therefore be deservedly styled the creator of cellular pathology. He was the first to open that path in which Virchow now treads with so much success.

The intercellular tissue, or blastem, is the medium from which the cells derive the elements of their formation; it is, according to Virchow's picturesque expression, their territory. Now there exist various conditions in which the blastem no longer contains the principles required for the normal development of cells; it is, for instance, indispensable that it should always contain glycose, albumen, and fat; the absence of a single one of these three substances is an insuperable barrier to cellular evolution; and we, therefore, constantly find them existing as well in the tissues of the embryo, as in those of the adult. But a variety of other conditions, essentially injurious to histological growth, may casually arise; and the existence of morbid blastems, which give birth to all tissues endowed with abnormal properties, may easily be conceived as of possible occurrence. Such are, no doubt, those very general dispositions of the economy, known under the name of diatheses, and which, when once they have firmly established their hold on a previously sound individual, are capable of being transmitted to his posterity; we must evidently consider them in the light of conditions of existence entirely new, which in the first instance are accidentally produced (for disease must evidently begin somewhere), but which when once called into existence, exhibit a strong tendency to maintain themselves in being. Thus, when food, insufficient in quantity, or of an unwholesome kind, has ultimately reduced to a consumptive state an animal previously enjoying perfectly sound health, its offspring often inherits the morbid disposition which, in the parent, was entirely accidental; and syphilis, that well-known and fruitful source of heteromorphous productions, is similarly transmitted from parent to child.

Such pathological dispositions, or diatheses, result from causes various in their nature, but which concur in one point. viz. the disposition which opposes all modifications favourable to the patient's health. Sometimes they are the result of a profound change in the fluids of the economy; sometimes they originate in the introduction of peculiar poisons, which, after having once penetrated into the system, can in no way be expelled: if there existed, for instance, a poison which none of our organs could eliminate, it is clear that after penetrating into the torrent of the circulation, it would nowhere find an issue, and would in consequence become the origin of permanent modifications in the economy. The possibility of a similar case may be rationally conceived, by referring to the singular fact already mentioned, that iodine when once introduced into the blood, is not eliminated before a long space of time, on account of the affinity which the salivary glands exhibit for this substance; we have, therefore, in this case an instance of a body which cannot (for a time at least) be expelled from the system; the animal is, therefore, during that period, laid under an iodic diathesis.

Viewing the subject in an entirely physiological light, it may be contended that individuals affected with local cancers are not properly, so to speak, in a state of disease, as long as the organs affected are not altogether essential to life; but when cancer attacks the limbs, the possibility of a Surgical cure may at least be presumed, if not expected actually to take place; and the patient is not really diseased—that is to say, life is not directly brought into danger. Thus, when cancer attacks the liver, if the disease is not too extensive, the morbid productions are separated by large tracts of sound tissue, which fulfil, as in the healthy state, their physiological duties; bile is secreted as usual, and grape-sugar exists within the glandular tissue. But when, at a later period, the disintegration of the elements which constitute the morbid production have poisoned in some measure the whole economy, by pouring into the torrent of the circulation fluids impregnated with the noxious principle, then, indeed, the affection becomes a general complaint, and its nature entirely changes. Cancer is not a diathesis in itself; but the subsequent cachectic state is evidently diathetical.

To conclude the history of these morbid evolutions, there yet remains one to be described; and this is imperfect nutrition. It is evident at the present day that the anatomical conditions, brought so prominently forward in Bichat's celebrated work, are quite insufficient to explain all the various modes of dying. Experience has taught us that patients often die without offering, in the post-mortem examination, the slightest modification in the anatomical condition of their organs. In the course of our physiological experiments we often see dogs arrived at the very last stage of emaciation, although the appetite continues unimpaired till the last moment. They sink from sheer exhaustion, while the lacteals are gorged with chyle; and, when opened, their bodies offer no trace whatever of pathological alterations.

The latent cause of this singular process is, that nutrition, when considered within the depths of our organs, is, in fact, nothing more than a peculiar mode of evolution. The economy produces within itself substances indispensable to life; glycogenous matter affords us an example of this: formed within the body by a special process, it plays an immense part in histological phenomena. As soon as it fails to be supplied, epithelium is no longer produced; various diseases are the immediate result; and, under similar circumstances, life is inevitably brought to a close. The physiological act called nutrition, comprehends, therefore, two distinct parts: formation of cells is the first; creation of blastems is the second; and the latter is no less indispensable to our existence than is the former; as soon as pathological influences arrest either the one or the other, death is the consequence. There exist, therefore, two distinct modes of dying: sometimes life is cut short at once by an important injury to some essential organ; somtimes, on the contrary, it gradually fails through imperfect nutrition; and this latter termination is the ordinary result of acute diseases, when they prove fatal. In certain cases, for instance, glycogenous matter is no longer produced; and after a given space of time the patient dies, although the appetite remains unimpaired till the last moment. In making the autopsy, the lacteals will be found in a state of repletion; but when analysed, the fluids of the economy no longer present the slightest vestige of sugar. Death then supervenes, and is the mere result of suspended activity in organs for which proper nourishment is no longer provided.

You therefore see, that to create laws especially intended for the use of pathology cannot in any case be justified; and that physiology furnishes, in every possible condition of health or disease, a key to the interpretation of vital phenomena. These general notions I look upon as indispensable to the study of particular points: it now remains for us, in order to complete this general survey, to examine the all-important question. "Whether medicines act on a sick patient in the same manner as on a sound individual?" and how far the results obtained in one case are fit to be compared with those observed in the other. It is our intention to examine this subject: its study is an indispensable introduction to the various investigations we are about to undertake; for, after producing artificially—no matter how—a morbid state in an animal, we shall have recourse to the counterproof, by seeking for therapeutical agents to effect its cure.—*Med. Times and Gazette, March* 3, 1860, *p.* 209.

11.—*On the study of disease by synthesis.*—At the Medical Society of London, Dr. Richardson read a paper, the third of the kind he has written on the subject, called the "Study of Disease by Synthesis." In detail, as Dr. Richardson admits, diseases have been studied by synthesis for ages; since the unknown time, in fact, when inoculation for small-pox was first performed. But it is his design to reduce this study by synthesis, or, in other words, the study of disease by inducing diseases artificially, to a perfect system adapted to the entire investigation of disease by philosophical experiment. Towards this end, Dr. Richardson divides diseases for study into six great classes—viz.: parasitic diseases; zymotic diseases; diseases the primary origin of which is in the nervous system; diseases in which from a malchemistry a new growth is evolved from the economy, and supplied out of the organic materials—malignant diseases; diseases due to simple transformations of natural tissues—degenerations; and, inflammations. In the paper to which we refer, the most interesting point adduced related to the question, whether inflammation is or is not a local change. According to Dr. Richardson's view, derived from his experiments in the production of endocarditis, there is no possibility of local inflammation unless there be brought to the part to be inflamed a foreign agent which shall excite a primary local change—congestion. But this excitation set up, all else—all future products, even to pus—are purely local, and may be independent of the condition of the blood. There is thus, according to this argument, no abstract difference between inflammation of the skin caused by a blister and rheumatic endocarditis. Such difference as exists is, in short, merely in conveyance of the exciting body; in the case of the blister, the irritant is conveyed by the hand to the part—in endocarditis, by the blood. The results are the same.—*Med. Times and Gazette, Feb.* 4, 1860, *p.* 121.

DISEASES OF THE NERVOUS SYSTEM.

12.—THREE CASES OF TETANUS, IN WHICH WOORARA WAS USED IN THE TREATMENT.

By T. Spencer Wells, Esq., F.R.C.S., Surgeon to the Samaritan Hospital, &c.

[So early as 1811, Sir B. Brodie experimented on the action of this poison, and more recently Professor Sewell, at the Veterinary College, and Mr. Morgan and Dr. Harley (an article by whom appears in our last volume, p. 37.) On the Continent, Vella, Manec, Chassaignac, and Bernard have recently experimented upon the application of woorara in the treatment of tetanus in the human subject.]

The author detailed the particulars of a case of chronic but severe trismus, going on to opisthotonos, appearing a fortnight after ovariotomy in a patient forty-one years of age. The exciting cause appeared to have been a draught of cold air. The great point of interest in the treatment was, that woorara was used hypodermically and epidermically, six grains of the extract having been used in six days. The patient recovered.

In a second case, acute tetanus appeared seven days after ovariotomy in a lady thirty-eight years of age, and proved fatal in three days. The treatment was commenced by assafœtida injections, and woorara was afterwards used, the softening extract having been inoculated in both arms.

In the third case, tetanus appeared four days after a simplified prineal operation for the relief of prolapsus uteri in a patient fifty-one years of age. It progressed slowly; was treated first by opium and ether, afterwards by woorara, and latterly by chloroform, the influence of which was kept up at intervals for forty-eight hours. The patient died on the evening the paper was read.

The author, after expressing regret for the little positive information he had been able to lay before the Society as to the real value of woorara in the treatment of tetanus, thought that he had seen enough to establish the following propositions:

1. That our knowledge of the physiological action of woorara—of its antagonistic effects to the artificial tetanus of strychnine; of the results of its use in idiopathic tetanus of the horse and ass; and the fact that two cases of chronic tetanus in man on the Continent, and one in this country, have recovered during its application—should encourage further experiments.

2. That although three cases of acute traumatic tetanus in man on the Continent and two in this country have died, notwithstanding the use of woorara, this should not discourage us from further trials, when we consider the very fatal nature of this form of tetanus, and the fact that in only one of these cases was the woorara applied in a large quantity.

3. That looking to the probable difference in strength of the specimens of extract brought to this country, and to the well-founded belief that they are not all prepared from the same species of vegetable it would be well, in future trials, to use a solution of the active principle of woorara—the alkaloid curarina.

4. That it is desirable to ascertain, as far as possible, by experiments on the lower animals, what dose of this alkaloid might be inoculated with safety in man; and whether the artificial tetanus of strychnine establishes a tolerance of curarina. This would afford some test as to the safety of using much larger doses in man when suffering from tetanus than when in a state of health.

5. That veterinary surgeons should be requested to aid us in our attempts to determine the value of woorara when treating tetanus in the lower animals, by carrying on the dose of the poison until its full effects were shown by suspended animation, and then restoring the animal by artificial respiration.

Dr. HARLEY, after complimenting Mr. S. Wells on the able manner in which he had treated his subject, said that he agreed with the author in believing that the conflicting opinions regarding the action of woorara on the human body were chiefly due to the fact of the specimens employed not being of uniform strength. He had in his possession at the present time five specimens of the poison prepared by different tribes, and, although the general action of all was identical, yet their strength varied considerably. There was, however, a second and equally important reason—namely, the mode of administration. Woorara differed from many remedies in not being absorbed equally and readily when introduced into the body by various channels. Thus, it could not enter through the unabraded skin, and when taken by the mouth, its action was extremely doubtful. Dr. Harley had made a pigeon swallow twenty and a mouse thirty times more than was sufficient to destroy them if introduced by a wound, and yet both remained unaffected. This did not arise, he said, from the poison being destroyed in the digestive canal; for he had seen Bernard poison a bird with the excrements of a dog, to which a poisonous dose of woorara had been given with impunity. As regards the value of woorara when compared with other narcotics, Dr. Harley remarked that he considered that its superiority consisted in its peculiar power of paralysing the motory, and not the sensory nerves —nay more, he said that it could be so administered as to destroy entirely the power of voluntary motion without impairing the consciousness of the animal. (Dr. Harley related one or two experiments in proof of this statement.) In administering woorara to animals labouring under tetanus induced by strychnine, he therefore gave sufficient to paralyse all the muscles except those of respiration. In this way he was able to allay the tetanic spasm without destroying the intelligence or arresting the performance of the organic functions; and by continuing the thus moderated action of woorara until the kidneys had time to eliminate the strychnine from the system, he had been able to save the life of the animal. The theory of its action in tetanus was, Dr. Harley considered, much the same—that is to say, you try to keep the spasms from killing the patient by their violence until the morbid state calling them into play has exhausted itself. The chances of curing by woorara a patient labouring under strychnine-poison, are naturally much greater than those of curing a case of traumatic tetanus, where the morbific matter may go on increasing until its effects are more than sufficient to counteract the influence of woorara.

Dr. SIBSON said, that as far back as 1838, he had been in communication with Mr. Waterton, on the subject of woorara, and that gentleman, after hearing of Mr. Sewell's experiments, went over to New Guinea to obtain a quantity of the poison, with a view to its employment in cases in tetanus and hydrophobia. He subsequently made experiments upon an ass to prove that an animal might be resuscitated, after what would have otherwise been a fatal dose, by the employment of artificial respiration. The experiments to produce the artificial respiration were performed by himself (Dr. Sibson). The animal lay apparently dead for upwards of an hour, the beating of the heart being the only sign of life which it exhibited. It was, however, at length made to breathe, and an hour afterwards Mr. Waterton walked the animal round the room. He (Dr. Sibson) subsequently experimented with some of the poison on a horse affected with tetanus. He administered a dose proportionate to that given to the ass, but no decided effect was produced, and on increasing the dose the animal succumbed. In another instance the horse respired at the end of three or four hours, but before the apparatus could be applied a second time, it died. During the whole time the experiment was going on, the animal was free from tetanus. He had also experimented on the lower animals in a manner similar to that described by Dr. Harley. No considerable result was obtained except in one instance' and in this the poison was administered by the mouth. He (Dr. Sibson) considered, however, that in this case some accidental abrasion had occurred. In his opinion tetanus could only be overcome by giving such a dose as would absolutely require the employment of artificial respiration. For years past he had been prepared to sustain the responsibility of employing artificial respiration in cases of hydrophobia; but hitherto he had no opportunity of testing its effects. The woorara poison he considered offered the best chance of success, from its complete destruction of the symptoms of the disease, for some hours at least. He therefore trusted that it would yet be tried, combined with artificial respiration, in some cases of hydrophobia. He was apprehensive, however, that tetanus would be found to be too severe a malady to be overcome, without, indeed, the means resorted to should destroy the patient.—*Lancet, Dec.* 3, 1859, *p.* 562.

13.—*A few particulars respecting Woorara.*—M. BONVIER, relying upon a work published by M. Reynoso, gave, at a late meeting of the Surgical Society of Paris, the following particulars respecting the composition of woorara.

There is a true and false woorara; the two are very different, but extremely difficult to distinguish from each other. The true woorara, presents, moreover, several species. This substance is, therefore, not always obtained of the same strength; it comes from different countries, and is extracted from one or several plants, which contain one identical principle, the character of which is to cause death when injected into the blood, and to be innocuous when taken into the stomach.

There is, however, one sort of woorara which acts on the gastric mucous membrane of certain animals, at given ages; which circumstance would tend to show that much difference exists in some samples of woorara.

It is well known that it is not always prepared from the same plants, nor from plants of the same nature. It has even been shown by M. Chombrook, that one kind of woorara is obtained from a great number of plants, almost as great as the number of ingredients entering into diascordium or theriacum.

Amongst the plants used are some strychneæ, but the rest has not as yet been determined botanically. It is doubtful whether snake poison is mixed with it.

Gunelli was the first who, in 1758, insisted upon the innocuous nature of woorara when taken into the stomach. Lacondamine and Humboldt corroborated his statements. Man can eat with impunity animals killed by woorara. Death by the poison occurs generally by paralysis of the motor nerves.

It is of importance to try the woorara in various manners before administering it to a patient, and to ascertain whether it produces no poisonous effects when introduced into the stomach, and also whether it paralyses motor nerves without affecting the nerves of sensibility. These precautions are indispensable, for there is a kind of woorara which may kill by gastric absorption.

As to the occasional inefficacy of the poison when inoculated, the experiment of M. Dequise may be mentioned. This surgeon had been given, by a traveller, a quiver full of arrows said to be poisoned with woorara. He found, however, on trying them upon a dog, that they produced no effect. If we consider the woorara as an extract, such a result need create no surprise, as extracts are very liable to change.—*Lancet, Dec.* 3, 1859, *p.* 567.

14.—ON TETANUS.

By John Hilton, F.R.S., Surgeon to Guy's Hospital.

[The following clinical remarks were made by Mr. Hilton on a case of tetanus, which lately died in Hospital.]

This disease may kill by spasmodic action of the muscles of the larynx, by spasm of the heart, or by general exhaustion of the nervous power. I once opened the larynx between the cricoid and thyroid cartilages, in a case of tetanus, in which immediate death seemed to be threatened by the closure of the larynx. The patient breathed freely through a tube introduced into the larynx, and the whole of the laryngeal distress ceased. The general spasm was much subdued during several hours, and then returned in severity, and he died suddenly, during a paroxysm of spasm affecting the heart, as proved by the post-mortem; for the heart was found firmly contracted, and nearly free from blood. Only a very small quantity was found in the right side. The mode of death in this case was spasm of the heart. The patient, whose case forms the basis of this lecture, died from exhaustion, or gradual subsidence of nervous power.

It is reported in the case before us that trismus or lock-jaw, was an early symptom. It is not at all necessarily the first indication of tetanus. Although the disease is called "lock-jaw," it often happens that twitchings of the muscles in the trunk or extremities manifest themselves as symptoms of tetanus before locked jaw; still it is an early and very prominent and almost constant symptom. What is the explanation of this local symptom showing itself usually so early? Experiment indicates that the grey matter of the interior of the spinal marrow is probably the structural seat of tetanus. The fifth nerve, or nerve of mastication—the one involved, and which must be the direct cause of trismus—has a larger connexion or continuity with the grey matter of the spinal marrow than any other nerve in the human subject; and in this fact, perhaps, lies the explanation of the early symptom of locked jaw; and no doubt the firm closure of the lower upon the upper jaw depends on the relative greater strength of the muscles closing the mouth, as compared with those which depress the jaw. It is curious to observe the gradual ascent of the cause of tetanus—to see how the disease encroaches upon the higher or anterior nerves of the base of the brain, ultimately reaching the third cerebral nerve. Then the muscles which are supplied by this nerve, become tetanic, and cause retraction of the globe deep into the bony orbits.[*] I have repeatedly observed this latter symptom in tetanus; and it is, I believe, always a very serious indication; for it points to the great extent of the structural lesion in tetanus; although what may be the exact character of the pathological state is not explained. *Med. Times and Gazette, March* 3, 1860, *p.* 219.

15.—*Case of Traumatic Tetanus treated by Internal Use of Chloroform.* By Dr. A. Dick, Buenos Ayres.—[The patient was a mulatto sailor, aged 32. Admitted into British Hospital, Buenos Ayres, under Dr. Dick, Dec. 27. 1858, with two slight incised wounds, one in the scalp, the other in the hand, inflicted five days before. On Dec. 31, premonitory symptoms of tetanus appeared. which on the following day were more marked.]

Jan. 2. All symptoms aggravated, and, in addition, frequent spasmodic attacks, amounting to opisthotonos. During Dr. Dick's visit, had three of these paroxysms, the least touch or breath of air serving to produce them; profuse perspirations. Ten drops of chloroform every twenty minutes. The patient being minus a tooth, it was administered through hiatus. Shortly after administration of chloroform the paroxysms ceased, but the tenseness and rigidity of cervical and abdominal muscles continued.

Jan. 3. Pressure on abdomen did not produce spasms, which had previously been the case, and observed by Dr. Dick in all cases he has seen. Chloroform, gtt. xxx. every half hour; olei croton. tig. gtt. iv., his bowels not having been moved since 30th.

Jan. 4. No paroxysms; muscles still hard and unyielding; quantity of dark fetid matter passed from bowels. Chloroform, gtt. xxx. as before.

Jan. 5. Pulse less rapid; no paroxysms. Continue as before.

Jan. 6. Id., id. Croton oil repeated. The same treatment continued up to night of 12th, *when the attendants having neglected to give chloroform, he had a very severe attack of eclampsia.* Neglect being guarded against, he had no return.

Jan. 14. Has slept for first time since invasion of disease, and bowels begin to act naturally.

From this time the symptoms began to mitigate. He had less difficulty in swallowing; the cervical and masticatory muscles became slightly moveable.

Jan. 23. Pulse very weak. Expresses desire for animal food. Chloroform, gtt. xxx. every two hours, to be discontinued after midnight.

Jan. 30. Muscular movements free and almost natural, save occasional convulsive twitch in wounded arm; speech not yet quite natural.

Feb. 10. Still in hospital; weak, but otherwise in good health.—*Edin. Med. Journal, Jan.* 1860, *p.* 640.

16.—ON EPILEPSY.

By Isaac Pidduck, M.D., London.

[Dr. Pidduck remarks that morbid anatomy failing us in our study of this disease, we are driven to a careful study of its symptoms, in order to arrive at a successful mode of treatment. The subjects of idiopathic epilepsy are for the most part remarkable "for the large size and high temperature of the head."]

The symptoms of epilepsy are those of a sudden explosion of accumulated nervous energy. From the periodical recurrence of the fits in many cases, it is inferred that the accumulation of nervous energy (*dynamis neurotica*) goes on for a definite time in the brain and spinal cord, till at length an explosion ensues upon the muscles of voluntary motion, which are thrown into violent action, and by this means the accumulation is exhausted. This explosion is followed by languor, and frequently by a state of coma, or prolonged sleep—indubitable signs of exhausted nervous energy.

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[*] Since delivering this lecture, I saw. with Mr. Mayor, of Park Street, a horse suffering from tetanus; and he observed that as the tetanus goes on, we shall almost lose sight of the eyes, they will become so deeply buried in the orbits.

There are two modes of preventing this gradual accumulation and sudden explosion of nervous energy, constituting the fits of epilepsy—namely, the natural and the artificial. The artificial is by the use of those medicinal agents which keep up a constant involuntary action of the voluntary muscles, such as strychnia internally, and electro-galvanism externally. The natural mode is by the constant exercise of the voluntary muscles. By these means the recurrence of epileptic fits may be deferred almost for an indefinite period—the next best thing to a cure. To the artificial mode there must, of course, be a limit; but not to the natural. By cold affusion over the head daily, to diminish its heat, and by the administration of strychnia, so as to produce a decided twitching of the muscles, the fits, of monthly, weekly, daily, and hourly recurrence, have been deferred for several months, and the same favourable result has been obtained by electro-galvanism.

But the most permanent benefit has been obtained by the natural method, which is the safest and best. In proof of the accuracy of this theory and the success of this practice, the cases of three of the greatest generals that ever lived might be adduced—Julius Cæsar, Napoleon, and the Duke of Wellington. It was in the later periods of their lives, after their active career was ended, that these giants of nervous energy became the subjects of epilepsy. In the case of our own great Duke, the recurrence of the fits was prevented by his temperance in living, by the activity and simplicity of his habits, by the multiplicity of his avocations, and by the hardihood of his amusements and pursuits.

The example of the Duke of Wellington is proposed for the imitation of epileptics who are desirous of enjoying immunity from the fits, in preference to all artificial modes of cure. The confinement of insane epileptics in asylums, though in many cases necessary for the sake of others, is highly prejudicial to the patients themselves, (unless accompanied by hard labour,) as leading to a life of inactivity, which favours the recurrence of the fits. That the cerebral disease found in some cases after death is the effect, and not the cause, of the paroxysms, there is little doubt. It is upon the principle of rousing the dormant nervous energy, and promoting its expenditure, that some of the remedies said to be successful in the cure of epilepsy have operated, such as drinking the warm blood of a slain gladiator,—a miserable remedy, only rendered tolerable by a more miserable disease,—being stroked by the hand of a criminal imediately after execution, swallowing live spiders, &c.

Upon the same principle, paroxysms of hysteria, which, in some cases, so closely resemble those of epilepsy as with difficulty to be diagnosed, are frequently prevented—namely, by making an impression upon the nervous system through the medium of the mind or body, by which the patient is roused into activity and consequent expenditure of the nervous energy.

As strychnia is advised for a remedy in epilepsy, it may be a safeguard against its poisonous operation in an overdose, to mention that camphor is the appropriate antidote to this poison. The fact was communicated by the writer in an article published in the 'Lancet.' He is induced to mention it again in consequence of a case of poisoning by nux vomica recorded in that journal for Oct. 22nd, commencing with this statement: "Nux vomica is one of those poisonous substances for which we unfortunately possess no antidote;" and, again, speaking of poisoning by strychnia,—"There is no remedy for it." Camphor, in five-grain doses, dissolved in mucilage, puts a stop to the tetanic spasms, and gives time for the action of the stomach-pump or emetics, if there is reason to believe that any of the poison is retained.—*Lancet, Dec.* 31, 1859, *p.* 663.

17—PARALYSIS AGITANS REMOVED BY THE CONTINUOUS GALVANIC CURRENT.

By J. Russell Reynolds, M.D., Assistant Physician to the Westminster Hospital.

[The patient was a married man and father of twelve children, aged fifty-seven. A carpenter by trade. For the last five years he has had much anxiety with regard to his children, though it is not certain that this is the cause of his malady. Occasional tremor of the right arm and leg appeared first, about two years since, and seemed to be increased by anything requiring expenditure of nervous power. These symptoms have much increased lately. The arm is worse than the leg; and in it the motion most constant and most extensive, is that at the shoulder joint.]

The involuntary movement of the arm can be arrested by his laying on the sofa, and pressing the forearm against the ilium; but any attempt to move the limb voluntarily at once reproduces the shaking, although he remains in the recumbent posture. The movement is, moreover, instantly arrested by my firmly grasping either the forearm in any part of its upper two-thirds, or the arm in its lower third. This is not a mere mechanical arrest of the movements, for it cannot be effected by holding the wrist; and the jerking recommences, if, while the extremity is grasped in the manner described, the patient makes any attempt at a voluntary movement. The pressure is not painful, nor is it so directed as to arrest the circulation.

The mental condition of the patient, and his general health, appear unaffected.

Sensibility is unchanged in the right upper extremity; there is no deviation of the tongue, nor distortion of the features. He can walk well, and without dragging either leg, there is occasionally tremor of the right leg.

A continuous galvanic current (direct) was applied to the arm and forearm, the movements of the latter being at the time arrested by pressure. At the end of five minutes he could execute voluntary movements without the least tremor, and emotional excitement failed to reproduce the jerking. The temperature of the two arms, examined after the current had been passing for half an hour, was equal. The involuntary movements did not return until three hours after the current was discontinued; they then reappeared, and continued throughout the evening; stopped at night, but returned on the following morning.

October 6th. The current was applied while the arm was in violent movement, but in two minutes it became perfectly still. Application continued for an hour.

7th. Last evening there was no jerking nor tremor for five hours after the current was discontinued; then it commenced, but stopped spontaneously in about half an hour, and during the remainder of the evening there was nothing more than very trifling tremor. The jerking has returned this morning, but is much less than on the first day of observation. There are but twenty alternations in fifteen seconds, and the range of movement is from three to four inches. The movement therefore, is only ·86 feet per second—less than one-eighth of what it was three days ago.

The current was applied on the 7th, on the 8th, and 10th, and after the 10th—*i. e.*, after five applications—the spontaneous jactitation completely ceased. When any weight is held in the hand, and it is lifted towards the mouth, there is tremor; but this is slight, is not more than has occurred for the last two years, and it immediately ceases when the effort is discontinued. The arm and hand are weak; every movement can be executed by them voluntarily, but such movements are feeble.

28th. Has written me a letter in a good and legible hand.

The current was applied about every other day, for an hour, until Nov. 10th, and during this time there was steady increase in the power of the limb, and the jactitation did not return. No medicine of any kind was given.

November 12th. Quinine and iron was ordered.

15th. W. F.—— is in perfect general health; there is no jactitation, and only the slight tremor already described when the hand with something in it, is raised towards the mouth.

The current employed in this case was derived from Pulvermacher's chain battery of 120 links.—*Lancet, Dec.* 3, 1859, *p.* 558.

18.—*On Electricity as a Means of Diagnosis in Cases of Paralysis.*—[The following extract from a work lately published by Dr. ALTHAUS on 'Medical Electricity,' gives shortly and clearly our knowledge at the present time on the vexed question of electricity as a means of diagnosis in cases of paralysis.]

We have thus arrived at the result that the muscles of paralysed limbs may present three different conditions when subjected to the action of the electric current, and that this may enable us, in certain cases, to form the diagnosis of the paralysing lesion.

1. If the excitability of the muscles—or rather the polarity of the motor nerves—be *increased* in the paralysed limb, the case is one of *cerebral paralysis*, connected with an irritative lesion within the cranium.

2. If the excitability of the muscles be nearly or totally *lost*, we have, in all probability, either *lead palsy* or *traumatic paralysis ;* but it must be kept in mind that certain hysterical and rheumatic palsies of long standing present the same peculiarity ; and that it may also be found in cases of disease of the brain and cord.

3. *If paralysed muscles respond readily to the electric current*, there is no lead in the system, nor is the connection between the motor nerves of the paralysed muscles and the cord interrupted ; but if such cases are of *long standing*, they are due to *brain disease ;* and if they are of *recent standing*, they are generally instances of *hysterical*, *rheumatic*, or *spontaneous* paralysis. —*Dublin Quar. Journal Nov.* 1859, *p.* 424.

19.—ON THE IMPORTANCE OF FREE RESPIRATION IN CERTAIN STATES OF THE BRAIN.

By W. C. HUNTER, Esq. (Being an abstract of a Paper read at the Western Medical and Surgical Society, January, 1860.)

The necessity of attention to the respiration was considered chiefly of value—1st, in coma, from injury or disease, sanguineous or serous ; 2ndly, in narcotism of the brain ; 3rdly, in epilepsy ; 4thly, in certain functional cerebral derangements, where a tendency to headache and lethargy exists.

The strictly anatomical connexion of the lungs with the brain having been alluded to, it was shown how, pathologically, disease of either of these organs might kill by its effects upon the other.

1st. Cases of coma from cerebral injury and apoplexy were then cited, illustrating how death generally took place by apnœa. The practical point, therefore, indicated was that life might not only be prolonged but saved by attention to the respiration, even in cases apparently the most hopeless.

2d. In narcotism. The greater the insensibility in narcotic coma, the more the respiration is affected ; the more therefore it requires to be watched. Coma and narcotism both kill by the lungs ; but several points of difference were shown to exist between the apnœa of coma and that of narcotism. In the apnœa of coma, death is not generally (or at all events primarily) due to cessation of respiratory action—*i.e.*, muscular paralysis, but to lung paralysis, evinced by extreme and rapid congestion of the lungs, accompanied by rapid effusion into the pulmonary air-cells and bronchial tubes. The death is apnœa by effusion, unless it can be warded off. The treatment for the apnœa of coma is venesection, to relieve the congestion of the lungs, and prevent its further formation ; but more especially important is the prone position (the prenopnœa of Dr. Marshall Hall), by which an enormous amount of bronchial effusion may be got rid of. Without the prone position, the brônchial effusion may so accumulate that the patient with coma may, in reality, die of narcotism from non-eliminated carbonic acid gas. Artificial respiration may not be necessary. A case of apoplexy of the medulla oblongata and one of consumption from injury were related, in which no muscular paralysis existed. In the apnœa of narcotism there is neither the rapid effusion nor the great congestion of coma; but if death ensues, it is chiefly from masculo-respiratory paralysis. The pulmonary congestion is less than in cases of coma, for the narcotic influence diminishes the strength and frequency of the cardiac pulsations, which are unrestrained in coma. In the treatment of narcotic apnœa, venesection is not necessary, nor is prenopnœa, to remove effusion, but it is especially necessary for a different reason—viz.: lingual paralysis. Artificial respiration is here invaluable. If employed, however, without attention to the tongue, it may be useless.

3rd. In epilepsy. The normal state of the respiration is a point in the etiology of this disease requiring, in the author's opinion, careful consideration. Many epileptics have a shallow, contracted, and feebly-acting chest, which predisposes to and must keep up the epileptic tendency. Several patients were alluded to whose respirations were not only very limited in amount, but in number also, being less than one to four cardiac pulsations. This feeble lung action acts injuriously to the epileptic in a two-fold way; it tends to retain carbonic acid in the blood (to which gas many of the symptoms peculiar to the epileptic, besides the seizure, seem attributable); and also prevents the lungs acting freely as diverticula to the cerebral circulation.

4th. Amongst the functional cerebral derangements were mentioned headache and lethargy, which free respiration in the open air would often remove; also cases in which respiration would sometimes, without any warning, become extremely difficult (as if from sudden deprivation of nervous influence); in such cases, fresh air, the inhalation of ether and ammonia, and deep inspirations (forced, if the patient can effect them, assisted, if not), appear to the author to be indicated, and were productive of great benefit in these conditions.—*Lancet, Feb.* 4, 1860, *p.* 120.

20.—ON THE RATIONAL TREATMENT OF DELIRIUM TREMENS.

By Professor Dunglison, Philadelphia.

[In a paper published some time ago, (Retrospect, vol. xxxviii, p. 41), Dr. Laycock asserted his opinion,]

That the delirium and sleeplessness indicate comparatively harmless conditions of the nervous system; that they are usually symptoms of some disease occurring in persons of drunken habits; that they usually cease within a given time, spontaneously; and that the proper method of cure is to treat the general or particular morbid state, whatever that may be, with which the delirium is associated. He further showed that opium and alcoholic stimuli, as generally recommended for this disease, were not only useless but dangerous drugs.

[The present article is a portion of a letter by Professor Dunglison to Dr. Laycock: it shows that the two observers had come to the same conclusion independently of each other. In May, 1842, Dr. Dunglison as Editor of the American Medical Intelligencer, inserted an article in that Journal on the "Eclectic treatment of Delirium tremens."]

We stated that the course pursued by us in the treatment of delirium tremens has been entirely eclectic, in many cases expectant, and that the results have been such as to satisfy us. Under the view which we entertain of the nature of the affection—that the irregularity of nervous action is usually induced by the withdrawal of an accustomed stimulus, and that the recuperative powers are generally entirely sufficient to bring about the necessary equalization—we have treated the mass of the cases which have fallen under our care without either excitants proper, or opiates. In the first instance, an emetic is given at times, if the patient is seen whilst laboring under the effects of a debauch, or any particular reason exists for its administration; and afterwards, a state of tranquility in the chamber is injoined—the intrusion of too much light and noise being prevented; and when the stomach will retain it, gently nutritious and easily digestible diet is prescribed, the bowels being kept open by gentle cathartics;—and this has comprised the essential part of our treatment. In time the hallucinations have disappeared, sleep has returned, and entire restoration has supervened. The preceding remarks are a proper prelude to the statistical account of the Women's Lunatic Asylum, at the Philadelphia Hospital, for the years 1840 and 1841, which is under our charge during the six months commencing on the 1st of November, and ending on the 1st of May, and under that of Dr. Pennock for the other half of the year. It may be proper to add, that, since the 1st of November 1841 to the present time (May 1, 1842), not a drop of alcoholic liquor has been used in the treatment of delirium tremens in the Women's Asylum, although some severe cases in the third stage have occurred, which, notwithstanding, terminated most satisfactorily.

Patients admitted into the Women's Lunatic Asylum of the Philadelphia Hospital, in the year 1840.

		Cases admitted.	Cured.	Died.
Intoxication,	. . .	25	25	..
Delirium Tremens,	1st stage,	34	34	..
Do.	2d stage,	10	10	..
Do.	3d stage,	4	3	1

The fatal case was not seen by us. The patient died on the morning after her admission into the hospital, and had been treated in the city for nearly a week previously.

Year 1841.

		Cases admitted.	Cured.	Died.
Intoxication,	. .	19	19	..
Delirium Tremens,	1st stage,	21	21	..
Do.	2d stage,	9	9	..
Do.	3d stage,	6	6	..

In the third edition of my "*Practice of Medicine*" (Philadelphia, 1848), I state further: "A more recent authentic abstract of the number of patients admitted into the same Asylum from the 1st of November 1844 to the 4th of February 1845, exhibits that 32 cases were received, 18 of which are classed as intoxication. Of these not one died. The treatment here again was eclectic, often expectant, and not a drop of alcohol was given."

The results of the above plan of treatment were referred to some time ago, in an interesting pamphlet on *Rational Medicine*, by Professor Worthington Hooke, of New Haven; but as I had doubts whether either the *Medical Intelligencer* or my *Practice of Medicine* is in the libraries of Edinburgh, I have copied from them what bears on the rational view which you have embraced of treating delirium tremens. As I have remarked in the latter work: "It has, in the first place, restored the individual to health, not, perhaps, as rapidly as either brandy or opium, but more permanently. The term, 'restoration to health,' is hardly, indeed, applicable to the change effected by the former remedy. The patient is merely placed in the condition in which he was before the stimulus was withdrawn; and as he 'was restored' by the brandy, he is apt, as before remarked, to regard it as indispensable to his healthy condition. In the 'total abstinence' plan, however, the habit of drinking is broken in upon; and even if it should require a short time longer to restore the individual, there is the consolatory reflection, that delay is not useless, and every day's privation of the wonted stimulus diminishes the feeling of necessity, and the desire for it. One evidence of the good effect of the course is, that they who are dismissed cured, rarely or never return to the wards. This is an observation that has been made at the Philadelphia Hospital; and as it concerns paupers, it is probable that the cures are real and permanent, for, were it otherwise, they would, in subsequent attacks, be compelled, in their destitution, to seek the wards of the same excellent charity."—*Edinburgh Med. Journal, April* 1860, *p.* 924.

21.—ON THE PATHOLOGY AND TREATMENT OF IDIOPATHIC PERIPHERAL NEURALGIA.

By HARRY LOBB, Esq. (Read before the Harveian Society.)

The author commenced by dividing neuralgia into (1) Central, (2) Peripheral, and (3) Reflected. Of the central, arising from disease of the brain or spinal cord, he did not speak. The third division he proposed reserving for a future communication. The second was divided into Idiopathic, Traumatic, and Neuromatous. Idiopathic peripheral neuralgia was described as a stabbing, darting pain referred to the course of a nerve, shooting down the nerve like lightning, coming on suddenly, lasting but a moment, and repeated at intervals; the pain is so acute as to be unendurable if continuous. The part or limb affected with this form of neuralgia is colder, and the skin supplied by the neuralgic nerve is more or less numb, not tender to the touch, the patient liking to be rubbed, and frequently grasping the part with the hands and pressing it. If the neuralgia has lasted any time, there is more or less paralysis in the muscles supplied by the accompanying motor nerve. The author then gave, at some length, his views upon the generation and distribution of nerve-force from the capillary circulation, in order to make his description of the pathology of this form of neuralgia intelligible. He described a sentient nerve as a conductor to the brain of sensations taking place at the periphery: neuralgia is not, therefore, a hyperæsthesia of its healthy function. During health, a nerve has no sensation proper in itself; and if it be struck, cut, or torn, the sensation is referred to the parts to which it is distributed: but in this form of neuralgia it is the nerve itself to which the pain is referred as darting up and down its course. A sentient nerve was likened to an iron conducting wire of a galvanic battery, which, if of a certain uniform diameter, conducts a given quantity of galvanism without being perceptibly affected by its passage; but if a portion of the wire be much finer than the rest, it becomes red hot, being unable to conduct the whole of the galvanism, the remainder correlating into heat. So in a nerve: from mal-nutrition, it is unable to conduct normal sensations to the brain; the nerve-current, by affecting the polarity of the nerve itself, gives the idea of pain in that portion of the trunk of the nerve, its peripheric terminations at the same time being numbed. The author considered the indications for treatment, therefore, to be increased, and healthy circulation, arterial and nervous. After enumerating the usual methods of treatment, he considered the only agent capable of carrying out these indications to be the continuous galvanic current; and he recommended, as the only apparatus that can be adapted to the surface of the body, and capable of generating a sustained current, the Pulvermacher chain—producing a continuous current of galvanic electricity in one uniform direction, mild, yet sufficiently energetic for medical purposes. Mr. Lobb then described the method of applying the chain, and exhibited some experiments with its aid; as the decomposition of water, contraction of muscles, &c. In the treatment of idiopathic peripheral neuralgia, he looked upon the Pulvermacher chain as a specific. Immediately upon adapting the excited chain to the part, a genial glow is felt —not mere warmth, but a sense of vitality in the part; the patient is aware of a life-giving agent, and immediately says that he is relieved; the neuralgia disappears, and sensitiveness of the surface returns.—*Lancet, Dec.* 10, 1859, *p.* 589.

22.—ON THE PHYSICAL CONDITION OF THE MUSCLES DURING MYALGIA.

By Thomas Inman, M.D., Physician to the Liverpool Royal Infirmary.

[Dr. Inman has, in several previous papers, (see 'Retrospect,' vol. xxxviii, p. 29, and vol. xxxix, p. 77) drawn the attention of the profession to the fact, that a vast number of symptoms once supposed to indicate inflammation of internal organs, rheumatism, neuralgia, hysteria, and a variety of other diseases, are due in reality to painful affections of the fleshy or tendinous parts of muscles. In myalgia from excessive exertion, various appearances are observed in the muscles. In the bodies of those who have died from tetanus,]

Microscopic inquiry shows that their blood-vessels have been emptied, that many of the muscular fasciculi have been ruptured, and that each of these fractures is attended with laceration of blood-vessels, and extensive extravasation of blood between and around the broken fibres.

[Dr. Inman then gives an account of an examination of a hare which had been coursed to death; the fibres were lacerated; blood effused between them, and between the ruptured ends of the fibres, and the transverse markings almost gone.]

This, then, being the condition to which the muscles of a hare are brought from excessive action, it is not straining analogy too far to assume that in a man who had overstrained his muscles as "poor Puss" had done, a similar state of things would be found, had we an opportunity for examining him; and it is easy to understand how such a condition would be attended with cutaneous tenderness, prolonged soreness, great disinclination to use the bruised organs, and severe suffering if the action was continued daily. We can readily see, with this physical condition of the muscles, how inflammation might readily supervene, in cachectic constitutions, upon the injury done, as an abscess occasionally follows a bruise, and how it would be difficult for the muscles to recover their healthy state without a large amount of rest, or a great reparative power in the system, or a long apprenticeship of pain. If the foregoing reasoning be valid, it is equally valid to assume that an amount of exertion, less than that we have described, might produce an analagous state of things, though of less severity. If so, we may conclude that in such myalgic cases as we have previously described, there is less or more rupture of muscular fibre, laceration of blood-vessels, and extravasation of blood.

It must next be noticed that the above phenomena are due to *excessive* muscular action, and we must pause to inquire what is the signification of those words. In a previous communication, I have endeavoured to prove that the standard by which *excess* must be judged is not the general average of human exertion, but the powers of the individual—*e. g.*, a daily walk of twenty miles is not excessive exertion for a country postman, while he is healthy, but a walk of twenty paces is excessive exertion for that postman when ill with typhus. A feeble woman, therefore, is quite as obnoxious to suffering from over-exertion, though she may never leave the house of her bed-room, as is a healthy man or a hunted hare.

Our chain of argument would be strengthened if we could show that the muscular fibres will fracture and the blood-vessels give way in the weakly, under a less amount of force than they do in the strong. I am unable to give demonstrative proof of this, and must content myself with the best available analogy.

1. Toleration of muscular exertion is in direct proportion to a muscular *tone*—*i. e.*, firmness, hardness, and development. In strong muscles, "soreness" is unknown, except from such very prolonged exertion as "pumping at sea," a thirty hours' "march," a long day at the oar, &c.

2. Myalgic pains are severe in the direct proportion to the atony or softness of muscles; they are, therefore, most common in those whose system has been reduced by cachexias of various kinds.

3. Experience in the dead house and dissecting-room tells us that the muscles of certain corpses give way sooner than those of others; *e. g.*, it is

not an uncommon thing, when dissecting the upper extremity, to find that both the pectorals are torn through by the simple weight of the arm. This easy rupture occurs only in those dying of cachexia. In such corpses, too, it is that the blood-vessels are commonly broken by "injection." We further know that those hearts are most liable to rupture whose tissue is pale, soft, and friable, and whose fibres are indistinctly marked with striæ; and we also know that when cardiac rupture does occur, it is the result of very trifling over-exertion, if there can be said to be over-exertion at all. The ecchymosis under the skin, and the hardness and other phenomena characteristic of myositis in the legs of scorbutic seamen, lead us to infer that a condition of things exist in them resembling that recognised in the tetanic patient or the hunted hare; and if it do exist, it cannot be from absolutely excessive muscular action, but from exertion disproportionate to the contractile power.

4. Experience among the living tells us that the blood-vessels are sooner ruptured in some persons than in others; that is to say, that some will have ecchymosis from a touch, a pinch, a blow, or other trifling injury; while others will endure far more violence without any extravasation of blood whatever; and, if we classify these, we shall find that the tendency to "breaking of blood-vessels" is great or small according to the cachectic condition of the individual, and *vice versa*. Thus, accidental hemorrhages into the muscular, mucous, submucous, cutaneous, and subcutaneous parts, are common in scurvy, in purpura, and in typhus; they are not uncommon in phthisis, scarlatina, Bright's disease, and general asthenia; they are very rare in the healthy countryman or robust sailor. It is amongst the former that we find myalgic pains are the most severe, prolonged, and intractable. If, then, we find that the cachectic are, *cæteris paribus*, far more liable to rupture of muscular fibre and of blood-vessel than are the strong, we are justified in the belief that such fracture will occur from a smaller *absolute* amount of muscular effort in the former than in the latter (the *relative* muscular effort is perhaps *greatest* in the weak); consequently, that there is nothing improbable in the belief that *excessive* exertion in the delicate and feeble, even though the effort seems to us very trifling, will produce similar painful effects to excessive exertion in the strong. But it is a physiological fact of great interest, that, as a general rule, the weakly suffer pain from any given cause to a far greater extent or proportion than the strong; *e. g.*, a bad tooth does not trouble us at all while we are well; but if we get "pulled down" in any way, that same tooth will produce both odontalgia and neuralgia, both of which complaints may be removed by full doses of quinine or other tonic, which builds us up again, the bad tooth still remaining the same. Consequently, we judge that the muscular and vascular ruptures, which are the cause of suffering in the strong, are the cause of a proportionably greater suffering in the weak.

If the preceding account of the physical changes produced in muscles from excessive exertion be accepted, it disproves most completely the idea of Dr. Handfield Jones that the pains and other phenomena of myalgia are simply "neurolytic" and dependent on "nerve-debility;" and that of others, which attributes them to hysteria, feminine fancy, or the desire of sympathy. It gives us a good ground for the belief that the sufferings described are *real;* and it unequivocally points to a plan of treatment of which practice has already proved the value; viz., that the muscles should have time to rest, and thus be enabled to repair fractured fibres, and "take up" extravasated blood; it leads us to expect that in cachectic subjects, ecchymosis may pass on to inflammation, and this to abscess or disorganisation; and it still farther points to the necessity there is, in all cases of myalgia, for improving muscular "tone" and increasing vascular tenacity. It demonstrates that, as a general rule, champooing, friction, galvanism, or other means which would aggravate muscular exertion, blisters or other counter-irritants, or leeches or local blood-letting, cannot be expected to do more good than they do in common ecchymosis.

In conclusion, I would only add, that the preceding remarks apply to myalgia of the fleshy portion of the muscles alone, and are not applicable to the pain, &c., which accompanies a prolonged strain upon their fibrous parts.—*British Med. Journal, Feb.* 4, 1860, *p.* 87.

23.—CASES TREATED BY THE HYPODERMIC INJECTION OF ATROPINE.

(Under the care of Dr. Cowdell, Dorset County Hospital.)

The following cases I beg to submit to the notice of the Profession as evidence of the great practical value of the hypodermic injection of atropine in sciatica, lumbago, and, in fact, in all external aches and pains. No claim to originality is made, since it is Dr. Wood's method, as modified by our French brethren, and reported in the 'Journal de Medécine et Chirurgie Pratique.' Dr. Wood's morphine injection has been practised on several patients without the results now obtained from the use of atropine. The morphine procured sleep in from three to ten minutes, but when the narcotic effect had passed off, the pain returned as bad as before. The atropine causes slight drowsiness, dryness of the throat, and dilatation of the pupil; the only symptom of moment being the disappearance of the pain. The sulphate of atropine being more soluble than the alkaloid itself, is preferred; the strength of the solution is gr. ij. to ℥j. of water. The instrument employed is an ordinary nævus injecting syringe, consisting of a graduated tube with screw piston, and a hollow needle which screws on to the tube. The needle is thrust under the skin as near the seat of pain as possible, and from ten to thirty minims of the solution gradually injected.

The cases subjoined will show the result.

Case 1.—S. E., aged 20, a domestic servant, admitted September 29th, 1859, and stated as follows:—"Had an attack of acute rheumatism five months since, was delirious at intervals during three weeks, and, when recovering from the rheumatism, sixteen weeks ago, had an acute pain in the hip, which did not yield to the leeches or blister which were repeatedly applied." On admission she was extremely anæmic, and experienced acute pain in the region of the sciatic nerve, increased by pressure. There was no hip affection: contour of hip normal; muscles very flabby; ham-strings contracted; knee bent; and the foot drawn up. She had during the whole time been unable to walk without crutches. Ordered meat diet. Ol. morrhuæ and a draught containing iodide of potassium.

30th. Passed the night as usual, without sleep, and in agonies of pain. Injice sol. atropii. ♏xxx, in par dolent.

October 1st. Slept well all night; felt immediate relief after the injection; says she has not had so good a night for sixteen weeks.

4th. Hip is sore, but no pain unless pressed. Rept. inject. horâ somni.

5th. The pain is entirely gone, not even the soreness remaining. Omit. mist. ℞ Ferri. am. cit. gr. iv. ex aq. ter dic.

She was treated for anæmia from this date to Nov. 10th, when the report runs thus:—

"November 10th. Patient has had no pain in the hip since the last injection on October 4th. Two injections only were employed at an interval of three days, the first of which gave immediate relief, the second removed pain and soreness altogether. Can walk with ease; no pain on pressure over the nerve, and she gains flesh rapidly. Discharged well."

Case 2.—H. P., aged 25, plasterer, admitted with chronic rheumatism, December 1st, 1859. He was somewhat relieved by the use of guaiacum and iodide of potassium, which was continued until January 12th, 1860, when complaining of an additional pain in the lumbar region which caused him to walk with his hands on his knees, the atropine injection was used.

January 13th. The pain in the back was greatly relieved last night by the injection. Omit. omn. meda, ℞ Syr. ferri. iodid. ʒij. ter die.

15th. Very much better; no pain in back; can walk quite erect; has a pain in the tensor [illegible]'s muscle. Inject. repetatur.

17th. Pain gone from the tensor vaginæ femoris muscle, but experiences slight pain in the gracilis and inner hamstrings. Repet. inject.

26th. Feels well and able to work; no pain whatever; says he is cured by the injections. Discharged well.

Case 3.—E. D., aged 50, nurse, admitted January 9th, with all the ordinary symptoms of acute sciatica of one day's standing. She could not stand. Agonizing pain in back and hip, and leg as far as the great toe. Ordered cal. cum opii. and the atropine injection at night.

13th. Found much benefit from the injection last night, leg much better, but still a pain on pressure.

14th. Used injection again last night, and this morning the pain is entirely gone; no pain even on pressure.

19th. She continued improving, and on this day she was discharged well.

The peculiarity in this case is, that she was admitted the morning following the commencement of the attack; and therefore it is worthy of notice that neither abstraction of blood, nor counter-irritation was resorted to, but the injection of atropine only, the effect of which upon the disease is remarkable.

Case. 4.—W. S., aged 38, labourer, admitted February 9th, 1860, with sciatica on the right side, which had existed since October, 1859. He walked, or rather limped, into the ward with both hands resting on a stick between his legs, dragging his right leg after him. Pain was increased by pressure over the nerve, from the lower leader of Glutæus maximus muscle to the popliteal space. Ordered the atropine injection at bed-time, and a mixture of iodide of potassium.

10th. The injection last night removed the pain; only an ache is experienced to-day, instead of the shooting pain of yesterday. Repet. inject. h. s.

11th. Feels no pain or soreness whatever in the hip. Pressure even into the sciatic notch gives no pain, and he can walk with as much freedom and ease as before the attack. He was discharged well on the 16th, seven days after admission.

Case 5.—D. S., aged 56, stone-mason, admitted February 9th, 1860, with sub-acute sciatica of fourteen day's duration. Walked with difficulty even with the assistance of a stick: could not bear the pressure of a finger over the nerve. He had also bronchitis of the larger tubes, accompanied with profuse expectoration. Ordered the atropine injection at night, and a squill and henbane mixture for cough.

10th. Cough less troublesome; the injection gave immediate relief; very little pain is now experienced even on pressure over the sciatic nerve, and he can walk without pain, but has a limp. Continue.

12th. Has now no pain in hip even on pressure; walks well and with ease; cough better.

From this day to the 23rd he was treated for his chest affection, being still free from pain in the hip. Discharged well on the 23rd.—*Med. Times and Gazette, March* 17, 1860, *p.* 267.

24.—A CASE OF POISONING BY STRYCHNINE, AND RECOVERY.

By George Bennet, M.D., F.R.C.S., Sydney, New South Wales.

[The patient, a lady aged forty-two, had, in a fit of despondency, taken the entire contents of a packet of "Battle's rat poison." Then repenting of what she had done medical advice was sought. Dr. Bennet administered scruple doses of sulphate of zinc, whilst he sent for the stomach pump, but by its arrival the spasmodic movements which had supervened precluded its use. The compound tincture of iodine was then sent for.]

Before it arrived, however, the spasms had increased so violently that life appeared extinct; the face and hands were livid, the eyeballs protruded, and there was violent opisthotonos; pulse not perceptible. The most severe spasmodic action came on about a quarter to eight A.M., and about two minutes after the subsidence of a severe fit, the medicine having arrived, I gave her twenty drops in a wine glass of water. Soon after a very violent fit came on, and in this the respiration was so feeble that we all considered she was dead. However, she again rallied; and as soon as she was capable of swallowing, being about ten to fifteen minutes after the administration of the first dose, I gave her thirty drops of the tincture of iodine. She soon had another fit; but it was evident to all about her that it was less severe, and lasted but for a very short period of time. On the fit subsiding, in about ten minutes I gave her another dose of thirty drops of the tincture. The convulsive fits had now ceased, and in about twenty minutes from the cessation of the last fit, violent vomiting came on; and this being assisted by diluents, the stomach appeared clear of the poison. Now, a question may arise how the iodine acted in this case. Probably by forming an insoluble compound of hydriodate of strychnine, and by relieving the system from the spasmodic action of the poison upon the spinal nerves, the emetics which had been administered were enabled to take effect. She continued free from any further spasm; and, after an hour, I left her. On returning in about an hour, I found she still continued better, with the exception of a few very slight twitchings. She complained of great debility; and the pulse being very feeble, I ordered her nourishing diet of milk and arrowroot, and gave an ammonia mixture, with compound sulphuric ether; and, under their influence, she rallied very rapidly. I remarked that during the convulsive paroxysms no screams were uttered; and on making inquiry after her recovery, she informed me that no pain was felt during the fits, only an inconvenience from the spasms drawing the legs so rigidly down. She also stated she was sensible during the fits to every thing around her, retaining the most perfect consciousness of all that was said or done—was unable to speak, but never felt any inclination to scream or cry out. In the evening she complained of a little soreness of the throat, and a slight feeling of cramps over the lower extremities.

On the following morning (Oct. 14), I found her much better; pulse regular; had passed rather a restless night; soreness of the throat had subsided; and she enjoyed some tea for breakfast. She had no pain, and complained only of a feeling of lassitude. The next day I found she had slept well; but the bowels not having been relieved, an aperient mixture was ordered, which, however, did not operate until the evening. The stimulating mixture was continued occasionally, and afforded great relief from the feelings of nervous debility which came over her. From the 16th to the 18th she improved daily; and on the latter day she was able to leave her room.—*Lancet, Oct.* 29, 1859, *p.* 434.

DISEASES OF THE ORGANS OF CIRCULATION.

25.—ON FIBRINOUS DEPOSITION IN THE HEART.

By Benjamin W. Richardson, M.A., M.D., Lecturer on Physiology, at the Grosvenor Place School of Medicine, &c.

[In a previous article Dr. Richardson considers the structure of fibrinous deposits, and the proofs that these deposits are in many instances laid down prior to death. In the present paper he passes on to consider those conditions of the system which are favourable to the deposit of fibrine, or, in other words, those diseases in the course of which those deposits are formed. To understand this subject clearly, it is first necessary to consider the modifications to which the fibrine of the blood is subjected under various diseased states.]

1. In various conditions the fibrine is, or seems to be, diminished in the blood. These conditions are indicated in diseases in which the hemorrhagic tendency prevails. True typhus, and yellow fever, are diseases of this nature. The effects of many poisons, as the cobra poison, the vegeto-alkaloids, the alkalies, and several organic acids, are manifested in this apparent

deficiency of fibrine. In these cases, the blood, as Plato would express it, "is no longer under the natural guardianship of its fibres; so it exudes into the soft structures, giving rise to petechial spot, black vomit, melæna, hematuria, and, in fact, to any form of hemorrhagic discharge, according to the degree of pathological change.

I have said that, in these cases, the blood seems to be deficient in fibrine. I say *seems* to be deficient, because I wish to guard myself from saying absolutely that the blood is so deficient; and I would impress this qualification earnestly, because, from some late inquiries on the effects of alkalies and alkaloids on blood containing, when drawn, the normal amount of fibrine, the same apparent decrease of fibrine has been produced by the mere addition of these agents in excess to the blood. We need not, therefore, bind ourselves to the idea that, in diseases attended with great fluidity of blood, less fibrine is really produced in the system; we may rather infer that, as all evidence tends to prove, there are conditions in which the blood is surcharged with a body having the power of holding fibrine in solution—of reducing the fibrine, in fact, into a form of albumen. In such condition of blood, whether my explanation of its cause be correct or not, this that I am about to say shall be correct: the blood, while in the condition named, never, either before or after death, yields a fibrinous separation. Ordinarily, it does not coagulate even as red clot, but at most it becomes a thick treacly mass, like newly drawn blood treated with an alkali and afterwards mixed with albumen.

2. There is a second variety of blood, not widely removed from the above-named kind, in which there is nevertheless a peculiar tendency to separation of fibrine; and this without any necessary increase of the fibrine itself, or the presence of those systemic states in which the combustion of the body is increased. The older writers were accustomed to notice this separation of fibrine in scurvy. They spoke of the stream of blood in that disease as having two colours, a purple and a white; and of a loose buffy coat forming on such blood. I am convinced to proof that the condition here expressed exists—exists, sometimes, as it would seem, by hereditary predisposition; at other times, by acquired disease, especially by disease derived from peculiarities of food. I do not pretend to be master of all the facts connected with this subject, but I will narrate such few as I know. I once attended a child covered from head to foot with purpuric spots—a miserable puny child, cold always, and emaciated. This child died suddenly, and I traced the cause of the sudden death clearly enough to a large concretion of fibrine which had been laid down in the right auricle. I also knew an instance of extreme anæmia terminate in a similar way; the only difference being, that the concretion had formed in the pulmonary artery, and not in the auricle. In a case of purpura at this very time under my observation, there is every disposition to effusion of blood under the skin, and large purpuric blotches are produced from the slightest cause; but, sometime after this effusion has occurred, there is a hardening of the matter at the effused point; and it is observable that the blood of this patient, although it is so easily effused, is not incapable of coagulation when it escapes from the body.

3. There is another condition, in which the fibrine of the blood undergoes relative increase. It may be difficult at first to understand what is meant by relative increase. I mean by it then, simply a state in which the fibrine is not abnormally increased, nor the system under any condition for the production of an increase of fibrine, but in which, the fibrine remaining normal, the watery constituent of the blood is below the usual standard. This condition is not uncommon: for, water being the grand menstruum in which the fibrine is held in solution, any reduction of it in amount will favour materially the tendency to deposit. It may seem, at first sight, that this statement runs counter to that which was made above relative to anæmia; but there is no contradiction. Let me explain. Water is, as I have said, the menstruum in which the fibrine is held in solution. But fibrine will not remain in solution in water simply; the water must be the menstruum also for an agent having the power of dissolving the fibrinous constituent. This

agent, as we now have proof beyond every kind of dispute, except sheer cavil, is an alkali of the volatile series. Now, in order that fibrine should remain in solution, there must be a fixed relationship existing between the menstruum, the solvent, and the substance dissolved. If the substance dissolved is but dissolved, and no more, as fibrine is in blood, precipitation of fibrine can be induced by addition of more water, because, in this addition, we distribute the solvent over a larger surface; we weaken, that is to say, the solvent power of the solution; and the result necessarily is, that the dissolved substance is proportionably precipitated. On the other hand, if from a solution of fibrine, as in blood, we draw off by osmotic action the watery part, together with the solvent, the fibrine is naturally increased in relation to the portion of water and solvent left behind. In both cases alike, there is, therefore, a tendency to the deposition of fibrine. Out of the body, I can produce in experiment all these phases; and in the bodies of the dead we constantly meet with the result of these changes, if we are observant observers.

The best marked cases in which the fibrine is found separated after death, as the result of loss of fluid from the system, are those where, during life, there has been an exhausting and rapid flux from the alimentary canal. In cases of cholera, the fibrinous deposits so commonly found after death is formed in this way; and the same may occur in other disorders affecting the alimentary system. I have seen them, as so produced, in death from mesenteric disease; and once in a debilitated person, after a fatal purging, induced primarily by self-dosing and overdosing with black draught. Deposition, brought about by loss of fluid, will further happen from rapid elimination through other channels than the bowels, as from the skin in the colliquative sweating of phthisis.

Once more; the fibrine may be relatively increased, not in proportion to the water, and not in proportion to the mass of blood, but in proportion to its own solvent. The old writers were accustomed to state that concretions were the result of what was called languor of the circulation. We cannot adhere to that phraseology at this time of science, because the phraseology is imperfect; but we may, nevertheless, accept that these writers did truly express a partial explanation of a very interesting fact. In many cases of great prostration of the body and of the powers of the circulation, where, notwithstanding, life is slowly destroyed, the blood making languid way for many hours—in such cases (and I may add, in the majority of such cases), the fibrine is left deposited in the heart. The cases themselves include: (*a*) deaths from shocks, where the life is not extinguished at once, but in which the act of death has been very considerably prolonged, say for a period of from three to four days; here the fibrine may be separated in a firm and decided mould; (*b*) deaths from the prolonged effects of some poisons, which depress the heart without adding any solvent substance to the blood, such as opium; and (*c*) those instances of disease where death is preceded by great degenerative change, and by slow and certain exhaustion. I have met with *ante mortem* concretions in examples of this nature, after death from mere senile decay, fatty degeneration of the heart, and slow alcoholic poisoning. The concretion, however, in these cases, is often but indifferently developed. It is gelatinous in character, very heavy at first from the presence of water, but containing in reality a very small amount of fibrine. Dr. Gould, whom I have noticed once before, laid great stress on debility as a cause of deposition. He opined—and the opinion has been reiterated by Dr. Meigs of Philadelphia—that, owing to the stasis produced in the blood during a fit of syncope, the commencement of a fibrinous deposition may be traceable, in some instances, to an attack of syncope. Nor is it to be denied that many arguments lend a colour to this view of stasis as a cause of concretion. The observation made by Lancisi respecting the mode in which fibrine is deposited in aneurism, the first point of deposit in the sac of an aneurism being that at which the stasis is most marked, was alone of sufficient importance to establish the stasis hypothesis for a long period; and the more so, because this hypothesis is not opposed to the truth, but is only faulty in that it does not express the whole truth. The position of the question is as follows:—When blood yet fluid is placed entirely at rest in a closed cavity, and under a pressure equal to that of the circulation, there is no separation of fibrine. On the contrary, blood so enclosed is in the best condition for its fibrine to remain in solution. Here is an argument, then, in direct opposition to the pure stasis view; flatly disproving it, indeed, as a bare hypothesis. Blood, therefore, from which *ante mortem* concretion is formed in the asthenic, is not blood in a state of absolute rest. No; it is blood undergoing slow and languid motion, in process of which the volatile solvent of the fibrine, a product of an active nutrition, is in the first place imperfectly supplied, and, secondly, is eliminated in the expirations of the skin and lungs, to the loss of the blood. The relationship existing between the fibrine and its solvent being thus destroyed, the fibrine is precipitated, and is found in the separated condition after almost all forms of death from asthenia, excepting those where a solvent poison is present as part of the disorder.

4. The last condition favourable—most favourable, to fibrinous separation, is that in which the blood undergoes an absolute increase of fibrine—that condition which the old men were content to designate as disease "attended with sizy blood," and which we moderns, following Franz Simon, call "hyperinosis" or superfibrination. We include under this head a large group of diseases marked by inflammatory symptoms, and always running an acute course; pneumonia; croup; diphtheria; acute rheumatism; erysipelas; inflammatory diseases of all the serous and mucous tracts; the inflammatory reactive fever succeeding on surgical operations; inflammatory lesion in the uterus and peritoneum after parturition; and cerebral congestive excitement.

In thus naming certain local symptoms indicating what we are pleased too often to consider as pure diseases, I write to make the subject intelligible. In my opinion, these local changes are all secondary to one grand systemic derangement, a primary and essential feature of which is increase of fibrine —hyperinosis.

Still further, I maintain that there are cases in which the hyperinosis is not only preexistent to any local manifestation of disease, but in which it is so marked, and the deposition of fibrine in the heart as a consequence of the increase is so rapid, that death takes place from the obstruction, before any local change is developed. Instances of this nature, while they are not by any means numerous, are, as a general rule, overlooked. There are, nevertheless, to be found in our literature certain extraordinary examples of this nature, worthy certainly of notice. One of the very best observers of disease in the last century, Dr. Huxham, in a letter written by him to Dr. Mortimer, describes what was sometimes called an epidemic of cardiac polypi. In this letter, it is narrated that several seamen, brought during cold weather into England from the West Indies, were seized with short coughs without expectoration, continued and violent palpitations of the heart, intermitting trembling pulses, anxiety, and pain and sinking at the heart, so that they could not lie down; they had heavy dead countenances, some pains in the side, but very little fever. Twenty men thus soon died. Two of these men were opened, and in their hearts were found monstrous polypi, tough and adherent to the walls of the ventricle. One of these weighed an ounce. They were on both sides the heart; the largest on the right side.

A still more remarkable illustration of a similar epidemic is described in the *Edinburgh Annals of Medicine* for the year 1800, by another equally good observer, Dr. Chisholm. I have often noticed the valuable paper, but now I will give it almost entire. It is entitled, "A Short Account of the Epidemic Polypus in Granada in 1790." The sufferers were negroes, who were subject to peculiar local influences at the time of the seizure. These influences Dr. Chisholm first describes. The foreground was the open sea, with an extensive and bracing beach of sand. On the left was a hill of considerable size and ascent, the base sides of which, with the reflecting surface of the sea, produced in dry seasons an immense degree of heat. The background was a marsh, extending from the sea to the mountain. From this marsh deleterious vapours were exhaled. Immediately behind the marsh, a ravine began, through which rushed a current of wind of great coolness. The negro houses of the population were built on the hill on the left, chiefly on its slope and towards the edge of the marsh. The negroes were, therefore, at once exposed to excessive heat, a cold chilling current of air, and the miasma of the marsh. Their diet was chiefly vegetable food. They had been employed, immediately before the appearance of the disease, in clearing the surface of the marsh, and in holing land for the reception of cane-plants. They were much given to the destructive habit of eating a species of pipeclay very abundant in Granada.

The disease commenced in the end of September, was most prevalent in the latter part of October, and went off in November. The whole number of the sick was about forty, of whom seven died.

The symptoms were, pain at the pit of the stomach and head, and difficult respiration, attended with dry skin, small quick pulse, and slight dry frequent cough. No febrile heat accompanied these symptoms. The surface, on the contrary, was remarkably cool; but there was a heaviness and dullness of the eye, anxious features, and depressed spirits. This state continued for three days; then the pulse became quick, from 120 to 140, and intermitted, attended with a penetrating pungent heat, which produced a pricking sensation on the hand of the person feeling the pulse.

The disease now became somewhat intermittent, the intermission lasting eight or nine hours. During the paroxysm, the struggle for breath, the aggravation of all the other symptoms, and *the very quick*, interrupted, and evidently visible, as well as audible palpitation of the heart, produced a scene of uncommon horror. The paroxysm was succeeded by a cold clammy sweat, and a state of approaching syncope. The second paroxysm generally put a period to the existence of the patient. The disease was also distinguished, during the latter stage, by an almost constant, disagreeable clammy sweat, overspreading the face, the upper extremities, and the body as low down as the scrobiculus cordis, all below remaining parched in a most remarkable degree. The disease seemed sometimes inclined to terminate by a metastasis. One instance of this was remarkable, wherein a patient, after labouring under all the symptoms peculiar to disease before the intermittent period, found himself all at once, and without an evident cause, relieved of them; but he perceived at the same instant an excruciating pain a little above the elbow, and nearly about the middle of the thigh. He continued ever afterwards quite free of all the symptoms of polypus; but they were succeeded by a large abscess in the parts in which he felt the pain. That in the arm disappeared gradually; but the other became so large as to occupy the whole of the under part of the thigh. The cure was effected by passing a seton through the whole length of the tumour; by the use of two dozen of Maderia wine, bark, and a calomel pill with opium, three times a day.

The audibleness of the palpitation of the heart in these cases, Dr. Chisholm remarks, may be considered as an exaggeration; but in one instance, particularly, a gentleman (Mr. McSween), to whom the negroes belonged, heard distinctly the palpitation from an adjoining room.

Dr. Chisholm states that at the onset of the epidemic, he knew not how to act until dissection showed him; and adds, that having no suspicion of the heart being the seat of the malady, he did net examine that organ in the first two bodies opened; but finding the brain and all other organs in a state of health, he opened the heart in the third body, and discovered what he considered might be the cause and seat of the disease.' In the right ventricle of the heart he found an enormous polypus, which extended considerably into the pulmonary artery. On extracting it, the body which was contained in the ventricle, measured two inches in breadth. In the fourth body there was a large polypus in the right and left ventricle, besides one in the right auricle. The heart of the fifth, sixth, and seventh were just similar; and in these five, except one, where the lungs were diseased, no other morbid appearance could be found.

It is interesting to remark, as an aid to our therapeutics, that Dr. Chisholm, after he had discovered what he considered to be the cause of death, viz., the concretions in the heart, changed his plan of treatment. On the very first appearance of symptoms, he took blood, and then freely salivated, with mercury. The results, he adds, were most happy; not one that was salivated died.

I can offer, on my own part, no such a narrative as that given by Dr. Chisholm; but I can very definitely refer to not fewer than six well marked cases in which death, taking place rapidly, with no other foreboding symptoms than a rigor and succeeding slight febrile heat, was traceable by the *post mortem* inquiry to deposit of fibrine in the right side of the heart; the deposit filling the auricle to the exclusion of blood, and extending in the course of the circulation, from the auricle into the ventricle and pulmonary artery.

In other instances, as we have seen, the deposit of fibrine is subsequent to the development of one or other of those forms of local mischief to which we assign a specific name. The obvious features of such cases are distinguished by the presence of the local change, but the essential characters are the same in all examples, that is to say, the fibrine is abnormally increased. The increase may extend from the normal two and a half or three parts per thousand, to six, nine, or it has been said to twelve parts per thousand. This latter figure is possibly derived from an error in analysis, but the fact of a great increase is without a shadow of doubt; the result of the increase is also often equally striking. The excess of fibrine, beyond a limited extent, cannot remain in suspension in the blood, and the heart, under such conditions—if so plain but expressive a term be allowed me—*churns* out the fibrine, and becomes the receptacle of the separated mass.

I can give, at this moment, no explanation of the first cause of increase of fibrine in the cases I have named, except by entering upon the whole question of hypercausis and its origin; which task I would rather avoid, or at least retain. It will be more instructive, at this time, to pass to the subject of the symptoms of concretion, concerning which all is demonstrative.

[There is a class of cases in which fibrinous deposit in the heart is the sole cause of dissolution. Many examples of this are met with in cases of death from puerperal fever. There are other cases, as acute pneumonia, pleurisy, and, in fact, most sthenic inflammations, which the deposition renders fatal, and which would not otherwise have proved so. In other cases the fibrinous deposit is a consequence of a condition in itself necessarily fatal, as phthisis and cholera.]

In all cases of the acute inflammatory type, whether slightly or broadly defined, the practitioner, who is conversant with the risks of fibrinous disposition will be on his watch tower. He will remember the symptoms by which the existence of deposit is recognized; he will be guarded in his promises, and careful in his predictions. Most of all, he will be protected from being misled by those rapidly developed changes from severe local disease to suppression of local symptoms, which so often give to illusory hopes, and usher in the death.

Again, as I have said, the knowledge of the existence of a fibrinous concretion in the heart may prevent the committal of meddlesome, and, because useless, mischievous practice. In days when bloodletting had its full swing, when everybody was bled once for every disease, and once a day in inflammatory diseases; when even the convulsion of hemorrhage was met by a recall on the lancet; in those days there arose, amidst all the confusion, this persuasion, that to bleed late in inflammatory disease was bad practice. If I remember rightly, immortal Mackintosh himself, on every page of whose 'Practice of Physic,' there is a gaping vein, admits this rule: and the reason of the rule is obvious enough. In the later stages of inflammation, the tendency to fibrinous deposition is increased. The effect of hemorrhage is to increase the volume of water in the blood, to distribute the fibrine solvent, and to encourage fibrinous deposition. When, therefore, late in the inflammatory state blood is drawn, the balance of the blood constituents, already disturbed, is disturbed the more, and all that is wanting to secure the deposition of fibrine is secured. The rule, therefore, as I say, originated that it was better in inflammatory disorders not to bleed late in the disorder; and this rule expanded now into wider pretension, goes pretty nearly to the length of telling us that it is well to avoid bleeding at all stages; not because bleeding is not a grand remedy, but because it is too grand a remedy to be used without a definite knowledge of the time when to use it. I have, nevertheless, seen the rule about late bleeding in an inflammatory disease disobeyed even in our own time; and therefore I enforce it, and give the reason for its enforcement. In case of acute pneumonia, which came under my observation some years ago, I was overruled to take blood. After the bleeding, the strong man fell like a shot bird. He fell into sleep, and he slept into death. A large fibrinous concretion blocked up the right cavities of his heart. This clot had possibly commenced to form before the venesection: afterwards its rapid increase was determined, and the death was hastened by the means intended for its prevention.

The remarks against the general abstraction of blood in the later stages of inflammatory disease tell with little less force against local blood-letting. Indeed, there is not unfrequently a greater danger in the local than in the general remedy, inasmuch as, owing to its supposed harmlessness, it is more fearlessly resorted to. "A few leeches can do no harm," is the common sentiment: it is an incorrect sentiment, nevertheless, and to be guarded against always, and with special care in cases of acute hyperinosis in the young. Speaking, indeed, of hyperinosis in children, I would, if my voice had any influence, raise it against the abstraction of blood altogether in such examples. There is a form of acute pneumonia often met with in well-fed fat children. We put on leeches; the bitten surfaces bleed furiously; but the disease is not stopped. From such a patient, the more the blood taken, the greater the risk incurred; for, whenever the local indications of inflammatory mischief subside, the general indications of deposition are in the ascendant. "Breathless, pale, and bloodless," the end of the patient is at hand.

The same rule which tells against venesection extends to the administration of two classes of medicaments—purgatives and opiates. To reduce the amount of water in the blood, by draining it off through the alimentary canal, is but to increase relatively the amount of fibrine in a blood in which the fibrine is already absolutely increased. And so, once more, to paralyse the heart with opium, during the existence of acute hyperinosis, is to double the danger of deposition by bringing into play that feeble condition of the circulation which we have described as in itself immediately favourable to fibrinous separation.

In those states where the increase of fibrine is relative, or where there is tendency to deposition from sheer feebleness of the circulation, the rule against depressent measures is the more strongly to be enforced. I can add nothing further in this direction.

We will move from what may be called negative treatment to our third point—the lesson taught by our knowledge, such as it is, regarding a positive course of proceeding for the prevention of deposition. And here, I think, the first great truth which appears is that, to treat disorders marked by hyperinosis successfully, we must treat them by depressants at their early stages only, and must be prepared to move from the depressant system at much shorter notice than is commonly considered necessary. Before there is positively established in the blood an absolute increase of fibrine, I have no doubt that the conditions leading to such increase may be struck down by the use of rational means for the reduction of the force of the circulation; and so sometimes an acute inflammatory disease may be averted by free venesection. But, as it is rare that the practitioner is called to see disease in this early stage, blood-letting is dangerous to recommend as a general remedy. Whither, then, shall we turn to a remedy? We must turn, certainly, to those remedies which, while they reduce the force of the circulation, possess also a solvent effect on the blood. Is mercury such a remedy? I see no reason, either of a physiological or practical kind, to lead me to an affirmative on this question. In its physiological effects on animal organisms, mercury, carried to salivation, produces a peculiar disintegration of blood, but not a fluidity: in many of the cases of acute hyperinosis, in which after death I found large concretions, I trusted throughout to mercury, and gave it boldly, but with no result. The old calomel and opium pill is, therefore, to my mind, a combination deserving its loss of caste. When such a pill does good, I suspect the opium should have the sole credit, if not a little more.

Is antimony deserving of more favour? I believe it is. In a large number of experiments, in which I placed animals under the influence of antimony, I found in every case that the effect of the substance was to maintain a fluid condition of the blood; while common experience confirms that the drug has a power of arresting the force of the circulation. The danger of antimony, indeed, is that it may kill by the very depression which it produces; and its value, to put the opposite, is in proportion to the sthenicity of the disease. Its value, consequently, is limited: add to which, the remedy may be put into the body very much quicker than it can be carried out; so that its administration in excess is a pretty sure way of securing a prolonged convalescence, should the hyperinosis even be checked, and the patient recover.

Salines, what of them and their uses in hyperinosis? In these, I opine, we have not only the oldest, but the best remedies. I speak specially of the vegetable salines. As I have often pointed out, it is a curious fact that the common vegetable acids, citric and acetic especially, act on the blood exactly as the alkalies; they maintain, that is to say, the fluidity of blood. In combination, therefore, these bodies have a rational intention in the treatment of all disorders attended by excess of fibrine, particularly when they are given with a free quantity of water. Given in full and frequent doses (and it signifies, probably, but little which acetate or citrate is chosen) they are, after all, the safest, and, according to our present knowledge, the surest medicines in the diseases under discussion. It is not easy to push them too far; they do not accumulate; they put a gentle check on the circulation; and their transformation in the economy does not apparently diminish the solvent effect produced on the blood by their administration.

The narcotic series of remedies, dangerous as they become in late stages, are invaluable in the early stages of inflammatory disorder. I do not now speak of opium alone, but of the volatile as well as of the stable narcotics. The whole class have the physiological property of arresting the oxidation of the tissues, and, as a result, of calming that excitement which is the preliminary and necessary step to hyperinosis. The more rapidly acting narcotic is therefore often the best; and if chloroform, as the most rapid of ordinary narcotics, were a practical remedy—a remedy which could be entrusted to other than medical hands for its administration—it were, I believe, the best antiphlogistic known. I have treated croup at its onset with chloroform by inhalation, with a success such as I have seen from no other plan of treatment. But the effects of the drug must be fully carried out; and, unfortunately for practical utility, the effects must also be sustained, often for many hours, and must be watched with an attention which none but an experienced mind can give. Opium, from being more convenient in its application, has the advantage of chloroform as a presentable remedy; but, as it is so much the less rapid in its action, and so much the more permanent in its effects when these are elicited, it is in a physiological sense so much the less valuable as an active remedy against deposition.

External applications in the shape of counterirritants, blisters, and sinapisms, whatever may be their influence on the local lesion, can have no direct influence in preventing deposition. If cantharidin applied to the cutaneous surface be absorbed, it may have an effect on the blood; but whether that is for good or for evil it is impossible to say. I do not make these remarks on counterirritants in opposition to them as local remedies for local affections, but simply to indicate that they are not to be considered as of any avail in meeting the more general mischief present in the inflammatory condition.

Presuming that the practitioner is under suspicion in a case of acute hyperinosis or in a case of an asthenic kind, that deposition of fibrine is recurrent, what course shall he pursue? I know of but one: it is to give up at once, all idea of carrying depletion further, and to throw in ammonia freely but with discretion. The carbonate of ammonia is the remedy usually given; but the liquid ammonia is the most direct and most satisfactory remedy; well diluted, it may be given in doses of five, ten, and by increasing it, twenty drops; and it should be given until it shows itself demonstrably and easily in the breath on the application of the hydrochloric acid rod.

It may be given at the same time that quina or the decoction of bark is given; but in most cases I suspect it forms alone the natural remedy. It is a chemical remedy; that is to say, applied for definite intentions.

But while I thus recommend ammonia as a remedial measure which has to me proved of most value in the cases under description, and as a remedy which I have often brought into use in the early stages of acute disease, and always with advantage, I warn you that, in prescribing it, its effects must be carefully watched. Carried to an extreme, it produces the very opposite condition to hyperinosis; and then it is no longer a remedy—it is a poison. In one case, recorded in my essay on the *Coagulation of the Blood*, in which five grains of bicarbonate of ammonia were given every two hours, the remedy acted, I fear, as a poison to a certain extent. The patient was sinking, and in some measure comatose, when the administration began; all the excreting processes being much diminished. After death, the odour of ammonia in the blood was most distinct. The blood was dark and thick, but uncoagulated. I collected half an ounce from the right side of the heart, and exposed it to the air for three days; it underwent feeble coagulation, but its true plastic quality was clearly destroyed. The corpuscles, as is common in such blood, were disintegrated, and modified in character; there was some stellate, others oval, others many-sided, others collapsed; but not one was of the natural appearance.

Let me, then, renew this precaution relating to the action of ammonia, and extend it to the other alkalies. Indeed, the effects of potassa and soda, carried to the extent of producing the same changes, are more serious than those produced by ammonia; for the volatile alkali, being volatile, can escape more quickly from the system than the fixed alkalies, after its withdrawal; it holds, in fact, as a physical agent administrable to man, the same relation to soda and potassa as chloroform holds to opium or belladonna.

In those extreme cases where the fact of absolute deposition is now clearly demonstrated, what, then, shall be done? Alas! I can at this moment point out no satisfactory solution of the question. The day will come when the veil shall fall from our or our succesors' vision; but now we can but grope our way, and pray for the light. I for one know however, the difficulties in the way; and this knowledge, perchance, may be useful to some one in the future. I will therefore sketch them forth as matter for thought, and aids to future work.

It will occur to you, that the proceeding to be first considered is the removal of the fibrinous deposit by causing its dissolution. This is a definite proposition: the difficulties in the way are equally definite. In the first place, we possess no agent having a sufficiently rapid solvent action on separated fibrine. When fibrine is as yet in solution in the blood, it is the easiest matter in the world to keep it fluid; the addition of a very small quantity of alkali, say two parts of alkali to a thousand of blood, is sufficient for this purpose. But, when once fibrine has separated, its resolution is a difficult matter. True, it is not less soluble than before; but time is destroyed in the process. If, for instance, a solution be made containing so much as four grains of alkali to the thousand grains of a blood containing two grains of fibrine, a period of at least six hours must elapse before such fibrine would be redissolved. But such an amount of alkali thrown into the mass of blood as would bring that fluid altogether to the alkalinity thus required, would be in itself impracticable, because fatal. Under such alkalinity, the blood-corpuscles would be entirely dissolved.

Again, when deposit is the fact, the administration of alkalies by the mouth is in no way a direct means of effecting solution of the obstructing body. Medicines introduced in this way, granting that they are taken up into the blood at all, which is in truth doubtful, are but slowly carried through the heart, and are brought into contact with the concreted mass at a single point. From such limited exposure we can expect no result.

The administration of ammonia by inhalation in combination with the vapour of water, seems at first a reasonable suggestion. I have tried it both in disease and in experimental work. It fails. Ammonia is not absorbed easily by the lungs; and, as absorbed, it is never present in the blood, passing stroke by stroke through the heart, so as to produce any effect on a deposit of fibrine in this organ.

The injection of alkaline solution into the veins may seem a reasonable project. It is worthy of trial in extreme cases, but I fear it would not succeed. If solution of clot could be determined by this process, I am afraid there would also be determined universal dissolution of the walls of the blood corpuscles; and an animal without corpuscles in his blood is no longer alive. The first microscopist of blood little knew upon what he had dropped when he beheld these little invisibles. If we should or could at any time light on a chemical body which will dissolve fibrine on immediate contact, without destroying the blood-cells or the coats of the vessels, then we should have a remedy on which we might set our hopes. The difficulties in which the question is involved, by means of this antithesis of argument, will be readily understood.

The observations I have made above, are, however, intended to bear entirely on cases of a kind where death is threatened within minutes or hours; in that series of cases where the symptoms extend over a long period, such cases as that recorded by Dr. Sayer and Mr. McNab, a course of treatment by a fixed alkali might possibly be productive of real good. Such treatment is based on philosophical and sound argument.

Returning to extreme cases: Would it be possible, when the concretion is on the right side of the heart, to remove it by operation? I will relate an experiment bearing on this point. In one case, where a cat died in oxygen gas, I took the animal out of the box immediately after she seemed to cease breathing, laid bare the right external jugular vein low down in the neck, pushed through it a crotchet needle, passed that into the heart, and brought up to the point of insertion a separated mass of fibrine, which had before obviously filled the auricular cavity. In cases of dying in the human subject, when all is hopeless, I see no harm in doing a similar operation; the diagnosis of concretion on the right side being clear.

Before I conclude I have to touch for a moment on a special question which has been specially asked of me. It is this. What are the conditions leading to hyperinosis? To this question I give frankly the answer, that it is at present unanswerable. To approach it even requires a new and extended inquiry into the whole subject of the oxygenation of animals—a point to which I may at some future day be enabled to call the attention of the profession, with a knowledge yet to be learned, and aspirations yet to be realized —*British Med. Journal, Feb.* 11, *and April* 7, 1860, *pp.* 101, 259.

26.—ON THE EXISTENCE OF A FIBRINOUS CLOT IN THE HEART IN SOME CASES OF CROUP.

By Henry Smith, Esq., Caroline Street, Bedford Square, London.

[Some years ago a controversy took place between Dr. Richardson and Mr. Henry Smith, respecting the causes of death in croup. Dr. Richardson asserting that in some cases, attended with peculiar symptoms, the cause of death is to be found in the existence of a fibrinous clot in the heart, formed several hours prior to dissolution. When death is brought about by obstruction to the breathing alone, the symptoms are widely different. These opinions Mr. Smith considered erroneous. Now, however, writing upon the same subject, he observes:]

Further inquiry on this matter, and observation on both the living and the dead, have convinced me that Dr. Richardson's views deserve serious attention. As on the former occasion I considered them incorrect, and opposed them as well as I was able; so now, from a sense of justice to that gentleman, I am glad to state my present conviction, that there are certain cases of croup in which the symptoms are more referrible to an embarrassed state of the heart than to a mere obstruction in the windpipe. These symptoms consist of great dyspnœa, pallid face and lips, cold extremities, and very feeble pulse; whereas, turgescence and lividity of the face, with blueness of the lips, accompanied with extreme dyspnœa, mark the insuperable obstruction in the trachea. In the former cases I am now disposed to agree with Dr. Richardson, that tracheotomy will not save life, as there is every probability that a coagulum has formed in the heart, whilst in the latter experience teaches that an artificial opening in the windpipe may save life.

It is not the mere reflection on Dr. Richardson's views that has brought about an alteration in my mind respecting this point; but a careful examination, both on the living and the dead, has led me to believe that that gentleman is in a measure, at least, correct; and in order to be brief, I will just mention the details of one of the last cases of croup, where I was able to make a post-mortem examination:—

I was called to a child, aged 6, who was suffering severely from croup, and had been ill three days. The breathing was most laborious, the child making violent efforts to get air, and not moving the chest at all; the face was, however, very pale, and the lips were not blue. The pulse was extremely rapid and small. My opinion was sought regarding tracheotomy; I did not think it a favourable case. The operation was not done; and the child died in an hour.

I opened the body with the express view of ascertaining whether there was a clot in the heart; for the child presented just those symptoms expressed by Dr. Richardson. Surely enough, on opening the heart, there was seen extending between the right auricle and ventricle a firm deposit, and there was another fibrinous clot at the commencement of the aorta. The lungs were healthy, but the larynx was lined with a thick deposit, which very much narrowed the cavity; a thinner membrane lined the trachea. Now, it struck me at once, that had tracheotomy been performed in this case, the existence of the coagulum in the heart would have prevented recovery had the obstruction in the windpipe been overcome; and I am now of opinion that some of the cases previously operated upon by myself, and presenting just the same symptoms as existed here, were instances where a fibrinous clot would have been found in the heart had it been looked for.

In another instance, where I was recently called to perform tracheotomy, the symptoms had come on within only a few hours. The child, aged 5, was lying in bed perfectly insensible, breathing with extreme labour; notwithstanding this, the pulse was pretty good, and the extremities not cold. I refused to perform the operation, and the child died in an hour and a half. On examination, there was not any coagulum in the heart whatever; but the lungs were excessively congested, and the upper part of the larynx and trachea were lined with an organised cast.

Without venturing to go so far as Dr. Richardson has gone, I cannot help thinking that his views regarding the existence of a fibrinous clot in the heart in certain cases of croup demand serious attention; and, as I once strongly opposed them, I am now happy to admit that I am, in a measure at least, converted to his side.—*Med. Times and Gazette, Dec.* 17, 1859, *p.* 617.

27.—ON THE INFLUENCE OF THE NERVES ON THE ACTION OF THE HEART.

By John Cockle, M.D., Physician to the Royal Free Hospital, &c.

The exact influence of the par vagum upon the heart is still a vexed question, although the accurate experiments of Edward Weber have never yet been challenged. He clearly proved that electro-galvanization of these nerves retarded the heart's beat, and prolonged the period of diastole; while, on the other hand, their section was followed by marked acceleration of the pulsations; so that, as Ludwig has remarked, the eighth nerve holds the balance of the heart's contractions. It is, in other words, the moderator nerve of the heart. But while admitting these experiments, what different conclusions may be drawn, if the views of Dr. Brown-Séquard prove to be correct? This physiologist maintains that the par vagum is the true vasomotor nerve of the coronary arteries, and that its irritation causes contraction of these vessels, and thus, by preventing the passage of the blood into the cardiac tissue, diminishes the frequency of, and eventually suspends, the heart's beat; while, on the other hand, their section, by allowing the coronary arteries to become over-filled with blood, excites the heart to preternatural pulsation. Still there are residual phenomena that do not admit of such a physiological solution. Take, for example, the palpitating heart of a chlorotic girl, beating for days at the rate of 120 or more per minute; then compare this condition with that of some cases of marked fatty degeneration of the cardiac muscle, where the heart's beat is lowered to 20 per minute; and who is to say, with the authority that should entitle him to credit, what part is in such cases due to the nerves, on the one hand, and to the impoverished blood and to the degenerated tissue, on the other? Look at the dilemma we should fall into, for example. if the action of the heart, in anæmia, were considered as dependent upon the motor nerves. In accordance with experiment, if the par vagum were irritated the standard of pulsation should be lowered, not raised; or, if the accelerated pulsation were referred to the central ganglia, why then the par vagum does not in reality hold the balance.

The explanation of the mediate influence of the sympathetic nerve upon the heart is still more obscure. It is familiar knowledge that tænia in the intestine can greatly derange the heart's rythm and rate of speed, but the exact *quo modo* is still open to conjecture.—*Lancet, Dec.* 17, 1859, *p.* 610.

28.—ON CERTAIN POINTS OF PHYSICAL DIAGNOSIS IN MITRAL VALVE DISEASE.

By John Cockle, M.D., F.L.S., Physician to the Royal Free Hospital.

[The writer, before passing on to the subject-matter more immediately in question, premises some interesting observations on the physiology of the heart's action, which, however, do not bear so directly on practice as to be inserted here. Dr. Cockle then continues:]

The diseases of insufficiency, perhaps, as a rule, entail less serious mischief than those originating in obstruction. Regurgitation into the left ventricle through the faulty aortic valves may, as is well known, last for years. with scarcely a subjective symptom, so long as the ventricle maintains its contractility unimpaired. So, in like manner, with moderate regurgitation through the mitral valve. This is, perhaps, of all diseases of the heart, when slight in degree, one of the most easily tolerated; it may also last for years without necessarily entailing any marked degree either of hypertrophy or dilatation of the heart.

The left auricle, with respect to the regurgitant blood, acts the part of a safety reservoir, just as the tricuspid valve permits blood to regurgitate into the auricle during temporary distension of the right ventricle, and, in the cases named, effectually prevents stress upon the lesser circulation. Its thick and highly plastic endocardium powerfully supports the muscular tone, and averts the danger of impaired contractility. Hence, in these cases, beyond the knowledge possibly of a prior attack of rheumatic fever, the systolic murmur at the apex is the only physical sign which we have to depend upon as marking the change. That the disease is kept within these moderate bounds, we may indirectly know by the auscultation of the lung. So long as the breath-sound remains pure, or but slightly exaggerated, we may conclude that the disease does not exhibit any tendency to increase. This is the surest criterion that I can indicate for such purpose. The mere existence of systolic murmur, beyond revealing the existence of structural change, is here at its minimum of value. Indeed, usually, the louder the murmur the less the actual amount of mischief actually inflicted.

To these cases, when chronic, medical treatment, in its strict sense, is quite inapplicable. We cannot restore the integrity of a valve which has been injured by an endocardial disease of long standing; and, so long as the general health remains good, we have simply to enforce the avoidance of all causes likely to induce embarrassment of the systemic and pulmonic circulation.

These slightly dilated hearts, however, are, it is well known, much more prone to attacks of palpitation than those in the opposite condition of simple hypertrophy. I would here momentarily allude to the distinction between palpitation of organic and inorganic origin, and observe that, now and then, perhaps, the surest diagnostic sign between the two conditions is the absence of marked dispnœa during the paroxysm of inorganic, and its presence in cases of organic palpitation. I have already stated that this stage may endure for years unless some causes, physical or mental, occur to impair the general vigour and induce impoverishment of the blood. Irritative excitement is then induced, and now it is that the first signs of impaired contractility of the chamber present themselves.

This failing muscular power of the heart is still further taxed by blood insufficiently stimulating. It is in this early stage of mischief that special treatment is so applicable—I mean the union of digitalis and iron. I know no treatment to compare with this combination. But for its special virtue, digitalis must be given in small (four or five minim) doses, the great indications being to lessen irritability, to promote regularity of rhythm and speed, and thus to ensure the complete contraction of the chamber, and, as a consequence, to impart stability and volume to the pulse. By this treatment not only are such objects achieved, but the physical signs are ameliorated, and the intensity of murmur greatly reduced. I believe that the external use of belladonna, in the form of either plaster or ointment, must act in an analogous manner. But should the dose of digitalis, however, be larger, and given to the extent of prolonging the period of diastole, we at once lay the foundation for the supervention of that condition it is our sole object to antagonize—dilatation of the chamber. There is another possible mode by which digitalis exercises its diuretic qualities. In marked regurgitant disease, diminished secretion of urine is a common symptom, and is probably a direct result of the diminished amount of blood supplied to the kidney. Now, when digitalis acts as a diuretic, it may be that, by permitting the ventricle to empty itself more completely, the aorta supplies the kidney with more blood, and consequently more urine is secreted. At least this is the opinion of Prof. Niemayer, of Greifswald.

But, in more exaggerated cases of regurgitation, the great question arises —how is such a condition physically to be recognised? Does a simple and permanent systolic murmur, harsh or soft, with its maximum intensity at or near the left nipple, reveal the inadequacy of the valves to close the orifice, or may such murmur simply indicate roughness of the ventricular face of one or both curtains, or some abnormal position of a tendinous cord? Skoda has stated, *ex cathedra*, that an absolutely intensified accent of the pulmonic sigmoid valves, in consequence of the impediment offered to the free passage of blood through the lungs, is the pathognomic sign of this stage of regurgitation, and I need hardly mention the weight of authority such a statement bears. Without pretending to deny that such accent, at times, unquestionably exists, it does appear to me (and I mention with the deference due to a great name) that there are some objections to the general reception of this theory. For example, I believe the following propositions susceptible of proof:—

1st. That the second sound of the heart, in health, varies in point of character; that is to say, it is sometimes most loudly intoned to the right, at other times to the left, of the sternum.

2nd. That the second sound stands in no fixed relation to the energy of the ventricular systole. Dr. Stokes has recorded many cases of fever, in which, the walls of the heart being softened, the first sound was annihilated, while the second retained all its intensity.

3rd. That the second sound stands in no fixed relation to the resiliency of the arterial wall; this sound being loudly heard in some cases of extensive atheroma, in which the elasticity of the aorta is more or less destroyed.

4th. That an obstruction at the aortic mouth would, on this theory, naturally tend equally to throw stress upon the second sound of the pulmonary artery.

5th. Where considerable regurgitation exists, the diminished stream propelled into the aorta would, theoretically, tend to weaken the closure sound of its sigmoid valves, and thus leave the sound of those of the pulmonary artery not absolutely, but only relatively intensified.

6th. That in many cases the systolic mitral murmur is so intense as to occupy an entire revolution of the heart, and entirely to mask the closure sound of the pulmonic sigmoid valves.

7th. That in some cases of auriculo-ventricular contraction, the cardiac sounds are so indeterminate, and the rythm so disturbed, that nothing like particular accent can be discovered.

Lastly. Many changes of lung tissue—such as cirrhosis, contraction from phthisis—may permit the pulmonary artery to come into more immediate contact with the chest wall, and thus give, relatively, intensified accent. Induration of the left lung border may, also, become a source of reinforcement of the sound of the pulmonary artery. So that, if these propositions are valid, accent upon the second sound of the pulmonary artery cannot always serve as a means of differential diagnosis in the diseases of the mitral valve.

The next point to determine is, are there any other physical phenomena which can at times subserve differential diagnosis, and even enable us to measure approximatively the amount of regurgitation? I believe that there are two.

First. The heart, in such cases, being under the influence of irritative excitement, with a varying amount of dilated hypertrophy, acts more energetically than it would do in a normal state, though transmitting a diminished quantity of blood into the systemic vessels, in consequence of the back current into the auricle. But the subclavian and carotid arteries, in their recoil, thus forciby and suddenly injected, closing on the diminished stream, give rise to the phenomena of collapsing and unfilled arteries, precisely as we see in cases of spanæmic palpitation, as, also, that following considerable loss of blood: the closure of the sigmoid valves, audible, at least, at the top of the sternum and in in the carotids, proclaims their integrity, while the smallness and softness of the radial pulse exclude the notion of mere uncomplicated hypertrophy of the left ventricle only. Thus, in a sense, this condition not only indicates actual regurgitation, but, to some extent, estimates the amount. This condition has been, in two cases, carefully noted during life, and the condition of simple inadequacy of the mitral valve verified after death. This sign, however, requires, of course, more extended verification.

The second sign of occasional importance, and immediately conditioned upon the preceding, relates to the reduplication of the second sounds of the heart, having, of course, their maximum intensity at the mid sternum.

The following is the theory I would suggest respecting the occurrence. The aorta, receiving a diminished supply of blood, in consequence of the regurgitant stream into the left auricle, fails of its accustomed stimulus of distention, and, as a possible consequence, lags behind the pulmonary artery in its recoil. The closure sound of its sigmoid valve would then, in point of time, be subsequent to the recoil of those of the pulmonary artery, and produce the phenomenon of reduplication. This sign, also, might both characterize the disease, and, with certain limits, measure the amount of regurgitating blood. There are, however, certain difficulties occasionally presenting themselves in this particular—namely, when the second sounds are trebled, or even quadrupled. Where reduplication occurs with the first sound, it is, I believe, nearly always the result of a decided rasp murmur. A great authority in this country, Dr. C. J. B. Williams, as also an observer in France, Dr. Charcelay, have supposed that the reduplication of the first sound originates in a want of synchroniety of the ventricles. If this explanation be correct, it appears to me that there should, from an almost inevitable necessity, be reduplication of the sigmoid valve sound also; but this result does not of a certainty always follow. Another physical sign in regurgitation through the mitral valve has also been mentioned by Skoda—viz., a murmur audible in the pulmonary artery; and he explains the production of such murmur by organic change in the end-arterial membrane of the vessel. Now, it is certain, that in mitral valve regurgitation disease, not only does systolic murmur exist at the apex, but it is, at times, heard loudly two or three inches above and around the left mamma, occasionally completely masking the second sound; it is also audible at the corresponding site posteriorly, and in the course of the great vessels. Now in such cases we may assume a double origin of murmur—that not only is vibration engendered at the faulty valve, but that it is reinforced, or even regenerated, by the refluent stream from the ventricle meeting the comparatively stiller layer in the auricle.

In other cases, when, with weak murmur at the apex, murmur—it may be of different pitch—is after a time heard in the vessels, it, in all probability, arises from the distended auricles pressing upon the aorta and pulmonary artery respectively, just as gentle pressure upon any large artery with the stethoscope produces temporary murmur at will. At all events, whatever be the explanation of this murmur in the great vessels, it is safer to assume a mechanical cause, similar to the one just named, in preference to the supposition that organic change exists in these vessels. In the young, at least, we see that double valvular disease is not the rule; inasmuch as two different pathologic causes are often necessary for its production. The aortic valves are mostly affected by the atheroma of advancing years, while endocarditis has a peculiar affinity for the curtains, cords, and fibrous zone of the mitral valve.

Regurgitant and obstructive diseases of the mitral valve differ from aortic regurgitant and obstructive diseases in their effect upon the chambers of the heart, but all agree in one great particular—viz., the gradually increasing tendency to under-supply of the arterial, and over-supply of the venous systems. This latter result in the systemic system arises from the diminished *vis à tergo* consequent upon the insufficient supply of arterial blood, and by such mechanism, these affections lead eventually to impaired contractility of the right side of the heart, producing those congestions, hemorrhages, and effusions which form the common terminus of the diseases of the central organ of the circulation.

In conclusion, I would select one case from the series, both for the purpose of illustrating the peculiarity of the physical phenomena, and of signalizing an error into which I was betrayed with regard to the differential diagnosis.

Charles T——, aged fifty-three, by trade a shoemaker, was admitted a patient of the City Dispensary in 1856, with the following symptoms, which had been gradually increasing for the three preceding years:—Palpitation, at times in severe paroxysms of long duration; considerable dyspnœa on exertion; cough, occasionally combined with sero-sanguineous expectoration. The extremities were cold, swollen, and livid; face of livid pallor, and tumid; lips leaden; the pulse in both radials, extremely small and most irregular; the carotid arteries throbbing and collapsing; the jugular veins swollen and undulating—though emptying on full inspiration, but soon distended from below when pressure was applied above the vein. The heart's apex was felt to the left of the left nipple. The percussion area of dulness was increased in both the verticle and transverse directions. The impulse was extensive, weak, and irregular, but without fremitus, extending from the apex to the epigastrium. On auscultation, the heart's rythm was markedly irregular, and both sounds were replaced, so far as could be made out, by an extremely slight and harsh murmur, of osseous (?) character, if I may use such a term. This double sound, apparently loudest at the neighbourhood of the left nipple and towards the left axilla, was still audible, in a less degree over the middle and lower half of the chest, right and left. Slight sibilating and mucous râles were heard over the lungs generally. His history showed that he had never suffered from rheumatic fever.

The above is a fair paraphrase of his symptoms, from notes taken on several occasions while he was under treatment. He was most carefully watched for nearly three years, in consequence of the symptoms noted, and the physical signs and murmurs heard, supposing them to indicate beyond doubt, auriculo-ventricular contraction of the left orifice, with insufficient tricuspid valves; and that, sooner or later, an opportunity might be afforded of substantiating the cause of the existence of a double murmur in such a case, and so setting at rest a vexed question: for I need hardly say that some of the ablest observers in France have most emphatically denied that a double murmur ever occurred in contraction of the left orifice. During the further course of the disease, frequently recurring dropsy, slight in extent, took place, and he at last died from increasing complications.

The post-mortem examination was made by Dr. Robinson, who saw the patient several times during the closing period of his illness. The following was the condition of the heart:—Great and general dilatation of all the cavities and vessels of the heart, excepting the aorta. The walls of the cavities were very soft, but this was, in all probability, the result of post-mortem change. The right auriculo-ventricular orifice was immensely dilated, admitting with ease the five fingers; the tricuspid valves being inadequate to the closure of the orifice, obviously the result of secondary insufficiency. The left auriculo-ventricular orifice was also greatly dilated. The larger or aortic curtain of the mitral valve was unusually dense and much increased in size: the smaller curtain was altogether *rudimentary*, not exceeding in size the thumb-nail. The chordæ tendinæ were extremely short and slender, so as scarcely to permit the valve to leave the wall of the heart. Considering the absence of rheumatic fever and change about the valve beyond its abortive condition, it was probable that it was a congenital malformation.

Now, I do not pretend to offer any explanation of the physical signs observed in this singular case, but I consider the following inference fairly warrantable from the post-mortem results. In the great majority of cases of heart disease, we can state that valvular lesion exists; beyond this, we may in many even predicate the particular orifice affected; but that occasionally our present lights are insufficient to guide us to a correct differential diagnosis between the obstructive and regurgitant diseases of the auriculo-ventricular orifices.—*Lancet, Dec.* 24, 1859, *p.* 636.

29.—ON THE TREATMENT OF PERICARDITIS.

By W. O. Markham, M. D., Physician to St. Mary's Hospital, London

Extended observation has proved that in the treatment of acute rheumatism there is no remedy which has such a powerfully beneficial influence over the disease as the salts of potash, when administered in sufficiently large doses. The proof of this assertion is to be found in the results of our daily practice; and in the concurrent—I may say universal—testimony of competent observers in different countries.* The remedy, indeed, is entitled to be considered as something of a specific in this disease, so constantly are the expected results found to follow its proper use. From one to two scruples, according to the age of the patient, of the bicarbonate of potass are to be given for the purpose every second hour, as prescribed by Dr. Garrod. A marked amelioration of the symptoms is generally—though, I admit, not invariably—observed to follow its administration in the course of about twenty-four or thirty-six hours; in fact, whenever the urine becomes alkaline.

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* The following are a few of the anthorities who may be referred to on this point:—Dr. Basham gives from one to three ounces of nitrate of potash in four pints of water during the twenty-four hours. (Med. Chir. Soc. Trans., 1848). Gendrin also gives nitrate of potash. (Med. Gaz., 1848). Dr. Golding Bird used the acetate of potash in doses of half an ounce in the twenty-four hours much diluted. Dr. Swett administers one drachm of the tartrate of soda and potash every two or three hours, until the urine becomes alkaline. (New York Med. Times, 1854). Dr. Garrod's method is to give two scruples of the bicarbonate of potash every second hour, night and day, in a wineglassfull of water, "until the joint-affection has ceased for three or four days." (Med Chir. Trans, 1855). To this excellent paper, I would especially call the attention of those desiring particulars in this matter.

Now, inasmuch as we are bound, in all reason, to believe, that the *materies morbi* which produces the rheumatic arthritis is identical with the *materies morbi* which produces the rheumatic pericarditis, we should, to follow out a rational treatment, anticipate that this remedy would be also efficacious in preventing, or arresting the progress of, the pericardial inflammation. The fact is not, for obvious reasons, easy of demonstration, but observation most certainly tends to show the correctness of the conclusion. The authority of many competent observers might be cited in support of the opinion thus expressed by Dr. Garrod: "Besides the influence on the duration of the articular affection which has been alluded to, I cannot help thinking that an effect is likewise produced on the cardiac disease, to a very considerable and important extent. In no case did the affection of the heart ensue after the patient had been more than forty-eight hours under the influence of the medicine; and it has appeared to me, that even when present on admission into the hospital, or coming on within a short period, its progress was powerfully checked by the treatment, and prevented from producing the terrible mischief which, when uncontrolled, it so frequently induces: this I should be inclined to ascribe to the altered condition of the blood, and especially of that portion giving rise to fibrinous deposits on the peri- or endocardium." (*loc. sit.*)

My own observation fully bears out Dr. Garrod's statement; and, necessarily, therefore, leads me to the conclusion that we possess no remedy more efficacious than this, in the treatment of acute rheumatic pericarditis, *quoad* the diatheric character of the disease. Its action is, we must suppose, (from its manifest influence over the arthritis), the neutralising of that poisonous element, whatever it be, which occasions the local inflammations—the arthritis and pericarditis; arresting the progress of the inflammations, when they have already seized upon parts, or anticipating and preventing the seizure.

[Where severe arthritic affections coexist with the pericarditis, it is absolutely necessary to tranquilize the nervous sytem, and opium must be given, if necessary, freely. Restlessness, sleeplessness, pain, and suffering, act very injuriously upon the case. The opium may be given in grain doses every hour for hours together, and it will not be found to "lock up" the secretions or contract the pupil.]

During the whole treatment of pericarditis, the secretions should be duly attended to. The bowels should be regularly relieved, and the action of the skin and kidneys maintained. Frequent purging, however, must be avoided; for the partial exposure to cold, which is its necessary consequence, and the extreme pain and suffering which movement is apt to occasion in these cases (especially when the co-existing inflammation of the joints is severe), sometimes more than counterbalance, by the nervous excitement and general disturbance they produce, the good which the purging might be supposed otherwise to bring about, as a process of elimination.

The *general* treatment, then, of rheumatic pericarditis is in the main—at least, during its first periods—the treatment of acute rheumatism. But the local inflammation demands especial consideration. In the commencement of the inflammation, the local abstraction of blood, proportioned to the degree of local pain, the strength of the patient, and the period of the disease is generally of service. Should there be signs of severe pulmonary and cardiac congestions, then small venesections will be required for their relief Warm fomentations or poultices should be constantly applied over the præcordial region. This method of bringing warmth to the inflamed part is of very great service; it gives much relief to the patient; and is worthy of more attention than is generally given to it in this country. The beneficial effects of blisters have been doubted by some observers; but I have so often seen them followed by mitigation of the local pain, and apparently by diminution also of the pericardial effusion, that I do not hesitate to use them, when the first acute periods of the inflammation have passed away. They do not prevent the application of poultices or fomentations.

At this period of the disease, another most important object, arising out of relation of inflammation to the heart, presents itself to our consideration.

In every case of pericarditis, indeed, whether rheumatic or non-rheumatic, and at all periods of the inflammation, we should carefully watch for signs of the heart's failing powers. Weakness in this disease is apt to manifest itself suddenly. This warns us to be cautious, in the first instance, that we do not injure the patient by depletion; and it also warns us not to delay too long the use of stimuli. There are cases, especially of the non-rheumatic class, in which stimuli are requisite from the very beginning of the attack; the condition of the system under which the pericarditis has arisen, demanding their use.

This attention to the condition of the heart in pericarditis is a point upon which Dr. Stokes has especially insisted; and there can be no doubt, most wisely. He is "convinced that patients are often lost from want of stimulation at the proper time." The following are the particular signs, which he has given, as indicative of a weakened condition of the organ: a feeble, intermittent and irregular pulse, especially when it has not had these characters from the first; turgescence of the jugular veins; change in the heart's sounds, particularly feebleness of the first sound; pallor, coldness of skin, œdema, and faintings.

Here I would again impress upon the student the facts: that inflammation is a sign of weakness not of strength; that in every inflammation where exudation has taken place, there is a process of absorption and reparation to be gone through, if the parts affected are to regain their integrity; and that for the proper furtherance of this process, vital force is absolutely necessary. Sinking or weakness of the vital powers should, therefore, be carefully anticipated in all inflammations; but especially so in pericarditis, because, here the paralysing effects of the inflammation fall directly upon the central organ of the circulation. Light and easily digestible nourishment—milk and weak broths—should therefore be assiduously administered during the earliest periods of the inflammation; and stimuli, when the acute period has ceased, and the process of absorption has commenced, or whenever the signs of enfeebled circulation begin to show themselves.

Observation, indeed, certainly seems to prove, that rheumatic pericarditis is an inflammation which rather attacks those of weak than of strong constitutions; that it is more common in the delicate and young, than in vigorous persons at the prime or middle periods of life; and that the degree of inflammation—that is, the general febrile reaction and the local exudation—is also greater in them than in the strong. The disease is sometimes especially severe in young females, and in such cases, the violence of the inflammation is shown by the abundance and rapidity of the pericardial exudation.

From all which considerations, this corollary necessarily results, viz., that in the treatment of pericarditis, the administration of our remedies must be measured by the general condition and the idiosyncracy of the individual attacked, rather than by the intensity or other peculiarity of the local inflammation. And herein—in seizing upon the special points of the individual case, and in modifying the remedies to meet their requirements—lies that peculiar practical tact which distinguishes the skilful physician.—*British Med. Journal, Dec*, 10, 1859, *p.* 996.

DISEASES OF THE ORGANS OF RESPIRATION.

30.—ON THE USE OF ANTIMONY IN TUBERCULAR PNEUMONIA.

By W. T. GAIRDNER, M.D., Physician to the Royal Infirmary, Edinburgh.

[In a case of Pneumonia probably tubercular in its origin, in which the patient was almost in extremis, (pulse almost inappreciable and skin cold, upper lobe of left lung condensed, and breathing amphoric) Dr. Gairdner, with the best results, gave small doses of antimony, along with diffusible stimulants; he gives antimony in these cases notwithstanding the weakness and exhaustion of the patient. Of course the recovery was only partial, it could scarcely be otherwise considering the state of the lung. The dose of antimony]

Should rarely exceed one-tenth or even one-twelfth of a grain to begin with in such cases, sometimes even less. But, on the other hand, it is sometimes so well borne, that I have given half a grain and even one grain doses to patients who were so weak as to be utterly unable to raise themselves in bed; and this without the slightest disagreeable effect of any kind. In general I regard the ordinary physiological effects of antimony as *quite opposed to its therapeutic action*; and whenever they occur, I make it a rule either to suspend the remedy or to diminish the dose—believing it to be, on the whole, much safer to forego the possible advantage of the antimonial medication, than to run the risk of superinducing the least degree of poisonous action.—*Edinburgh Med. Journal, April* 1860, *p.* 953.

31.—ON SOME CONSIDERATIONS IN THE TREATMENT OF PHTHISIS.

By EDWARD SMITH, M.D., LL.B., Assistant Physician to the Hospital for Consumption, Brompton.

[A normal alkalinity of the blood is absolutely essential to the proper performance of the combustive process, whilst acidity is necessary to the formative process, (hence the muscular fluid, and, indeed, the fluid of every tissue has an acid reaction), therefore,]

Excessive waste in the system will occur from two different causes; viz., from excess of acid and excess of alkali; and each, perhaps, is equally injurious. Now, we know that acids and alkalies constitute the great part—indeed, on reflection, we might say almost all—of the remedies in phthisis, as, indeed, in most diseases; and by practice we attain to a sort of undefined notion as to the cases in which they are respectively applicable. The difficulty is to determine with accuracy these respective states. In reference to the acid state of the blood, I think we may always affirm that unusual acidity of the excretions (not due to local states of disease of the excreting organs) indicates an excess of free acid in the blood; and, in phthisis, I am disposed to consider the state of the skin-excretion as the best indication of this state of things. If, in such cases, I find that the perspiration has a sour odour (and the number of such cases is legion), the administration of a combination of the carbonates of soda and potass, say fifteen or twenty grains of each combined, or of the acetate of ammonia, thrice a day, is followed by the happiest results; and the measure of the effect is still the state of the perspiration. This immediately tends to increase the oxygenating process, but it does not of necessity remove that state of assimilation by which an undue amount of acid is thrown into the blood (assuming that, on inquiry, the excess of acid is owing to that circumstance); and hence a reduction in the starchy and saccharine food, and the invigoration of the system by change of air, &c., must be added. The amount of perspiration does not guide us in this matter; for in many cases, with excess of acid, there is profuse sweating; and with profuse sweating we are accustomed to associate acid in our treatment, yet, in such cases, the proper administration of the alkali tends to remove this excess of perspiration. I have recently had a case of early phthisis under my care, in which there was a constant excessive elimination of acid by the skin, and which, without other symptoms than excessive and general exhaustion and wasting, did not progress satisfactorily during three or four months. Under the influence of the combination of potass and soda, he had a very profuse sweating, accompanied by an exceedingly acid and acrid odour; and obtained immediate and almost marvellous relief. The following is a short account of this case:

J. E., a young single man, aged 20, engaged in a grocer's shop in the country, had complained for three months of bad appetite, with brown tongue, foul breath, and slight emaciation. There was slight hæmoptysis occasionally, with slight dyspnœa. There was scarcely any cough, but the throat had been sore from the commencement of his disease. The general expression of the symptoms was nervous exhaustion and muscular debility. The chest presented no signs of disease, except the slightest shade of dulness on percussion, and lessened respiration on the left side.

On August 21st, he began to take iron, with a mineral acid and quina. Cod-liver oil was added on August 28th. In September, he continued in the same state, but with a little increase in cough and debility. In October the cough continued, but he felt a little general improvement. There was also a little pain in the left breast. His state was becoming more anæmic; and the iron, being intermitted for a time, was renewed in November. During all this period he took out-of-door exercise, used salt and water ablutions; and was directed to take milk and other nutritive kinds of food at short intervals.

Thus three months passed away, without any visible improvement; and the progress of the lung-disease appeared imminent. At the end of November, however, he had a most profuse sweating during four nights, in frosty weather, accompanied by an exceedingly sour and acrid odour, which appeared to be the crisis of his condition; for, under the influence of bicarbonate of potass and gentian, he improved most rapidly, and the increase of perspiration, with its acid state, ceased in December. Throughout January he continued to improve, and became again cheerful, active, and strong; and the cod-liver oil being again prescribed at the beginning of February (it having disagreed with him in the early part of his attack), caused so much improvement in a fortnight, that he was led to look upon it as the principal means of his restoration to health. I cannot but think that, if the alkaline treatment had been begun earlier, we should have been spared some anxiety. The case shows well the peculiar condition of the blood under discussion, the efficacy of the remedy, and the different action of the oil under the two conditions.

It is in this class of cases that I believe fats to be the most beneficial, if their administration is preceded and accompanied by the administration of these alkalies. There can be no doubt that the administration of starch tends largely to increase the acidity of the blood; and there is reason to believe that the action of the fats (the other common hydrocarbonaceous material) tends in the contrary direction—a belief based upon the physiological action of fat upon the respiration, the influence of fat over oxygenation, its beneficial action in cold climates, and its influence under the condition now being discussed. Hence I think that the state of the perspiration offers an important guide for us in the administration of alkalies and fats. A similar remark may also be made in reference to the action of numerous nitrogenous substances over the respiratory functions; for my experiments have proved that they have this action, and also that it is an indirect one by influencing the rapidity of the oxygenating process, and not a direct one by supplying carbon. As I have recently discussed these subjects elsewhere ('Philosophical Transactions,' 1859; 'Philosophical Magazine,' December 1859; 'Dublin Medical Quarterly Journal,' Feb. 1860), I shall not enter into them here, except in reference to one which, both from its power and its universal employment, should be better understood; viz., tea. We do not take tea without other food, except it be after the principal meal of the day, when the system is still replete with food; that is to say, we do not take it at a period when the blood is unusually alkaline, but when it abounds in free acids. Its action is rapid, and so powerful that a maximum increase of one-fourth or one-fifth of the whole quantity of carbonic acid exhaled is obtained in less than half an hour; and it has so accumulative an action, that twenty-five grains, taken every quarter of an hour, gave an excess of two hundred grains of carbonic acid within about one hour—a quantity of carbon much greater than it contained. Hence it has a most powerful indirect influence in promoting oxygenation, and is especially fitted for the condition in which there is a disposition to excess of free acid. I am desirous to press this upon your attention; for I believe that, in such cases, tea is our best and most certain medicine. I have also found it of great value in cases of night perspiration, when a large breakfast-cup is taken cold in the night—of greater value far than any other remedy, except the removal of too much bedding, or warmth, or damp. The detection of the exudation of the blood, in which there is an excess of free alkali, is rather by negative than positive indications; but when there is wasting, langour, defective appetite and assimilation, soft and perspiring skin, and no excess of acid in the urine or perspiration, we have the indications of its existence. This is a condition which prevails in hot weather and climates, and one on which the administration of phosphoric as well as the vegetable acids is of great avail. Fat is commonly less indicated, and such remedies as tea may be injurious by increasing the waste. Cold bathing with salt and water is of great value in both classes of cases, but especially the latter; and the preparations of iron in cases of anæmia are then also much more indicated, since the excess of alkali promotes their solution, and permits a larger amount to be collected and retained in the blood. Milk is a food which I advise to be taken at frequent intervals of the day, and especially in the early morning, and is in a degree applicable to both these states of the blood; since, while it has a free alkali and albumen on the one hand, it has fat and phosphates on the other. I believe, however, that skimmed milk, or milk made sour, is much more fitted than new milk for cases in which there is no excess of acid; and it will be found to be better borne by the patients. Milk has a direct action, by suplying plastic and heat-forming materials; and an indirect one by its casein, in promoting assimilation. I also take the opportunity of saying that we have been too much led away from the use of milk and cream in supplying fat to the system, by the habit of administering separated oils; and I think it is easily capable of proof that the combination of the oil in a molecular state with the other components of the milk renders it far more assimilable than any separated fat can be, and especially a fat administered alone; that is, apart from the nitrogenous substances which I have proved to be necessary for its assimilation.

From the point of view in which I have been accustomed to regard phthisis for some years, I have considered that voluntary deep inspirations were well fitted as adjuncts to treatment; and this, both because with greater amount of air inspired even voluntarily there is increase in the chemical changes which follow, and because with a lessened degree of distension and vital action proceeding in the air-cells there must be a progressive tendency to a degradation of their structure, and, as I believe, to the filling up of their cavities by epithelium, or the earliest state of tubercle in these organs. The advantage of this method has for advocates no less authority than Lehmann, who, on physiological grounds, commends it in cases of deficient respiration, in bronchitis; and Piorry, who has recently affirmed, as may be seen by an extract in the Medical Times of January 5th, 1859, that by this means he has been able to cause a large amount of expansion in cases where *deposits* even had heretofore prevented it. Dr. Muhry, a German writer, who has recently treated of climate, seasons, and elevation, over the degrees of expansion of the lungs, has supported the view that expansion of the air-cells is a desideratum of great importance. Hence, in addition to my own affirmation that in numerous cases of early phthisis I have been able to increase the existing diminished vital capacity of the lungs, I think there is evidence to convince the minds of those who regarded my plan with distrust, that there is good reason for its adoption, and that it is not too mechanical to be excluded from physiological and even pathological employment. The increase in the function of the lungs, and the prevention of that tendency to deficient expansion of the air-cells, both alike tend to prevent the further deposition of tubercle; and if, from various reasons, they should fail, the present unsatisfactory result of all kinds of treatment leaves us nothing to regret. I cannot but attach great importance to both of these objects; viz, the increase in the vital actions of the vesicles, and their due expansion and expansibility; and both, in fact, are accomplished in our ordinary directions for violent exercise for the maintenance of health.

I cannot avoid the expression of my conviction that in many cases, owing to the deficiency of vital action, whilst the appetite for food is not materially lessened, the system becomes overburdened with ill-digested material, tending to the accumulation of carbon in the blood, and materially impeding the respiratory process. In such cases, diminution of food, with increase in the action of the liver and bowels, do very much to relieve the oppression of the breathing, and to aid in the work of increasing the expansibility of the air-vesicles. In such cases, alcohols are directly contraindicated; and, indeed, in almost all cases of phthisis (excepting, perhaps, those in which the depressed action of the heart is a marked feature), the advantage of the employment of alcohols is apart from, and in spite of, the alcohol.—*British Med. Journal, Dec.* 3, 1859, *p.* 982.

32.—ON THE USE OF THE HYPOPHOSPHITES OF SODA AND OF LIME IN THE TREATMENT OF PHTHISIS.

By Richard Quain, M.D.

[The plan of treatment of phthisis, by phosphorus, recommended by Dr. Francis Churchill, has been tried by many physicians, and has been reported fully by Dr. Cotton (Retrospect, vol. xxxvii., 72). Dr. Quain has lately, in consequence of the perusal of Dr. Churchill's treatise, been engaged in re-examining the subject, and has embodied the results of this enquiry in the following article.]

It will, perhaps, be fair to say, in the first instance, that Dr. Churchill states that he was led to adopt the use of the hypophosphites in consequence of his belief that the tuberculous diathesis depended on some disturbance in the process of sanguification; that this disturbance, which affected the inorganic and not the organic elements, was due to a deficiency and not to an excess of some one or other of these elements. He argued with himself, that it could not be the sulphur, the iron, the chlorides, or the alkalies, for these substances were daily used as remedies, without any real effect on the disease. Eliminating, then, the elements first-named, he concluded that the failure was in phosphorus as a constituent of the body.

It should here be noticed that these propositions of the author can only be regarded as theoretical speculations, inasmuch as they are unsupported by either chemical or physiological observations.

By a similar course of reasoning, but one more in accordance with physiological facts, Dr. Churchill arrived at the conclusion, that phosphorus, the missing element, could be best supplied by the administration of this body in its lowest state of oxidation, as it was thereby given in a form more capable of assimilation. With that view, he administered the hypophosphites of soda and of lime, which he declared to be prophylactic, and to be curative in every stage of the disease. He says: "I *know* that they will prove, not only as sure a remedy in consumption as quinine is in intermittent fever, but also as effectual a preservative as vaccination in small-pox."

Encouraged by statements like this, and by a lengthened catalogue of the phenomena of improved health which it was said resulted from the use of these remedies in Dr. Churchill's hands, I determined on giving them a fair trial in a certain number of cases. They were, therefore, administered n twenty-two cases, taken without selection from amongst the ordinary in-patients of the Brompton Hospital. Of this number (twenty-two), twelve were males, and ten were females.

Stage of the Disease.—Two cases were in the first, ten in the second, and ten in the third stage of phthisis.

The Dose of the Remedy.—Dr. Churchill recommends ten to thirty grains as the dose, of either the hypophosphite of soda or of lime, daily, in any simple fluid. The dose to be increased, until the general symptoms disappear. In some cases, he prefers the one salt to the other. For example, he thinks that the salt of lime checks the expectoration, and thereby increases the cough; whilst the salt of soda is less energetic in its action. I met with nothing confirmatory of this impression. The dose given to the patients at Brompton was, in the first instance, ten grains, three times a-day, except in the case of a child, when only five were given. The disease progressing, or being stationary, or the effects of the remedy being *nil*, the dose was gradually increased. Thus, in four cases, it was increased to a drachm three times a-day; in ten cases, the dose reached two scruples or more; in eight, the dose remained under half a drachm. It will thus be seen that the remedy was given freely. In no case, let me add, was there any appearance of the troublesome symptoms indicated by Dr. Churchill as following large doses.

The Duration of the Treatment.—One case was under treatment for six months, one for four months, six for three months, nine for two months, five for one month. During this lengthened course of treatment, I looked anxiously, but in vain, for those marked physiological effects described by Dr. Churchill. There was no evidence of the "improved powers of innervation;" "the hair and nails did not grow more rapidly;" there was no "appearance of plethora or of fulness;" the patients did not describe "an unaccustomed sensation of feeling better and stronger after a few doses of the remedy." Nay, I would say that there was nothing more felt by the patient nor noticeable by the physician than if so many grains of carbonate of soda or prepared chalk had been taken.

The Results.—To return, then, to the more immediate object for which these agents were administered, viz., to ascertain their value in the case of consumption—I have to state, that of the twenty-two cases, six were more or less improved while under treatment. Of these six, three were improved in but a slight degree, and only for a short time; in three the improvement was marked, but in one only of the latter has the improvement been permanent; of the two other cases one continued using the hypophosphite for three months after leaving the hospital, during which time she grew gradually weaker, and finally died; the other, a man, after leaving the hospital, continued the treatment for some time, but gradually grew worse, and is now dying. All the other sixteen cases steadily lost ground whilst using the hypophosphites in the hospital. Happily, in six of these cases, the treatment by hypophosphite was suspended, and the usual treatment by cod-liver oil, tonics, &c., being substituted, a decided improvement in each was the result.

[The opinion of one who signs himself "R. C. H." is entirely opposed to that of Dr. Quain. He says:]

I have now prescribed the hypophosphites in about twenty cases. They were in various stages of the disease, chiefly in the first and second stages; two were in the third stage. One case has taken the remedy twelve months; all the others, about six months. They are still under treatment. So far from no physiological effect being produced, I have generally found in each case the following effects: A great increase of appetite; increase of animal heat (in some cases, not in all); a *marked diminution* of the expectoration in every case; the patients also expressing a feeling of improvement in their breathing power—that they could fill their chest better with air. On making an examination of the chest with the stethoscope, I have found the moist clicks to diminish in number; in one instance they disappeared entirely, and were replaced by vesicular breathing, perhaps somewhat harsher than natural: this was an instance where the disease was of limited extent in the upper part of the right lung, but nevertheless a well-marked case. The hypophosphites appear to act as respiratory excitants; and I should certainly be disappointed upon testing their breathing power with the spirometer, if the volume of air inspired at each act of inspiration was not increased. I have not a spirometer, and, therefore have not made the experiment; but, judging from the respiration becoming almost puerile in some portions of the lungs, when before its administration the respiratory murmur was so feeble and indistinct as to be scarcely heard, and also from the loss of breathlessness experienced by the patients themselves, I certainly should, from a consideration of the above phenomena, expect to find that such would be the case. Physicians connected with public institutions could easily settle this point. In October last I gave a young man, æt. 20, five grains three times a day. He had all the symptoms of phthisis; had diarrhœa; was drenched with morning perspirations; had no appetite; and his loss of flesh and strength was so rapid, that it threatened to be a case of galloping consumption—there was unmistakable evidence of crude tubercle in the lungs. When he had taken the hypophosphites for a month, his appetite became *voracious;* he gained weight at the rate of three pounds a week for three successive weeks, and felt so strong and well that he resumed his work as a factory operative, and has continued at it until the present time. I saw him again last Saturday (February 25th); he is now losing flesh, and has a dry cough, with partial aphonia—no night sweats; I advised him to rest, and resume the hypophosphites with what results, time will show. No other remedial agent was used in this case, except nutritious diet.—A sister, æt. 25, of the last patient, has persevered with the hypophosphites for twelve months. She has had chronic phthisis for five years: it had been kept in abeyance by cod-liver oil, until twelve months ago, when, from some unexplained cause, her digestive organs would not tolerate it any longer, when she rapidly declined in health. I then gave the hypophosphites. The improvement was more marked and permanent than in her brother's case. I saw her again on February 25th, when she expressed herself as being quite well—never better in her life. She had a robust appearance, and was very stout. All the general symptoms have disappeared. I had not an opportunity of examining the chest. So far from no effect being produced, I am in the habit of prescribing hypophosphites in any exhausted condition of the system where loss of appetite and debility are the most prominent symptoms. The following case briefly illustrates their utility in a case of strumous dyspepsia, attended with debility:—On June 14th, 1859, the treatment with hypophosphites began: his weight was six score seventeen pounds; no appetite, and weak. June 20th, seven score one pound. August 13th, seven score five pounds, his appetite and strength keeping pace with his increasing weight. On January 14th, 1860, I gave the hypophosphites to a farmer, æt. 43, who had taken various tonics for a month with no improvement to his appetite and strength.

On making an examination of his chest, I found evidence of a slight deposit in the upper part of the left lung. He had gradually lost flesh, and had been troubled with dyspeptic symptoms for six months. On the administration of the hypophosphites, his appetite gradually returned, and he began to gain weight, having gained four pounds in a fortnight. Surely here is evidence of some physiological power? The largest dose I have given has been half a drachm a day, in doses of ten grains each. Very few of my patients can take more than five grains three times a day, without producing headache and unpleasant feelings of fulness about the head. In one case, epistaxis came on. All my patients took plenty of out-door exercise when the weather permitted. No very satisfactory improvement took place in a less period than three weeks. I should be afraid to give a drachm-dose three times a day.—To recapitulate, I claim for the hypophosphites a power of stimulating the appetite for food—which has taken place in every case; a power of rapidly diminishing the expectoration in all cases under my observation, and, in some, completely arresting it; a power of increasing the expansion of the chest; and, theoretically, of being respiratory excitants; and they present us, at the same time, with "very efficient blood-generating agents." My experience does not enable me to say they cure phthisis; but that they retard its progress, particularly that of *softening*, I am quite convinced; and that they are a useful auxiliary in the treatment of phthisis, whatever other power they possess, has been established to my satisfaction.

[Dr. Dickson, of Jersey, writes thus:]

In the month of October last, while in Paris, I visited Dr. Churchill's Dispensary, and being satisfied from what I saw that there was something in his treatment of consumption by the use of hypophosphites, on my return to this place I put the treatment in practice, and have found it so beneficial in the majority of the cases so treated that I am induced to persevere. I have tried it in about thirty cases, *in every stage of the disease*, and in the majority—say *two-thirds*—the improvement has been very marked. My experience has proved that it is decidedly beneficial in the earlier stages of the disease, although one very material effect of the medicines (viz., their power of checking *night sweats* makes them very useful in all stages. What convinced me most in the cases I saw under Dr. Churchill's care, was the fact that those who presented themselves at the Dispensary were generally those of the lower classes of society, who came there labouring under phthisis (as proved by auscultation and percussion), and who then returned to their daily avocations, bad ventilation, bad drainage, bad food (half the time perhaps nearly starved), and obliged to work for that food—therefore I reasoned, if these cases improve there must be something in the treatment. Since my return from Paris, I have requested several medical friends to try the treatment on their patients, and they have mentioned to me that the *result was most extraordinary*. They have given the treatment a much fairer trial than Dr. Cotton and the Brompton M.D.'s have ever done. Time alone can show how far the improvement may be permanent. My experience, however, satisfies me that in the hypophosphites we have a powerful agent, and that, on the score of humanity alone, they ought to have a fair and impartial trial. I may mention that the result of the treatment on a near relative of mine, who has been for some months under Dr. Churchill's care, is as satisfactory as it is gratifying.—*Lancet, March* 17, 1860, *p.* 267, *and Med. Circular, March* 14, 1860, *p.* 177.

33.—*Pneumonia: Production of Ægophony.*—At a recent meeting of the Association of the King's and Queen's College of Physicians in Ireland, Dr. Law read a paper on a case of pneumonia which he had had under his care in Sir Patrick Dun's Hospital. The inflammation had occupied a very large portion of the left lung, occurring in a young man 23 years of age. The case had been treated by the administration of minute doses of calomel frequently repeated. Dr. Law is often able to produce salivation with three and a half or four grains of calomel, by giving but the twelfth of a grain each dose, and repeating it every hour. He considers that the system becomes more easily affected by the mercury if it be combined with small doses of tartar emetic. In the present case the disease was completely got under in five days. In the course of his observations, Dr, Law combated the opinion of Laennec as to the manner in which ægophony was produced. He did not think that great author correct in attributing the peculiar sound referred to, to the action of the voice in causing a vibration in the fluid effused in pleuritis, but rather believed it to be due to the vibration in the compressed lung, and looked on ægophony and bronchophony as produced by different degrees of compression. He held that there might be ægophony without any effusion into the cavity of the pleura.—*Dublin Med. Press.*—*British Med. Journal. Jan.* 21, 1860, *p.*

34.—SIMULATION OF PNEUMOTHORAX BY AN ENORMOUS CAVITY IN THE LUNG.

Under the care of Dr. James Jones.

The following case is interesting, as an example of the simulation of the physical signs of pneumothorax by an unusually large tubercular cavity. The size of the cavity, and the almost complete limitation of disease to one lung, are also features of interest:

M. A. T., aged 20, was an out-patient of the Infirmary for Consumption, Feb. 1859, under the care of Dr. James Jones. The early history of her case, so far as could be learned, presented all the symptoms of phthisis. At the time of her admission she was mush wasted, pale and weak. The left side of the chest was very resonant throughout, except over the posterior part of the lower lobe of the lung where, for the space of four inches, there was well-marked dulness on percussion. Amphoric resonance was heard all over the side, but loudest between the fourth and sixth ribs, where, also, metallic tinkling was frequently audible. Pectoriloquy was painfully strong. There was no displacement of the heart, no distention of the intercostal spaces, nor was any change in the site of the dulness on percussion produced by alteration of posture. About two months before death, œdema set in in the feet, and soon became general. The lips were not livid until the last week of life. Dyspnœa increased concurrently with the œdema, and loud mucous *rales* were audible all through the right lung. There was abundant expectoration of a mixed kind, partly muco-pus and partly viscid mucus. From the setting in of œdema the urine became loaded with albumen. Consciousness remained perfect to the last. She died on Dec. 19.

Necropsy.—The left lung was found completely excavated from the apex downwards, to within about three inches from the base. The interior of this huge cavity was smooth, the lower part being imperfectly sacculated. Its external wall was from one to two lines in thickness, and strongly adherent to the costal pleura. It contained about half a pint of very fluid pus. Several bronchi of the third and fourth magnitudes opened into it; some above and some below the level of the fluid. The right lung was highly congested, exuding on incision an abundance of frothy fluid. It contained a sprinkling of granular gray tubercles. A tubercular cavity the size of a kidney-bean was found in the apex. The liver was healthy and free from tubercle. The heart also was healthy. The kidneys were not examined.

The points of interest of this case, as pointed out by Dr. James Jones, are,—

Firstly. The almost complete absence of old-standing disease in the right lung at a period of so advanced disease of the left.

Secondly. The enormous size of the cavity which excavated nearly the whole of the lung. The continuance of life under such circumstances depended to a great extent on the capabilities of the other lung in performing the functions of respiration to the extent necessary for existence.

Thirdly. The almost perfect simulation of the physical signs of pneumothorax resulting from the large size and the smoothness of the interior of the cavity, the thinness of its external walls, and their close adhesion to the thoracic parietes. Generally when amphoric resonance and the metallic clink are produced by tubercular cavities, they are heard only in the upper part of the chest—in a word, over the cavity—but here they were audible all over the chest, and most clearly between the fourth and sixth rib, as is usually the case in pneumothorax. A splashing sound was heard low down in the chest when the patient coughed. No respiratory murmur was audible in any part of the left side, the portions of lung which were left unconsumed by the disease being too much solidified to admit the entrance of air into their structure. The absence of any displacement of the heart, or of any distension of the intercostal spaces, could not much assist the diagnosis, as their presence or absence in pneumothorax must depend on the greater or less freedom of communication, through the bronchi, between the air in the pleural cavity and the external atmosphere.

In the investigation of cases of this kind, the absence of suddenness in the manifestation of the physical signs will assist the diagnosis. A tubercular cavity is formed with comparative slowness, and the stethescopic signs gradually develop themselves; pneumothorax arising from the formation of an opening between a vomica and the cavity of the pleura, is sudden: to-day there is no amphoric sound, to-morrow there is. The advanced period at which these cases frequently present themselves at the hospitals and dispensaries unfortunately, however, deprive us of this source of information.

The various amounts of resonance to the stroke in different parts of the chest, depending on the usual inequality of the thickness of the external walls of the cavity, will also afford some assistance.

The quantity of fluid, also, contained in the cavity is seldom so great as to cause a marked change in the seat of dulness on percussion by alteration of posture. Besides which, the irregular shape or sacculated condition of the bottom of the cavity, where the progress of excavation is most recent and imperfect, prevents its fluid contents from flowing freely, whereas, in pneumothorax any fluid present immediately changes its seat, in accordance with the posture of the patient.—*Med. Times and Gazette, Feb.* 18, 1860, *p.* 166.

35.—EPISTAXIS ARRESTED BY INJECTION OF PERCHLORIDE OF IRON.

By Dr. Fountain Davenport, Iowa, U.S.

I am induced to publish the following case from reading the report of "Death from Epistaxis," occurring in the practice of Dr. Triplett, of Virginia.

About two years ago I was summoned in haste to see a young man who was reported to be in a dying condition from loss of blood. The bleeding had continued uninterruptedly for about thirty hours, escaping constantly from the nostrils, and frequently thrown out in clots from the posterior nares.

He had been attended by a German physician, who had not succeeded in arresting the hemorrhage, and before my arrival he had abandoned the case, from motives which I need not here mention.

I found him in a frightful condition, his face, hands, linen, and much of the bed-clothing, and the floor, being covered with blood. In one corner of the miserable apartment, where many people were crowded, was a pile of rags, and towels, saturated with blood.

He was so impoverished that he could not support himself in an upright position, and the extreme pallor of his skin and colourless lips indicated plainly that he had lost a large quantity of blood.

I immediately plugged the nostrils, anteriorly and posteriorly, in the usual way. I supposed this would arrest the hemorrhage, but I was mistaken. Very soon the blood escaped through the plugs in each direction, and the flow returned as profuse as before. I then removed the plugs, and rolling up two quite large pieces of fine, dry sponge, I introduced them as before, and so firmly, that I thought it would be impossible for a drop of blood to escape. I then left him, and returning in an hour after I was astonished to find the blood escaping as freely as ever. I again removed the plugs, and washing out the nostrils by injections of cold water, I pressed into each a roll of *tannin*, made into a soft mass with a little water and glycerine; I packed the nostrils full with this, but it did no good. I then introduced the plugs a third time, using compressed sponge, and forcing them in so firmly, that I feared I might have great difficulty in removing them. I then had ice applied constantly to either side of the nares, and kept the patient perfectly quiet. This answered the purpose for about half an hour, and then the bleeding returned as bad as ever. By this time the patient experienced the alarming symptoms of excessive loss of blood—ringing in the ears, occasional blindness, &c. The case looked desperate to me, while the patient, and even some of his friends, protested against further effort, as useless and cruel. I saw that it must be checked very soon, or that it would surely be fatal. I removed the plugs with some difficulty, and washing out the nares, I passed into each a piece of nitrate of silver, about a quarter of an inch in length, carrying them back with the forceps about four inches. I also introduced a curved injecting instrument, perforated towards its extremity with a number of small openings, and forcibly injected a strong solution of the same caustic. This did no good.

It now occurred to me that the perchloride of iron might answer the purpose. I soon procured it, and after washing out the nostrils as before, I injected a quantity of the undiluted perchloride into each nostril. This immediately checked the bleeding, and proved an effectual remedy.

Twenty-four hours after this the bleeding again returned, when I repeated the injection, and with the same happy effect.

There was no recurrence of hemorrhage after this, and the patient soon recovered, so as to go about, but for quite a long time felt the effects of the loss of so much blood, which had been so excessive as to leave him a very slender hold upon life. I know not in what other way it could have been saved; and the publication of this may be the means of rescuing others in similar circumstances.—*Medical Circular, March* 14, 1860, *p.* 170.

36.—*Injection of Sulphate of Atropine in the Track of the Pneumogastric Nerve in Asthma.*—The last Paris novelty consists in this treatment, by M. Courty, of the paroxysms of asthma in a case which had resisted a great variety of medicinal agents. He injected along the track of the pneumogastric nerve, on the inner side of the sterno-cleidomastoïdeus and on a level with the thyroid cartilage six drops of a solution of the sulphate of atropine, which produced vertigo, dilatation of pupil, and other symptoms of narcotisation. Next day the injection was repeated on the other side, and thrown in more deeply, with the effect of producing still greater, though not alarming narcotism. A third injection, two days after the last, completed the treatment—the asthma having gradually diminished, and now, four days after the first injection, entirely ceased. The patient (a lady, aged 54) continued quite well two months after the treatment had been put into force.—*Moniteur des Sciences.—Med. Times and Gazette, Nov.* 26, 1859, *p.* 535.

37.—ON TRACHEOTOMY IN CROUP.

By James Spence, Esq., F.R.C.S.E., Surgeon to the Edinburgh Royal Infirmary.

[The operation of tracheotomy is often spoken of as one perfectly simple and easy; but when a patient is struggling in the agonies of suffocation its performance is by no means so simple as generally described. Success depends a great deal upon attention to little minutiæ in its performance.]

There ought to be no attempt at rapidity or brilliancy in this operation; every step should be methodically gone about. First, as regards the position of the infant, the shoulders should be well raised, the head bent moderately backwards and supported by an assistant, who should pass one arm under the pillow behind the neck, so as it were to project or support it forwards, whilst with the other hand he keeps the head fixed with the chin in the middle line as a guide to the surgeon; the arms and legs may be controlled by a small sheet or large towel pinned round the body, the arms placed by the side; and when thus secured, one person can control the movements: care should be taken to see that the teeth are not firmly closed, but that the mouth be partially open and the nostrils kept free.

The external incision should begin over the cricoid cartilage, and extend downwards exactly in the middle line for at least two inches. In general, two large veins, one on either side of the mesial line and close to it, are seen when the skin is divided; the fascial texture between these, corresponding to the intermuscular line is readily divided by the point of the knife, and then these veins can be easily drawn aside. Sometimes one crosses the line to join the other, and is embarrassing, and may even require to be divided after being secured by ligatures. But the point most to be attended to is caution after separating the tracheal muscles. If the finger be placed deep in the wound at this stage, the trachea is felt pretty distinctly, and may seem so distinct as to warrant the surgeon entering the bistoury to divide the rings; but I hold this ought not to be done, because not only may there be the substance of the thymus gland flattened under the finger, but deeper, and passing from that gland to the thyroid, there exist numerous vessels which would bleed profusely. I have more than once pointed out this to the gentlemen assisting me at my operations. After separating the lobes of the thymus gland, even after these vessels and the thymus are pushed aside, I clear the rings of the trachea with the knife from loose cellular tissue, so that there may be no obstruction to the entrance of the silver tube when the opening is made in the trachea. On the tube being introduced, the head should be bent slightly forwards.

The form of the tube itself is important: it ought to be of such a size as will admit air freely without over-distending the trachea, and always double; the inner tube fitting pretty close, but not secured to the outer, either by spring or other fastening, so that it may be easily removed for cleaning, or expelled by the efforts of coughing, if obstructed by mucus.

In the after treatment, when there is much bronchial effusion, the use of emetics is very beneficial; but under no circumstances would I now resort to antimony, for its effects are most dangerous, as diminishing the expulsive power, and depressing the patient, inducing sinking. I am glad that my opinion on this head is strengthened by the high authority and great experience of M. Trousseau. In all respects, I find ipecacuan answer better as an emetic, without depressing or leading to dysenteric purging, whilst it induces moderate diaphoresis and allays the febrile condition. M. Trousseau is very decidedly opposed to the use of blisters in these cases, on account of the sloughing which follows. But in this respect I cannot so fully concur; for though I would be far from advocating the use of the ordinary fly-blister, still I have found the application of the liquid blister highly useful in some cases of bronchial complication, and when carefully attended to, I have never seen any bad results.

In regard to diet, in many cases it is necessary to give beef-tea and wine from the first; but in general I prefer for the first day or two a nutrient non-stimulating diet, such as milk and farinaceous food, and afterwards gradually giving animal food; but in respect to this we must be guided by the condition of each individual patient.—*Edinb. Medical Journal, Feb.* 1860, *p.* 698.

38.—ON THE MERCURIAL TREATMENT OF CROUP IN THE RURAL DISTRICTS.

By C. Ferguson, Esq., High Hesket, Carlisle.

[It is too much the practice to record the experience of large hospitals and towns, and overlook the experiences of country practitioners. Disease presents very different characters in large towns to what it does in country places. The following communication is one of much interest.]

The treatment was first suggested by one of the Hamiltons, I believe the younger. My father would give to an infant of six months a grain and a half of calomel every hour till it purges freely; to a year-old child two grains, and to one two years old sometimes even four grains, until you get the bowels well acted on, or it vomits repeatedly. He has always noticed that the stools are of a greenish colour, something like chopped spinach; and immediately consequent upon this discharge an alleviation of the symptoms follows. When that is observed, the dose must be repeated less frequently, perhaps only once in two hours for some time, then still seldomer, and, lastly, gradually abandoned altogether. Should the child be much weakened by its use, or from other causes, stimulants must be given freely. He says it is wonderful how much calomel children can take *in a short time* without affecting the bowels or purging violently afterwards. Sometimes he has known fifty, sixty, and even a hundred grains to have been given before the disease succumbed, or any material effect was produced. Salivation in this way cannot be produced, give it to any extent you like, *in children*. Of course he used occasionally, as the case indicated, auxiliary treatment of different kinds, such as emetics and the warm bath, both of which he is extremely fond of.

These suggestions, you know, are the result of experience in a *purely country practice*, where the children are strong and robust, and so different from those in a densely-populated town; and in carrying out such a plan of treatment you must be guided by that difference in constitution and stamina, and modify it accordingly. For instance, in strong children, he used to leech preparatory to the mercury. I suppose you would hardly do that in any case in London. However, that does not affect the virtue of the mercurial treatment, he thinks, in the least. By the way, he would commence it at the most advanced stage—in fact, as long as he could get it swallowed. He is fond of an emetic of pulv. antim. tart. at the commencement, if the patient can bear it, and occasionally applies sinapisms over the root of the neck.—*Lancet, March* 10, 1860, *p.* 241.

39.—ON THE ANATOMY OF THE LUNG.

By A. H. T. WATERS, M.R.C.P.L., Liverpool, and JAMES NEWTON HEALE, M.D., Winchester.

[Dr. Waters and Dr. Heale are at issue upon the anatomy of the bronchial arteries. The following extracts from the correspondence on the subject express the views of each on the subject. Dr. Waters observes that the opinions generally entertained by anatomists may be summed up as follows:]

That the bronchial arteries, being distributed to the bronchial tubes, areolar tissue, &c., return their blood *partly* by the bronchial veins, and *partly* by the pulmonary veins; the bronchial veins being supposed to accompany the arteries *within* the lungs. Such was the view I entertained when I began my researches; but observation of a large number of human and other mammalian lungs convinced me that no bronchial veins exist *within* the lungs, and that the arteries pour their contents, except so far as they supply the structures about the root of the lung, into the pulmonary veins.

This was to me the discovery of a *new fact*, alike opposed to the opinion of Dr. Sharpey and other anatomists; and although I subsequently found that a similar view had been held by Reisseisen, sixty years ago, yet that author gave no proof in support of his opinion, and it remained without being refuted or confirmed. I do not, therefore, claim, with reference to the fact, priority of discovery; but I venture to think I have given the proofs which were wanting to establish it, and I believe subsequent researches will confirm them.

I shall not dwell on any physiological or pathological importance the fact may have, but simply refer to another instance in the body where an artery terminates in a somewhat similar way—viz., in the liver.

In my Essay I have endeavoured to do justice to previous writers, and I have pointed out what I believe to be the cause of the mistake—for such I must be allowed to consider it—into which Dr. Heale has fallen; and to the observations I have made, I beg leave to refer that gentleman. His views are of so novel a character, that they naturally arrested my attention, and I was particularly cautious not to reject them until repeated examination and experiment had satisfied me of their unsoundness.

[Dr. Heale writes:]

Dr. Waters seems to take up the ordinarily received opinion that the bronchial mucous membrane is supplied from the (so-called) bronchial arteries; that these anastomose with the pulmonary system of vessels, and that the bronchial arteries are the vessels by which the bronchial mucous is furnished.

1st. Were this indeed the fact, it would inevitably happen, in all cases of chronic bronchitis and bronchorrhœa, that the bronchial arteries would become prodigiously enlarged. I deny that such takes place.

2nd. Were that the true distribution of the bronchial arteries, whenever these vessels were fully injected it must necessarily occur that the bronchial membrane itself would also be injected. I assert, on the contrary, that it is impossible to inject the bronchial membrane from the bronchial artery; that the thing has *never* been done, and *never* can be done. There are many who will SAY that they have accomplished this; but I challenge them to produce a single instance (proper precautions which I shall enumerate having been taken) in which this has ever been fairly done. Should even one such case be duly established, I will assent that my views shall be considered as utterly refuted. I make it a *sine qua non*, however, that the bronchial arteries shall be injected from the aorta, otherwise it is quite easy to select a small twig of a pulmonary vein, and inject it under the supposition that it is a branch of the bronchial artery. This fact will account for many of the blunders which have been made on the subject. Moreover, I stipulate that the subject selected for injection shall not be a child that has died of croup, nor a patient who has suffered from extensive ulceration of the air-passages, since in such cases it might be found that the true lining membrane had been partially replaced by false membrane, and this might possibly be injected from the bronchial arteries; but even in such a case, the distribution of the blood-vessels would be strikingly different from that which prevails in the true bronchial membrane.

When the bronchial artery has been minutely injected, if the bronchial tubes be laid open, certain injected bloodvessels will be obscurely visible, and at the first sight one might, perhaps, be tempted to declare triumphantly that the bronchial mucous membrane was injected, but if a careful inspection be made, it will be found that the mucous membrane may be wholly raised, leaving the injected vessels behind, and if this raised membrane be then clarified by acetic acid, spread out on glass and compressed, a few hair-like vessels, sparsely scattered through its tissue, will be seen, by the aid of an object-glass, with a focal distance of half an inch. This fact shows that the cellular tissue merely of the raised membrane derived its supply from this source, but that the bronchial plexus and the function which this discharges are wholly distinct from the bronchial artery, and its branches, since had there been any anastomosis between that artery and the bronchial plexus, the latter could not have failed to have been injected when vessels of such extreme minuteness as those above described were filled. If the bronchial tube, after it has been thus denuded of its lining membrane, be then subjected for many weeks to maceration in salt and water, and if the flocculent tissue which continues to float upwards be daily, or several times a day, removed by means of soft brushes, (those used for painting on velvet are such as I have employed,) at length the fibro-cartilaginous structure of the bronchial tube will alone remain, and then the minute distribution of the bronchial artery will become apparent, and the fibro-cartilaginous structure of the bronchial tube will be found to be minutely injected from it.

On the other hand, in those specimens in which the pulmonary set of vessels, and not the bronchial arteries, have been injected, it is found that the fibro-cartilaginous structure of the tubes is not in the slightest degree injected, however minutely the plexus surrounding the air-cells, and that lining the bronchial tubes, may have been filled, proving that there is, likewise, no anastomosis in this situation.

Whenever the pulmonary veins are injected with fluid, so as to spread at all minutely, it is impossible to avoid injecting the membrane lining the bronchial tube—the connexion is intimate and direct. The distribution, also, of the vessels lining the bronchial tube, which are filled by this means, is totally distinct from, and unlike to, anything that prevails amongst the capillary vessels in connexion with the bronchial artery in any part of its ramifications. Nothing can be easier than to inject the plexus lining the bronchial membrane from the pulmonary veins, but, as I have said before, it is wholly impossible to do so from the bronchial arteries.

Furthermore, it is not difficult to inject the bronchial mucous membrane from the pulmonary artery; but in order to do this effectually, it is necessary to tie the pulmonary veins, because it requires a greater force to compel the fluid to flow through the plexus of the bronchial tube than it does to make it pass through those capillaries which surround the air-cells; therefore, unless the precaution be taken to tie the opening of the pulmonary veins, the injection will make its escape in this direction, and the bronchial memdrane will be only partially injected.

To sum up these particulars:—The air-cells and the mucous membrane lining the bronchial tubes are supplied from the pulmonary system of blood-vessels, and the basement fibro-cartilaginous structure of the bronchial tubes and the cellular tissue throughout the lungs are supplied by the bronchial arteries derived from the aorta; and these latter have no sort of anastomosis nor connection with the former, but are carefully secluded from them by much flocculent tissue, which requires a considerable amount of dissection in order to be got rid of, so that the ultimate distribution of the branches of the bronchial artery may be clearly demonstrated, when these latter have been injected instead of the pulmonary set.—*Lancet, Feb.* 18, 1860, *p.* 179.

DISEASES OF THE ORGANS OF DIGESTION.

40.—ON THE DIAGNOSIS AND TREATMENT OF ULCER OF THE STOMACH.

By Willoughby F. Wade, B.A., M.B., Physician to the General Dispensary, Birmingham.

[Dr. Brinton in his recent work on "Diseases of the Stomach," states that ulcer "occurs very frequently, that it may generally be detected in the living subject; . . . . . that is usually curable." With this opinion Dr. Wade agrees, observing that symptoms of a peculiar or special character and grouping are observed in these cases, and that these symptoms differ materially from those observed in ordinary dyspepsia.]

The symptoms which enable us to discover the existence of this lesion are: pain after food, and the length of time after eating at which this comes on. This varies in different cases from a few minutes to an hour or two, but it is pretty constant in any given case; the pain is severe, and burning or lancinating, felt in the epigastrium and in the corresponding region of the back, sometimes radiating into the shoulders or down the legs; it is relieved often by one position, aggravated by the opposite one, thus giving us a clue not only to the existence of an ulcer, but to its site. It usually is not much alleviated till vomiting occurs, and from this the relief is speedy, sometimes instantaneous. The ejecta consists of food, more or less digested, with or without a quantity of thin watery or viscid mucus, and at times blood; the appetite is often unimpaired, the tongue natural or pallid, and thirst often absent; tenderness of the epigastrium is seldom wanting, though its extent is generally limited, so that it may escape notice. Tenderness is not unfrequent at that part of the back, in which pain, when dorsal, exists. There is often an absence of heartburn, pyrosis, sour eructations, flatulence. The regularity of the bowels depends chiefly upon the amount and kind of food taken into, and retained by the stomach. This remark applies also to the degree of emaciation present, which is also dependent in a great measure upon the extent of the ulceration. Another very marked peculiarity of this disorder, is the worn and suffering countenance presented by patients who have suffered from it for any length of time, quite different from the nervous or fidgetty look of simple chronic dyspepsia. If the stomach have become adherent to any other viscus or to the parietes, or if, as may happen, there be much inflammatory infiltration of the gastric tissues, we shall possibly discover by manipulation some tumour or percussion-dulness or hardness in the abdomen. When we further reflect that these symptoms continue for months or even years unchanged, both in themselves and in their relations to each other, we have certainly a group of phenomena altogether peculiar, and, as I think, characteristic. Of these symptoms it is customary to describe the hæmatemesis as the most convincing sign of a breach of continuity in the mucous membrane; but when we remember upon how many causes it may depend, it can only be considered valuable as *one of a group*, without the other members, of which it loses much of its significance; and it is upon the *presence of all or the majority of these symptoms* in a given case, rather than upon the prominence or severity of any one of them, that I should be disposed to diagnose the existence of a gastric ulcer. At all events I am sure that we shall benefit our patient by looking upon and treating such a case as one of ulcer of the stomach. As the treatment is a somewhat irksome one, it is beneficial to tell the patient what we think of the nature of the disease, the name of which will probably alarm, and thus tend to ensure obedience to our direction.

The treatment which is more beneficial than all others is the adoption of a purely milk diet; but as a large quantity of milk would produce as much exercise and irritation of the stomach as almost any other aliment, and since the irritability of this organ varies much in individual cases, we cannot specify any fixed quantity. The patient should be directed to take as much at each time as he can do without bringing on the pain. This quantity—at first perhaps not more than a teaspoonful—may be repeated as often as compatible with the ease of the stomach; this is a simple rule by which to decide the amount of nutriment: at first, patients cry out that they shall be starved, and so forth, but they soon find that they gain strength as well as comfort, and indeed it may be well to point out to them that they will gain more strength from the complete digestion of a teacupful of milk, than from eating a mutton-chop which the stomach will reject again in perhaps half an hour. It is sometimes difficult to induce patients to try this absolute diet, but once tried the glorious ease which it produces soon commends it to them.

Patients will have some medicine; otherwise I believe that the plan here recommended is of itself not unfrequently adequate to cure. We cannot well advise a person, whom we limit to a teaspoonful of milk at a time, to take an ounce of mixture thrice a day; I therefore use pills, small in size. The irritability of the stomach in these cases suggests the use of nitrate of silver and opium, of each of which I give about a third of a grain three times a day, with one-eighth of a grain of extract of belladonna, and, if the bowels require it, a little castor oil, or henbane and colocynth. This, with a very careful return to ordinary diet, and, during convalescence, some citrate of iron, will, for the most part, readily cure the cases which I have now described, the symptoms of which indicate, as I believe, the existence of ulceration in the stomach.—*British Med. Journal*, *Oct.* 22, 1859, *p.* 848.

41.—ON THE TREATMENT OF DYSENTERY.

By W. S. Oliver, M.D., Assistant Surgeon 1st Battalion 60th Rifles, Calcutta.

[The following remarks apply to the treatment of dysentery of a peculiarly chronic character, differing much from that usually met with in Europe.]

I have tried, with varied success, every remedy that experience has suggested, but from no other medicine have I obtained the same satisfactory results as from the following three, no matter what previous treatment had been adopted. Those are pure subnitrate of bismuth, creosote, and balsam copaiba.

The cases to which I have found bismuth peculiarly applicable, are those in which the tenesmus and tormina have ceased; but their still remain colicky pains over the entire abdomen, principally referred to the hypogastrium; and, where the stools are of a dark, pitchy colour and consistence, accompanied with impaired appetite and marked abdominal pulsation, and, I may here remark, that this abdominal pulse in dysentery, as in all other abdominal irritations, has been of the greatest assistance to me as a prognostic guide, from the earliest onset of the disease.

I have been in the habit of administering creosote where there is much irritability of stomach accompanied with an inflated abdomen, tenderness on pressure in both iliac regions, and the stools consist of a frothy, coffee-coloured fluid, mixed with shreds of bloody lymph, and a few small masses of scybala; paroxysms of intense griping are generally present in those cases.

Balsam copaiba has proved a valuable remedy in those tedious cases of chronic dysentery occurring in strumous patients, who seem to have a wonderful tolerance of the medicine. It has also been of marked service in those cases of chronic ulceration of the intestine, accompanied with a stool resembling very much vermicelli soup mixed with large pieces of lymph excrescences, very similar to flat fungus growths which have become detached from the surface of the ulcers.

The following is the mode in which I have been in the habit of administering these medicines:—

Muriate of morphia, gr. ¼ to ½; subnitrate of bismuth, grs. viij. to x.; magnesiæ carb., pulv. aromatici, āā, grs. vj. three times a day. The copaiba is given in 15-minim doses, with henbane and almond mixture every four hours; and the creosote I direct to be taken in two or three-drop doses thrice or four times daily, in effervescence with tartaric acid and soda, the latter being in excess.

Should nausea be caused, as is sometimes the case, when the balsam is first administered, I generally order half a grain of opium to be taken half an hour previous.

The use of the above medicines ought to be continued for at least a week or ten days, should their good effects not be apparent before that time, which I think will seldom be the case. The usual dietetic and local remedies, with mild laxatives, must of course not be dispensed with in those cases, and full opiates at night form a useful auxiliary.

Creosote enemas, too, in combination with opiates and mineral astringents, (as first instituted, I think, by Surgeon Innes, late of this Regiment, and now Deputy-Inspector-General,) will be found one of the most decided means of allaying tenesmus in all cases of dysentery, even in its earliest stages, but more especially when it assumes a chronic nature.—*Med. Times and Gazette, Feb.* 11. 1860, *p.* 151.

42.—*Diagnosis of Cancer in the Liver.*—[In a discussion upon the subject of cancer of the liver, at one of the meetings of the Medical Society of London,]

Dr. **Harding** mentioned that he now believed one characteristic symptom to be always present in cases of, and to be pathognomonic of, cancer of the liver—that is, a small subcutaneous tumour of the breast; but in the male subject he had found it absent there, though a similar small mass had been found in the subcutaneous tissue of the loins. He had met with it in the breasts of females so affected in five cases in succession. He believed it to be a small mass of cancer; it is often painless, is not, therefore, complained of, and requires to be sought for; it may be the size of an almond or small marble, but at first not larger than a pea; it is not situated in the gland substance of the breast, but just beneath the skin, and most usually a little to the outer side of the nipple. If he (Dr. Harding) found this small mass, and at the same time discovered any symptoms whatever of hepatic or stomach derangement, he should diagnose positively the presence of cancer in the liver, even if no other sign of this latter condition existed. He had met with one case of cancer of the liver in a child, aged four years.—*Lancet, March* 3, 1860, *p.* 220.

43.—ON THE PATHOLOGY AND TREATMENT OF GALL-STONES.

By J. L. W. Thudichum, M.D.

(Read before the Medical Society of London.)

[The author commences his paper with the proposition that the formation of gall-stones must be the result of some pathological cause, either co existing with, or preceded by some period the actual deposit of these bodies. The nature of this primary disease, or diseases, is unknown, at least with any degree of accuracy or certainty. Then follows a case of heart-disease, dying with anasarca and intense dyspnœa. The age was sixty. The gall-bladder was large and contained numerous gall-stones, but the duct did not seem obstructed.]

On examining the bile in the gall-bladder, I found it to consist of a homogeneous fluid, containing little colouring matter in solution, but a large amount of brownish-yellow colouring matter, together with many crystals of cholesterine, were suspended in it. The bile and colouring matters dissolved in alcohol, yielding a yellow solution, which was separated from mucus and epithelium by filtration. The alcohol was distilled off, and the residue tested with sugar and sulphuric acid. But no colour-test could be obtained with the greatest care, and although the test was repeated several times: *there was not a trace of biliary acid in this fluid.* But it contained some margaric and stearic acids, which, together with the cholesterine, crystallized from the concentrated extract.

The gall-stones were sixteen in number. On dividing the largest one into two halves, I found a large nucleus of brown pulpy matter, which could easily be removed with the point of a knife, or washed away with a brisk stream of water from a wash-bottle, or so-called blow-bottle. When collected in a white china dish, the matter appeared to be composed of thread-like fibres, of different diameters, some a quarter of an inch long; some shorter pieces were one-sixteenth of an inch in thickness. All were cylindrical, as if moulded in tubes; many had branches, others were divided dichotomically. The thinnest portions had a diffuse broom-like end, as if the matter had not had time or quality to solidify in the tubular form, or as if it had solidified in a bag-like enlargement of the cylinder, in which the rest of the cast was moulded. The matter composing these productions was granular, without a trace of crystallization of any kind; was purely yellow in the thinnest branches, but became darker brown the thicker the forms grew. All further description becomes at once unnecessary, by the faithful representations of the more striking portions which I have caused to be executed by Mr. Lens Aldous. Some forty or fifty medical gentlemen, to whom I had an opportunity of submitting both the specimens and their drawings, have borne witness to the accuracy of the representation. All of them agreed with me that these peculiar formations can be nothing else but *casts of the biliary ducts.*

I had often searched human livers for some analogue to the casts of the uriniferous tubules but in vain; and I should certainly not have dreamed of finding them in the centre of gall-stones. I have since examined many, but only old and dry gall-stones; most of them contain a nucleus of colouring matter which has very great analogy to the matter forming the casts, but their shape I have not found repeated. It is indeed a mere chance whether such casts shall be preserved, or whether they, on being discharged from their place of formation, shall pass into the intestinal canal. And even where they arrive in the gall-bladder, and become entangled and covered with cholesterine, evidently a secondary occurrence, it is again a mere chance whether they shall not be so surrounded and interlaced by cholesterine crystals, that, on breaking the stone, they must necessarily come to pieces. They are so fragile that the mere weight of a thin glass cover, as used for microscopical preparations, was sufficient to crush the thinner ones. When shaken in the watch-glasses, in which I kept them, moistened with glycerine, the mere friction of one against the other would damage and disintegrate the most characteristic feature. About half the number of gall-stones, and among them some large-sized ones, although containing a good brown nucleus, yet did not admit of the separation of characteristic casts, although among the *dèbris* I could distinguish fragments of casts with ease and certainty.

The material of the casts was not chemically homogeneous. Alcohol extracted a yellow portion; another portion, probably bilifulvine, remained. Some had a peculiarly ragged or variably projecting outline, which made me examine for cylindrical epithelial cells; but, however, great the probability that such cells might adhere to the circumference, or enter into the body of the casts, being epithelium proper to the biliary ducts, certain it is that no such formations could be identified.

Numerous are the reflections suggested by this observation. The pathological process which effected the effusion of a congealing colouring matter into the biliary ducts was probably an acute one; if it involved only a portion of the liver, it probably presented itself at the time under the form of a true bilious attack, that is to say, a derangement of digestion, fever, headache, and a yellowish colouration of the skin, and albuginea of the eyes. If the process involved the greater part or the whole of the biliary ducts, it must have shown itself under the form of acute jaundice. It is not impossible, or rather probable, that those forms of idiopathic jaundice which hitherto have been ascribed to spasmodic closure of the duct, or to failing secretory activity, a sort of paralysis of the liver may hereafter find an explanation by the discovery of a real and material obstruction of the passages of the bile by formations similar to those which I have described. Frerichs saw a case of jaundice coming to a favourable crisis by the discharge of brown flakes and threads, which were very conspicuous upon the light coloured fæces, and ushered in the reappearance of the ordinary brown colour of the evacuations.

The branched calculi of Plater, and the branched gall-stones, which Faucanneu-Dufresne in one instance discovered in the finer ramifications of the biliary ducts, have perhaps some analogy to the casts of the bile-ducts brought under your notice.

In continuing for a moment the consideration of the etiology of gall-stones, I beg to offer some fragmentary speculations on the possible or probable circumstances which may produce gall-stones. It remains yet to be ascertained whether a foreign body in the gall-bladder can simply predispose the cholesterine to crystallise upon it, or whether a change in the composition of the bile must precede that separation, as alkaline urine must precede the phosphatic incrustation of foreign bodies in the urinary bladder. There are cholesteric calculi containing pins, flukes or ascarides as nuclei. I shall have an opportunity of bringing some experiments on this question before you on a subsequent occasion, as it seems to admit of a satisfactory solution. At present we must allow, that the nucleus being given, whether it be a foreign body, or casts of the biliary ducts, or amorphous precipitated colouring matter, or blood, or cast-off epithelium, or even the problematical mucus —the subsequent crystallization of cholesterine may be either a consequence of the effects of the nucleus upon the bile and gall-bladder, or a continuation of the cause which produced the nucleus. The casts may have acted as foreign bodies simply, or the liver-cells, which effused the abnormal congealing matter, may have thrown out an excess of cholesterine, or bile incapable of holding the ordinary quantity of cholesterine in solution.

As there are gall-stones which possess no tangible nucleus, but consist throughout of pure pearl-white cholesterine, the circumstances under which cholesterine may crystallize from bile, are particularly deserving of investigation. Excess of cholesterine merely, which Chevreul thought to have found, though a very likely thing to occur, is in itself not sufficient to explain its precipitation; a change or destruction of its solvent, taurocholic acid, on its passage through the ducts and bladder, seems essential. This change may be produced by specific disease of the liver-tissue, by retention of bile and subsequent endosmotic changes, or by decomposition of bile from any cause. Bile, whether stagnating in the human body, or in a glass bottle, on decomposition setting in, gives up its cholesterine in a crystallized form.

This decomposition of the solvent of cholesterine, once set up in the gall-bladder, continues, like ammoniacal urine in the urinary bladder, to contaminate all fresh portions of bile poured out into the gall-bladder, and to precipitate their cholesterine, and often their colouring matter upon the centres of attraction. In this manner are formed the layers of gall-stones, which in their eccentricity exhibit the periodical growth of the concretions. Mixed with the cholesterine layers are patches or layers of colouring matter, namely, bilifulvine and biliverdine, in various proportions and shades. Bistearate of lime in starlike crystallized small masses, and a little phosphate of lime mixed with shrivelled cylindrical epithelial cells make up the list of constituents which we observe in the greatest number of ordinary gall-stones. Concretions, consisting of pigment, or inspissated bile, or of phosphate and carbonate of lime, have a different pathological, and by their rarity, less practical significance.

In the *symptomatic treatment of the passage of gall-stones* we have to rely mainly upon opium, which is sometimes better borne in the form of pills than that of tincture. We must carefully guard the patient from an overdose of this drug, which he is even more apt to take on his own account, from the torturing pain he is suffering, than the practitioner is likely to prescribe it. Nevertheless, severe narcotism sometimes follows large doses of opium—when the pain which caused them to be given suddenly ceases, from local causes. Such a case happened in Ireland some years ago, and was the subject of a painful legal inquiry. Happily, the life of the patient, who was a member of Parliament and a large estate proprietor, ended fatally only a few days after the severe narcotic symptoms had been successfully combatted, so that the death and the narcotism stood in no immediate connection with each other. Whatever be our opinion, this case suggests caution.

Persons afflicted with dormant gall-stones should regulate their living in the most careful way possible, avoid all and every description of spirituous liquors, and let fruit and cereals, and light vegetables, enter largely upon their dietary.—*British Med. Journal, Nov.* 19, 1859, *p.* 936.

DISEASES OF THE URINARY ORGANS.

44.—OBSERVATIONS ON SOME OF THE DAILY CHANGES OF THE URINE.

By William Roberts, M.D., Physician to the Manchester Royal Infirmary.

[The opinion held previous to 1849 was, that the reaction of healthy urine was necessarily acid. At that time Dr. Bence Jones propounded the hypothesis that the stomach and kidneys were, as to the reaction of their respective fluids, in a state of antagonistic correspondence, i.e., when the gastric juice was most actively secreted, the urine was least acid, or slightly alkaline, and *vice versa*. This hypothesis, however, though apparently well supported by experiments, has not been found correct by subsequent observers, as Dr. Beneke, Vogel, and Dr. Sellers, of Edinburgh.]

It seemed, therefore, not undesirable, in the face of this contradictory testimony, to submit the question to a new investigation, and to inquire if means could not be found to reconcile the conflicting facts, and ascertain with some precision what effect food had on the reaction of the urine.

To this end the urine was examined at short intervals,—after a meal, usually every hour; and at periods more remote, every two hours. The hourly flow being thus ascertained, the absolute quantity of free acid discharged per hour was estimated by a test solution. Dr. B. Jones, in his experiments, was content to gauge the amount of acid in 1000 grains of the urine, without reference to quantity per hour; and it was therefore objected to them, that the varying concentration of the secretion, especially the great dilution which follows free potations, vitiated the conclusions deduced. The objection, however specious, lies with very little justice against these researches; for Dr. Jones was always careful to specify the density of every urine which he examined, and in this way to guard against any very great fallacy from unequal concentration. The method of hourly estimates has, however, unquestionably the merit of superior exactness; hence it was adopted in the following observations.

The mode of proceeding was to heat five hundred grains of the urine, and to add gradually a dilute solution of caustic soda to saturation. If the urine became alkaline, dilute sulphuric acid was used in the same way to ascertain the degree of alkalescence. Both test solutions were made of equal saturating powers, each 100 grain measures being equivalent to one grain of dried carbonate of soda. In this way the acidity or alkalinity per 1000 grains was ascertained; and from this, and the quantity of urine secreted during the hour, the hourly amount of free acid and free alkali discharged was computed.

The results of these experiments confirmed in the fullest manner the conclusions of Dr. Bence Jones, that a meal, be it of animal, vegetable, or mixed food, has a powerful and constant effect in lowering the acidity of the urine, frequently even rendering it alkaline.

The mean results obtained are unequivocal in their affirmation of the power of mixed food to depress the acid reaction of the urine; and the one set is remarkably in agreement with the other. The urine became alkaline for about two hours after breakfast, and for about three hours after dinner. The several days differed a good deal from each other as to the intensity of the effect. The urine did not always change from acid to alkaline after a meal, but it invariably showed a greatly lowered acidity. Some of the causes which operate to produce these inequalities will be presently adverted to.

On some additional days four meals a day were taken, according to the usual mode of life of the subject of experiment, and malt liquor was taken with dinner and supper. This did not alter essentially the results obtained; but a modification was caused by the quick succession of the meals, for it was found that the *remote* effect of one meal interfered with the *immediate* effect of a succeeding one.

Two sets of experiments were also made with a diet of purely *vegetable food*. In the first of these, the days alternated with days on which animal food was used. The experiment, too, only embraced a portion of the twenty-fours, namely, from 7 a.m. to 11 p.m. The second set embraced five complete days, of which three were consecutive; the other two succeeded to days of mixed diet, on which dinner had been taken at 2 p.m., after which no solid food was taken until breakfast next morning. It may be observed that a very considerable difference exists between the results of the two sets; —that on the days composing the first, the urine never became alkaline at all, and even the depression of the acidity was comparatively slight; whereas on the days of the second set, vegetable food showed a very powerful influence over the urinary reaction. The articles of diet used were bread, sugar, coffee or tea, without cream, for breakfast; and bread, potatoes, rice, and a vegetable soup for dinner. Occasionally carrots and celery were added.

In the last place, purely *animal food* was tried; and it was found most efficient in depressing the acid reaction of the urine.

I could not perceive any distinct difference in the effect of the diverse forms of animal diet; fish, flesh, and fowl and game appeared to act with equal power.

The conclusion, then, to which these experiments lead, is that ordinary food, whether it was purely animal, purely vegetable, or, as was more usual, an admixture of the two, invariably caused a diminution of the free acid separated by the kidneys. The conclusion is the more remarkable, as it scarcely squares with what is known of the reaction of the urine in carnivorous and herbivorous animals. In the former, the urine is described as always acid, and in the latter, as invariably alkaline; and the difference is attributed to diversity of food in the two classes. The experiments of Dr. Cl. Bernard seem conclusive on this point. He found that when rabbits (whose urine is normally alkaline) were fed for some time on an exclusively animal diet, they passed an acid urine, and that its alkalescence was not restored until a vegetable diet was substituted. Dogs also, when restricted to a vegetable fare, secreted an alkaline urine, turbid from deposition of phosphates; but when restored to animal flesh, their urine resumed its natural clearness and acid reaction. How to reconcile these contradictions, I am unable to say; but it may be pointed out as worthy of notice, that of the three *consecutive* days of exclusively animal and exclusively vegetable diet, the greatest effect in the former was on the first day, and it fell progressively on the second and third days; whereas the exact reverse took place on the days of vegetable food.

Another point was brought out with great distinctness in these observations, namely, the falling off which takes place in the hourly amount of free acid after prolonged fasting, and the still greater falling off in the hours of sleep. But this second decline has a totally different origin and significance from the depression that follows a meal. The latter is a true *alkaline tide*, and if it but flows strongly enough, the urine altogether loses its acid reaction and becomes alkaline; but in the former case the intensification of the phenomenon has no tendency to change the reaction of the urine, because it depends simply on the diminished secretion of the urine, which, as we have already seen, invariably accompanies the state of fasting and sleep. That this is the true interpretation of the fact, may be seen by comparing the degree of acidity per 1000 grains of liquid urine; or, still more exactly, by comparing the amount of free acid corresponding to 100 grains of solid urine with the amount separated per hour. It is seen that the *degree of acidity* of the urine steadily rises, after recovery from the depression of the alkaline tide, until the next meal.

I was at first led into error by studying the oscillations of the free acid too exclusively from the hourly point of view. It appeared as if an *acid tide* followed immediately on the ebb of the *alkaline tide*; for the hourly secretion of acid was uniformly found at its highest shortly after the urine had recovered its acidity; but this was entirely due to the fact, that the increased activity of the kidneys, called forth by the meal, persisted for two or three hours after the normal acidity of the urine had been restored. There seems, however, a limit to the increase of acidity in prolonged fasting; for Dr. B. Jones found, that continuing to fast for twelve hours beyond the usual time of taking food did not intensify the acidity of the urine.

There is evidently the closest relation between the reaction of the urine and that of the blood. By adding to the alkalescence of the blood through artificial means, as by exhibiting caustic or carbonated alkalies, we are able to depress in a corresponding portion the acidity of the urine. On the other hand, also, by exhibiting acid (although this seems less readily accomplished), we can similarly heighten the reaction. We may, therefore, look on the urine as a measure of the state of the blood. When the urine is highly acid, the alkalescence of the blood is below par; when on the contrary, the urine is alkaline, it is an indication that the alkalescence of the blood is above par. It follows, therefore, that to the kidneys is delegated the important duty of regulating the reaction of the blood. When the blood tends to become too feebly alkaline, the kidneys separate acid largely; when, on the contrary, its alkalinity mounts beyond the physiological requirement, the same organs secrete a feebly acid, or even an alkaline urine.

The alkaline tide—and in this term I would include the whole period of depressed acidity, and not merely the time during which the urine is actually alkaline—was found to set in earlier after breakfast than after dinner, and its duration was more brief. After breakfast the acidity showed nearly always a sensible declension in forty minutes. The urine, however, never became alkaline, nor even neutral, so soon. During the second hour after breakfast (from nine to ten) the alkaline tide usually culminated; but in about a third of the cases the point of least acidity was not reached until eleven o'clock. Then the tide turned; and from eleven to twelve the urine was found fast recovering its reaction, and about one the normal level was usually attained. Thus, for about four hours the morning meal exercised a depressing effect on the acidity of the urine; but it was not actually alkaline usually for more than an hour, sometimes for two hours, and once for three hours.

The effect of dinner was not perceptible until the second hour after the meal. During the next three hours (third, fourth, and fifth hours) the alkaline tide ran in its greatest strength. On the third and fourth hours the urine was always (with two exceptions) found alkaline when the meal had been of mixed food or animal diet. At the end of the sixth hour the tide had generally turned, and the acid reaction been restored. Three hours was the usual duration of the alkalescent state after dinner; sometimes two hours, more rarely four hours, and on one occasion five hours. The amount of free alkali discharged after dinner was, generally, not far from double the quantity observed after breakfast; so that, in duration and intensity, the effect of dinner proved about twice as great as that of breakfast. The difference arose, probably, simply from the fact that breakfast was a much lighter meal than dinner, and its impression on the system consequently smaller.

The alkaline urine that was passed after food owed its reaction to a fixed alkali, and not to ammonia. It did not effervesce with acids. It was rich in earthy and alkaline phosphates; and on these latter, in a basic state, depended apparently its alkaline reaction. As might have been anticipated, the loss of acidity entailed the precipitation of the earthy phosphates; and the urine, when passed, was frequently turbid. But this was not always so. Not unfrequently, especially after breakfast, the urine, although alkaline, retained its transparency. Generally, such a urine was of feeble alkalinity and dilute; but now and then it was observed to be tolerably concentrated, highly alkaline, and still clear. All transparent alkaline urines were rendered immediately turbid by caustic ammonia, and by mere heating; so that the transparency did not depend on the total absence of earthy phosphates. It was also found, that in the turbid urines, after subsidence of their deposits, caustic ammonia caused an additional precipitation.

The degree of turbidity varied from a barely perceptible cloudiness to a thick muddy opacity. The deposit subsided quickly, and left a clear, yellowish-amber supernatant liquor, often with a greenish tinge.

The odour of this alkaline urine resembled that of the fresh urine of the horse. It had lost the charateristic urinous odour, and exhaled a strong sweetish aroma, so peculiar as to indicate with certainty the change of reaction without the aid of test-paper.

The urine of the alkaline tide was rich in uric acid (as caculated per 1000); rather more so even than the night urine, which nearly always let fall a copious urate deposit on cooling. The hourly quantity was almost three times greater than during the succeeding period when the acidity was restored (from nine to eleven), and more than three times greater than during sleep. The differences are not so great when the uric is reckoned as a percentage on the solid residue; although here also the alkaline urine shows notably the highest proportion.

The remarkable poverty of the urine in uric acid from nine to eleven, was probably a direct result of the excessive separation during the preceding hours of the alkaline tide; the blood having been thereby left poor in uric acid, and therefore unable to supply the kidneys with the usual quantity.

Although, as we have seen, the immediate effect of a meal was to depress the acidity of the urine, the *more remote consequence* was to uphold, and even to increase the acidity. This was seen most distinctly when comparison was made of the acidity of the urine on mornings succeeding supperless nights, and mornings following a hearty supper. In the former case, the mean hourly rate of acid discharged between 7 and 8 a.m. was only 0.51; in the latter, it was 0.88, or nearly double. And not only was the hourly discharge thus increased, but even the degree of acidity per 1000 showed a distinct exaltation,—the mean numbers being, for the mornings after supperless nights, 1.83, and for mornings after a supper, 2.15. This latter reckoning, however, did not always exhibit results in accordance with this general mean, and exceptions occurred; but for the *hourly* reckoning all the separate results were consistent, sometimes in a greater, sometimes in a less degree, with the general mean.

The remote effect of animal diet appeared considerably greater than that of vegetable food. So that a highly animalized diet would seem to tend in the long run to heighten the acidity of the urine; a conclusion quite in harmony with ancient belief. Here, no doubt, lies the explanation of the otherwise puzzling difference between the effect of vegetable food in the first and in the second set of experiments on that diet. In the first set, the urine showed great unsusceptibility, not once losing its acid reaction, nor even reaching the neutral line; whereas, in the second set, it became alkaline, and even strongly alkaline, on several occasions. In the first, a diet rich in animal flesh had been used on the alternate days; and the remote effect of this evidently overpowered the immediate effect, that would otherwise have shown itself more prominently. This circumstance is of importance to bear in mind when the effect of food on the reaction of the urine is to be traced. A heavy supper the night before, especially a meat supper, will greatly diminish the effect of breakfast the next morning. For this reason, it is well, in order to obtain distinct results, to fast for eight or ten hours or more if night intervene, before taking the meal whose effect it is wished to observe.

It was a marked feature in these experiments, that although there was the greatest constancy and certainty in the successive changes of reaction, there were very considerable, indeed, the widest, differences in the *absolute amounts* of free acid or free alkali separated by the kidneys at corresponding hours after a meal on different days. Whence these differences? This question may be conveniently considered in connection with another. Is the action of food, in lowering the acidity of the urine, a universal phenomenon? It is impossible to give a complete answer to this question until observations on separate individuals have been greatly multiplied; but the facts already known warrant the statement, that at least some healthy persons, whose urine otherwise appears perfectly normal, suffer constantly a diminished acidity of urine after meals, and often emit a urine which is alkaline and turbid, for four and six hours daily, without the least evidence of impaired function.

I am disposed to believe that the depression of the acidity of the urine after a meal is of universal occurrence; but at the same time observation has convinced me that there are very great differences in the degree or intensity of the phenomenon in different individuals. The source of these differences may be looked for in certain fixed peculiarities of constitution; and among these, the following may be indicated as probably operative to that effect:—

1. *Differences in the Amount of the Insensible Perspiration.*—As the sweat is always acid, individuals with a moist active skin would separate through this channel a large quantity of acid. and consequently leave less for the kidneys to remove. In such persons, it might be expected that the alkaline tide would present more prominence than where the cutaneous surface was habitually dry and hard. Whether the sweat, under any circumstances, loses its acid character or not, I am unable to say; but I have invariably found it sour, even when the urine has been kept continuously alkaline by bicarbonate of potash for several weeks successively.

2. *The exaltation which takes place in the respiratory changes after a meal.*—The effect of an increased absorption of oxygen would be to increase the

formation of acid. Those persons, therefore, in whom this exaltation takes place promptly after a meal, and is unusually great, would probably show but a feeble alkaline tide, because the acid-producing power of this excess of oxygen would counteract the opposing tendency of the meal.

3. *A power of quick digestion and absorption* would probably enhance the effect of a meal, inasmuch as the more suddenly a meal is thrown into the channels of the circulation, the greater the disturbance it would temporarily cause.

4. I am also of opinion that an overworked or "below par" state of system is promotive of a diminished acidity of the urine; and that a high state of health and vigour tends to a heightened acidity.

In addition to these modifying conditions, which apply specially to different individuals, but also in a less degree to the same individual at different times, there are other circumstances which have a stronger bearing in the latter case. Of these, the most powerfully operative is the remote effect of a previous meal, which has already been fully considered. There is also the condition of exercise or rest. It is thought that, in a state of muscular activity, there is an increased production of the acid muscular juice; and this transuding into the blood, and passing through the kidneys, would as Vogel surmises, probably intensify the acid reaction of the urine. And lastly, the richness or poverty of a meal in basic phosphates may, almost with certainty, be named as having great influence on the strength of the alkaline tide.

Some of these considerations have scarcely an intelligible force until the next inquiry is answered:—*Why should a meal diminish the acidity of the urine?* And to the solving of this question our attention must now be turned.

Dr. Bence Jones' view has already been explained. The acid generated in the blood has become diverted to the stomach to digest the food; hence there is less—or none at all—to pass away by the urine. There are some difficulties, however, in the way of adopting this as the *sole* explanation of the diminished acidity of the urine after food; and another theory is here offered, based on totally different grounds, which appears to square better with the facts ascertained.

It seems from the observations of Dr. Beaumont on Alexis St. Martin, that the gastric juice required to digest a meal is poured into the stomach with great celerity; so that in twenty minutes, half an hour, or an hour at most, the whole quantity demanded is secreted, and no further flow takes place. Absorption then commences, and the acid begins to return with the chyle back again into the blood. If this be so, the reaction of the urine ought, on Dr. Jones' hypothesis, to reach the point of greatest depression in an hour or an hour and a half after a meal, instead of from two to five hours after. Moreover, how may it be explained, on this supposition, that the alkaline tide after dinner sets—in an hour or an hour and a half later than after breakfast? Besides all this, the acid of the gastric juice and that of the urine are certainly not identical, nor apparently even similar. These objections, however, although not without considerable force, would not suffice to upset a theory so plausible, were there not distinct reasons, and, as I believe, sufficient ones, to adopt an altogether different explanation: which I will at once proceed tolay before the reader.

We have seen that the alkaline tide coincided, in point of time, with the increase in the secretion of solid urine; may not the alkaline tide, therefore, be considered concomitant with the absorption of the meal into the blood, rather than with its digestion? But how can the passage of a meal into the blood affect the reaction of the urine? *By raising temporarily the alkalinity of the blood through the means of its mineral constituents.* The blood of all animals is alkaline; and Liebig informs us, that this arises from the fact, that "all articles of food which alone, as bread and flesh, or when mixed with vegetables, are capable of sustaining the process of sanguification and nutrition, contain carbonic acid, or phosphoric acid, and alkalies,—the two latter in such proportion, that if we suppose them dissolved, the alkalies invariably predominate." Every meal, therefore, that is dissolved and absorbed into the blood, increases the alkaline reaction of that fluid, and raises it for a time above the natural level. But, as already explained, changes in the reaction of the blood are immediately reflected in the urine. A basic alkaline phosphate or a carbonate artificially introduced into the blood, inevitably lowers the acidity of the urine, and, if in sufficient dose, renders it alkaline. What then is to hinder the carbonates and basic phosphates of our food from acting similarly? A meal, consequently, may be regarded from this point of view as a dose of alkali, which first exalts the alkalinity of the blood, and as a more remote, but equally certain consequence, diminishes the acidity of the urine.

In harmony with this explanation is the fact, already pointed out, that the increase of the solid urine (which is an index of food passing into the blood) takes place simultaneously with the declension of the urinary acid. It is true that the subsidence of the alkaline tide is not synchronous with the completion of absorption; for the former begins to ebb while absorption (to judge by the quantity of solid urine) is still proceeding actively. This want of coincidence appears *primâ facie* to militate against the solution here offered; but it may be explained in two ways. Either it arises from the saline constituents being absorbed with more celerity than the rest of the food; or, more probably, it depends on the increased absorption of oxygen by respiration, already noticed as occurring after a meal, which, after the lapse of five or six hours, by generating acid, counteracts the contrary effect of the food —in other words, from the remote effect of a meal overlapping the immediate effect.

When food devoid of mineral constituents was used, there was no lowering of the acidity of the urine. On two days sugar or honey was taken instead of ordinary food, and the acidity of the urine maintained itself almost unchanged, with a tendency to elevation rather than depression.

It must not be supposed, however, that the urine is never alkaline from fixed alkali, except as a consequence of taking food. Clinically, the urine is observed not very unfrequently in this condition. Dr. B. Jones has related such cases; and I have once and again met with them myself. In persons of a debilitated constitution, especially in the anæmic state which sometimes follows obstinate subacute rheumatism or gout, and in chlorotic females, I have seen the urine alkaline for a day or two, during the time of fasting. Next day, however, the urine would return to its natural state; reverting again, perhaps in the course of a week or ten days, to the alkaline state.

According to Homolle and Duriau, the urine becomes alkaline by immersion of the body in a bath. It is even stated that the addition of nitric acid to the water does not prevent the urine of the bather from losing its acidity.

Finally, let us offer some considerations which may explain why the alkaline urine of food is not oftener met with in actual experience; and why there should exist such important discrepancies between the different observations made in relation to it.

The great source of fallacy is neglecting to isolate the product of the alkaline tide from the acid urine secreted before and after its flow. To do this, it is necessary to empty the bladder at the commencement and at the close of the period during which the urine is alkaline; and from the uncertainty of the time after a meal at which the point of greatest depression is reached, this can only be accomplished by examining the urine at hourly intervals. This is the reason I apprehend, why the observations of Beneke and Vogel, and probably also those of Dr. Sellers, are at variance with those of Dr. Jones and myself.

In Dr. Beneke's experiments, the bladder was not emptied oftener than about five times a day; and by comparing the times of the meals with the times of micturition, it is evident that it was impossible for him to obtain results other than nugatory. The urines which he examined were mixed urines, and he did not in any wise isolate the secretion at the critical periods.

The same objection appears to lie even more strongly against the observations made under the supervision of Vogel. The urines were collected for three periods—namely, between breakfast and dinner (morning urine); between dinner and evening (afternoon urine); and during the hours of the night. All such urines would be acid mixtures; but it by no means follows that they did not mask an alkaline state, even it may have been, of some hours' duration.

Nothing could more strongly prove the importance of frequent examination of the urine, than the fact that the subject of these experiments was not in the least suspected to void at any time an alkaline urine until the urinary product was, as it were, analyzed by hourly examinations; and this although the urine had been under observation for years. And it took me by surprise to find the phenomena of the alkaline tide so strongly and so remarkably pronounced, after such long and effectual concealment.

5.—*The Appearance of the Urine.*—Dr. Prout tells us, "that in a perfectly healthy condition of the urine, this secretion not only remains transparent after cooling, but continues transparent until it begins to suffer those spontaneous changes incidental to all fluids containing organized matters in solution." This is a much too absolute statement even for the urine of ordinary micturition; and is certainly quite erroneous in relation to the urine as it leaves the kidneys.

The highly acid and concentrated secretion which is separated after six or eight hours' fasting, may be *expected* to let fall a deposit of amorphous urate on standing awhile. I found the urine secreted between seven and eight in the morning (before breakfast), *nearly invariably*, to become sedimentary on standing a few hours; and usually even on mere cooling. I cannot, therefore, subscribe to the doctrine that the occurrence of a urate deposit *necessarily* indicates some departure from the healthy state. A low temperature, with a concentrated and highly acid state of the urine, are of themselves sufficient to throw down the urates. I have never witnessed a urine turbid from urates *when passed*. Dr. Prout states, that in gouty persons he has observed such urine; at the same time, he signalizes the extreme rarity of such a condition. Dr. Thudichum relates a most singular example. A child suffering from fever had retention of urine: and when the urine was withdrawn by the catheter, it was found loaded with a lumpy deposit of urate of soda.

Like Dr. B. Jones, I found that the occurrence of a urate deposit on cooling was by no means a sign of excessive discharge of uric acid; on the contrary, the urine of the alkaline tide (which never deposited urates) was found to be in every respect richer in uric acid.

An alkaline urine depositing earthy phosphates is always turbid *when passed;* cooling does not increase the sediment; indeed, I have thought that a very slight cloudiness from this cause has disappeared on cooling, and left the urine transparent. At first sight, it might seem that the presence of urine containing suspended flakes of earthy phosphates in the urinary passages, would lead inevitably to the formation of concretions either in the renal tubuli or in the bladder; but it is notorious that the primary formation of a spontaneously-arising urinary concretion never is phosphatic, but nearly always composed of uric acid or oxalate of lime. And I am disposed to believe, rather, that the occurrence of an alkaline tide after meals is an admirable provision of nature for diminishing the risk of calculous formation.

After long fasting, and during the night, the secretion that flows through the kidneys is very acid and very concentrated—in a condition most favourable to the precipitation of crystals of uric acid or oxalate of lime; but if any such fall, they are dissolved and washed away by the alkaline tide after breakfast or dinner. The earthy phosphate deposit has itself no tendency to aggregate in masses, nor to adhere to any foreign body. When, however, ammonia begins to be developed in the urine, crystals are formed, and the deposit shows strong tendency to run into masses or small concretions. This deposit, therefore, as long as it is free from ammonia—and it is free from ammonia, as it appears, in the alkaline tide after meals—can safely pass through the urinary passages without fear of its forming a calculus.

The colour of the urine was found to vary chiefly in degree; and, for the most part, simply from greater or less concentration. The night urine, however, was always noted as having a reddish tinge, and the urine of the alkaline tide had often an olive cast.

Healthy urines seem divisible into two distinct classes,—1. *Urines of Fasting (urinæ sanguinis)*, and 2. *Urines of Food (urinæ cibi)*. The former are highly acid, inclined to deposit urates on cooling, but always clear on passing; apt, also, to be high coloured, especially during sleep. Estimated according to the amount of solid constituents discharged per hour, they are scanty. The urines of food fall into two well-marked varieties—those with a *diminished acidity*, and those with a *restored acidity*. Both varieties are of weak acidity, and abundant in quantity. They show no tendency to deposit urates on cooling, although they are rich in uric acid. They are rich in earthy and alkaline phosphates also; and the alkaline variety is usually turbid when passed.

Either class may be watery or concentrated, according to the relation between potation and the requirements of the system as respects water. The *urinæ potus*, therefore, do not merit to be regarded as a distinct class.

Very frequently the urine filtering through the kidneys is of a transitional character, as urine of one class or variety passes into another. The urine of micturition almost always belongs exclusively to no division, but is a mixture of several kinds; hence it scarcely ever presents, in well-marked degree, the special features of the urine of fasting or of the urine of food. Perhaps, if this classification of urines were kept more in view, the method of estimating the solid constituents from the density would yield more accurate results.—*Edinburgh Med. Journal, April 1860, p. 906.*

45.—ON URIC ACID AND URATES.

By Lionel Beale, M.B., F.R.S., Physician to King's College Hospital.

Uric Acid and Urates are present in certain proportion in healthy urine but in disease a large increase is very frequently observed. The substances occur as urinary deposits, either from existing in too large a proportion to be dissolved in the urine when cold, or, as is probably the case in the majority of instances, from the development of an acid in the urine, which causes them to be precipitated from their solutions. In many acute febrile diseases, the proportion of uric acid is increased, and the period of resolution of inflammation is marked by diminished frequency of the pulse and respiration, by a fall in the temperature, by free perspiration, and by a very abundant deposit of urates. In health, from five to eight grains of uric acid are excreted in twenty-four hours; but, in some acute diseases, the proportion may amount to twenty grains. In a case of fever, Dr. Parkes found that 17·28 grains of uric acid were excreted in twenty-four hours. Urate of soda is very readily caused to deposit crystals of uric acid. If the amorphous deposit be merely dissolved by warming the urine, the urate often becomes decomposed; and, as the solution cools, crystals of uric acid are deposited. In some cases, the quantity of uric acid held in solution is so great that, upon the addition of a drop of nitric acid to the urine, an abundant amorphous precipitate, exactly resembling albumen, is formed. Such a precipitate has many times been mistaken for albumen, and even if examined under the microscope immediately after it is formed, its nature cannot be made out; but if it be allowed to stand for some time, the amorphous particles gradually increase in size, and assume the well-known crystalline forms of uric acid. The instances in which I have met with urine exhibiting these characters, have almost all been cases of liver-disease. Although the reaction is acid, no precipitate takes place upon the application of heat, which at once distinguishes urine of this character from albuminous urine.

The presence of an abnormal quantity of uric acid in the urine shows that more of this substance or its salts is formed in the blood than in health. It would appear that, in consequence of certain conditions, a large proportion of the uric acid resulting from the disintegration of albuminous substances is not further oxidised and converted into urea, but combines with ammonia, or lime, forming urates of these bases. In gout, the presence of uric acid has been detected in the blood by Dr. Garrod. *During* the attack there is less in the urine than in health; but *after* it is over, a large quantity of uric acid and urates are often carried off from the system in the urine.

In cases characterized by a tendency to the formation of much uric acid, the principal objects to be obtained by treatment are, to favour the further oxidation of the uric acid formed, and to promote its solution and elimination from the blood as rapidly as possible. Good air, and moderate exercise, with attention to the action of the skin, will fulfil the first object; and the solution and elimination of the urates will be encouraged by giving alkalies in solution in a considerable quantity of water.

The satisfactory change which in chronic gouty and rheumatic cases, frequently ensues from following some of the much vaunted "systems," or going through a course of bathing in Germany, or elsewhere, obviously arises from the increased action of the skin, and the improvement of the health generally, effected by the exercise, good air, simple diet and temperance, wisely enforced in the establishments. If patients could be induced to retire to a pleasant part of the country, where they could take moderate exercise and be free from mental anxiety, meet with agreeable society, live regularly, take small doses of alkalies, and soak themselves for an hour or two a day in warm water in which some carbonate of soda had been dissolved, they would receive as great benefit as by travelling hundreds of miles away, and at much less trouble and expense. I am convinced that there are many patients who would prefer to carry out such a simple plan, rather than submit themselves to all the useless routine and absurd formalities involved in many of the professed universal systems, such as homœpathy, hydropathy, which cannot but be extremely offensive to their common sense, while they are claimed as converts and supporters of doctrines which they do not really believe in. There are many who, for the sake of the advantage they derive from the regular system of living, air, exercise, &c., express no disbelief in doctrines and propositions which they probably feel to be absurd, and which a little reflection must prove to be false.

In all such cases, the nature of the derangement of the physiological processes should be carefully considered before any plan of treatment is adopted. We must ascertain in what points the condition differs from a healthy state, and then consider how the deranged actions may be restored. In such cases, it is obviously quite useless to attempt to relieve the patient by giving drugs, without enforcing attention to all the circumstances which are likely to improve the health. Neither will it be wise to attempt to treat the case as if the presence of the uric acid deposit were the most important symptom. The physiological changes taking place in the organism generally must be attended to, and the patient must be instructed to employ those means which are likely to restore healthy action.—*British Med. Journal, Oct. 22, 1859, p. 850.*

46.—ON THE CHLORIDE OF SODIUM OF THE URINE.

By Lionel Beale, M.B., F.R.S., Physician to King's College Hospital.

[The quantity of the inorganic salts in the urine varies greatly in disease. Deficiency may depend on a deficiency in the food. Excess in such as are formed in the organism by the disintegration of the organic tissues (as sulphates and phosphates) may depend on increased disintegration of these tissues.]

The fluctuations observed in the quantity of common salt excreted in the urine are very great even in health. The circumstances which affect the proportion of chloride of sodium are very numerous, and these are greatly increased in disease. It was found by Redtenbacher, many years since, that in pneumonia the quantity of chloride in the urine gradually decreased as the inflammation advanced; and that in many instances, when the lung became hepatised, not a trace could be detected in the urine. Some years ago, I determined quantitatively the amount of chloride in the urine from day to day in acute pneumonia, and the following case illustrates very well the changes which occur in the urine in this affection.

The patient was a plasterer, aged 24, and was under the care of Dr. Budd. On the third day of the disease, there was dulness two inches below the left mamma in front and behind over the space below the spine of the scapula. Bronchial breathing and bronchophony were audible over the lower angle of the scapula. Expectoration viscid, frothy, and slightly rusty; pulse 144, small and weak; respiration, 52.

On the fourth day of the disease, bronchial breathing and bronchophony were more distinct. Pulse, 116; respiration, 28. He was treated with small doses of antimonial wine, and was put upon milk diet and beef-tea. Turpentine stupes were applied to the chest. He progressed favourably, was convalescent within three weeks after the commencement of the attack, and was discharged well in little more than four weeks.

13. *Fourth day* of the disease. Urine high coloured; acid; specific gravity, 1017; contained a little albumen.

14. *Fifth day.* Acid; specific gravity, 1013; natural colour.

15. *Sixth day.* Acid; specific gravity, 1016; pale; still continued a trace of albumen.

16. *Tenth day.* Acid; specific gravity, 1022; no albumen.

17. *Twenty-second day.* Acid; specific gravity. 1016; pale; no albumen.

Analysis.

	13		14	15	
Water	956·60		957·40	954·00	
Solid matter	43·40	100·00	42·60	46·00	100·00
Organic matter	40·28	92·82		44·64	97·05
Fixed salts	3·12	7·18		1·36	2·95
Chloride of sodium	0		traces	0	

	16		17	
Water	955·00		968·40	
Solid matter	45·00	100·00	31·60	100·00
Organic matter	42·12	93·64	23·26	73·61
Fixed salts	2·88	6·4	8·34	26·39
Chloride of sodium	0		4·56	14·43

The decrease of the fixed salts generally, during the stage of hepatisation is remarkable. The last analysis of the urine, when the patient was well, shows the healthy proportion. In vol. xxxv. of the 'Medico-Chirurgical Transactions' will be found several other cases showing similar results. In some of the cases, it was shown that, although there was not a trace of chloride in the urine, and the blood contained less than its normal proportion, the sputa were very rich in chloride of sodium. In one case, it amounted to upwards of 18 grains in 100 in the solid matter of the sputum. In a fatal case, much chloride was found in the products effused into the air-cells of the lung. In most exudations, and in growing tissues, there is a considerable amount of sodium. In acute inflammations generally, the proportion of chloride in the urine gradually diminishes until the disease is at its height. When resolution occurs, the chloride reappears, and gradually increases as convalescence advances, until it attains its normal standard. The amount of chloride in the urine is much influenced by the nature of the food, and by the quantity of fluid taken, as remarked when considering the chloride in healthy urine; but the results above described cannot be explained in this manner; for although patients take less food when they are ill, and therefore less salt, the same results are observed if salt be given to them. Moreover, the disappearance is gradual, and the reappearance is marked by a change in the symptoms of the disease, although the food has remained the same during the whole period of the illness. I have expressed the opinion that, in all probability, the chloride is attracted from the blood in undue proportion to the point where the inflammatory changes are taking place; and that, instead of passing through the organism as it does in health, it accumulates at this point until a certain stage of the morbid process is passed, when it is reabsorbed into the blood, and excreted by the ordinary channel. The precise office which the salt plays in these processes is not understood; but certainly, in all the specimens of inflammatory lymph that I have examined, I have always found common salt plenty in large quantity. In many cases of bronchitis, acute rheumatism, pleurisy, in some cases of skin-disease, and in some other instances in which its absence would appear to be merely an accidental circumstance, no salt can be detected in the urine. We cannot, therefore, regard this diminished proportion or absence of chloride in the urine as a point of any value in the diagnosis of pneumonia, although it must be looked upon as a fact of great interest with reference to the morbid changes which are taking place at the time. The conclusions to which I arrived, after examining the urine, blood, sputum, and inflammatory products, in several cases of pneumonia, are as follows:

1. That in pneumonia there is a total absence of chloride of sodium from the urine at or about the period of hepatisation of the lung.

2. That, soon after resolution of the inflammation, the chloride is again present in the urine, and often in considerable quantity.

3. That, at this period (resolution), the serum of the blood is found to contain a greater amount of chloride than in health.

4. That the presence of chloride of sodium in the urine may be taken as evidence of the existence of a greater quantity of the salt in the blood than is required for the wants of the system generally, or, at least of an amount sufficient for that purpose, and that the absence of the salt from the urine indicates that the circulating fluid contains less than the normal quantity.

5. That the sputa in pneumonia contain a greater quantity of fixed chloride than healthy pulmonary mucus, if there be not much less than a normal amount in the blood, although there be a complete absence of the salt from the urine. In all cases, however, there is found in the sputa a quantity many times greater than exists in an equal amount of blood at the same period of the disease. The absolute amount present is subject to variation at different periods of the disease, and in different cases.

6. That, in one case which was fatal, the proportion of chloride present in the sputum underwent a decrease, while the amount of solid matter, and especially the attractive matters, increased in quantity. At the same time the sputum became acid; and in the matters expectorated within the last few hours of the patient's life, a large quantity of grape-sugar was found; but, in that obtained on the day previous to his death, none could be detected.

7. The absence of chloride of sodium from the urine during the stage of hepatisation seems to depend upon a determination of this salt to the inflamed lung; and, when resolution occurs, this force of attraction ceases, and whatever salt has been retained in the lung is reabsorbed, and appears in the urine as usual.—*Brit. Med. Journal, Nov.* 19, 1859, *p.* 941.

47.—ON THE PHOSPHATES OF THE URINE.

By Lionel Beale, M.B., F.R.S., Physician to King's College Hospital.

Alkaline Phosphates.—Much has already been said upon the quantity and origin of the alkaline phosphate in the urine; and I have brought forward evidence to show that the greater part of the phosphoric acid eliminated is carried into the organism in the food. A certain proportion, however, there can be little doubt, is formed in the body by the oxidation of the phosphorous of albuminous textures (nervous tissue). In diseases generally, the alterations which have been observed in the quantity of phosphate removed in the urine is to be attributed, to some extent, to the altered diet of the patient. It is reasonable to suppose that, in some conditions of the system in which a more than usual disintegration of tissues rich in phosphorous takes place, more phosphoric acid is formed in the organism than in health. This excess should be found in the urine in the form of alkaline phosphate, and the amount ought to correspond to the activity of the changes taking place. By ascertaining the proportion, we should be able to form an estimate of the quantity of phosphorus oxidised, and therefore of the nerve-tissue disintegrated, of which it was a component part. The really difficult part of the inquiry is to ascertain how much of the total proportion of phosphate present is derived from the food, and how much is actually formed in the organism. The sulphuric acid is almost entirely produced in the body; and there is not, therefore, the same difficulty in estimating the amount of sulphur oxidised, as there is in the case of the phosphorus.

Of late, the importance of this subject has been much increased by attempts to advance the experimental results already obtained in favour of the hypothesis, that the amount of phosphate excreted in the urine is to be regarded as an index of the activity of the nervous system. Those who labour to prove that all the changes in the body are the direct result of certain chemical decompositions, have not hesitated to bring forward these results in favour of their theory. It seems by some to have been regarded as a settled point, that the quantity of phosphate in the urine varies according to the amount of nervous tissue disintegrated; and it has been assumed that the quantity of work done by the brain is in direct proportion to the activity of the chemical changes going on in the nervous tissue. This question is obviously a most important one, and much more is involved in it than at first appears. I propose, therefore, to mention a few of the facts which have been ascertained; and I think we shall find that, in this matter, speculation has taken the place of reasoning founded upon facts and experimental observations.

Dr. Bence Jones, as is well known, has written several important papers upon this subject. The general conclusions to which he has arrived are the following:—

"In delirium tremens, and in other delirium, a remarkable increase in the amount of sulphates in the urine was frequently observed; and the total phosphates were in the same cases occasionally remarkably diminished.

"In acute inflammatory diseases of the nervous structures, during the most febrile symptoms, an increase was observed in the amount of sulphates in the urine; and the total amount of earthy and alkaline phosphates in these diseases appeared to be increased in the same proportion as the sulphates were increased." ('Phil. Trans.,' 1850, p. 66.)

"In fractures of the skull, the phosphatic salts increase only when any inflammatory action occurs in the brain; and in acute phrenitis, an excessive increase takes place. In delirium tremens, there is a marked deficiency of phosphates, unless they are introduced with the ingesta; an excess is, however, met with in some functional affection of the brain."

These conclusions are founded upon analyses of 1000 grains of urine, in eleven cases of delirium tremens and eight cases of acute inflammatory affections of the nervous centres.

[Some time since the author examined the urine of many patients in St. Luke's Hospital for Dr. Sutherland, with the view of ascertaining the proportion of alkaline phosphate excreted in different cases of mania, dementia, and paralysis of the insane, &c.]

The conclusions at which Dr. Sutherland arrives are the following:—

1. A plus quantity of phosphates exists in the urine in the paroxysms of acute mania.

2. A minus quantity exists in the stage of exhaustion in mania, in acute dementia, and in the third stage of paralysis of the insane.

3. The plus and minus quantities of phosphates in the urine correspond with the quantitative analysis of the brain and of the blood; for a plus quan-

tity of phosphorus is found in the brain, and a slight excess of albumen in the blood of maniacal patients; and a minus quantity of phosphorus and albumen are found in the brains of idiots, and a minus quantity of albumen in paralysis of the insane.

4. The plus quantity of phosphates in the urine of cases of acute mania denotes the expenditure of nervous force, and is not a proof of the existence of acute inflammation in this disease.

[The author concludes this part of the subject by saying that, though not denying that increased nervous action may be associated with the formation of an increased quantity of phosphoric acid, which is eliminated in the urine, yet the facts hitherto advanced in favour of this view are by no means conclusive.]

Earthy Phosphates.—The proportion of earthy phosphates does not seem to vary much in disease. Dr. Bence Jones has shown that, in cases in which the alkaline phosphates are increased, there is no corresponding increase in the proportion of earthy phosphates. Much of the earthy phosphate eliminated in the urine in health is doubtless derived from the food, but a certain proportion is set free in the disintegration of the tissues, especially the osseous tissues. An increase of earthy phosphate is observed in the urine in some very rare cases of disease, in which the earthy matter of the bones is absorbed (*mollities ossium*). In one acute case of this disease, Dr. Bence Jones ('Phil. Trans.,' 1848) obtained indistinct evidence of the presence of chlorine, and suggests that in future this substance should be searched for, as it may possibly be directly concerned in the removal of the earthy material from bones.—*British Med. Journal, Nov.* 26, 1859, *p.* 962.

48.—CASE OF BRIGHT'S DISEASE.

In which permanent recovery took place, the results being verified by dissection, the patient dying of obstruction of the bowels eight years afterwards.

By Robert Scott Orr, M.D., Dispensary Physician to the Glasgow Royal Infirmary.

[The patient was a gardner, 57 years of age, thin, active, and of temperate habits. His illness commenced slowly and insidiously, with dropsy, but he rapidly grew worse, and presented the following symptoms :]

His face and eyelids were swollen and puffy. His lower limbs were hard and œdematous, and so great was the anasarca, that his thighs were swollen to the size of the body of a man of average bulk. His urine was frequently examined, and found to be highly coagulable, and of low specific gravity.

[He was seen also by Drs. James Watson and John Couper, of Glasgow, and his case considered almost hopeless. The treatment adopted]

Was the administration of mild diuretics, such as the acetate of squill and sweet spirit of nitre, the bitartrate of potass being given freely as a diet drink. He also had five grains of Dover's powder three times a-day, and an occasional warm bath. Latterly, the tincture of muriate of iron, with tonics, was found very beneficial.

[Under this treatment, in about three months, the graver symptoms subsided very gradually, and after a time the health was slowly restored, and recovery became complete and permanent. This patient lived for eight years longer in perfect health, and ultimately died from an intestinal affection. At the autopsy it was found that]

The right kidney was small, and in some parts deeply notched, its envelope being puckered in many places. The cortical part of it, on a section being made, was found very much narrowed and thinned. The tubular tufts were, some of them, very healthy-looking; others were much encroached upon by granular matter. The granules appeared to be much less in size and more compressed than as seen in ordinary cases of granular kidney, and as if a process of atrophy had been going on in them. Indeed, the whole kidney appeared to have become atrophied, although in this man's body all the viscera were found unusually small.

The left kidney could nowhere be found in its usual situation, but after diligent search was discovered lying on the sacrum, exactly in the fork formed by the bifurcation of the aorta. It was very much smaller than the right, having undergone much atrophy; it was nearly circular in form, presented the same notched and puckered appearance externally as the right, but contained more granular matter, of the same atrophied, compressed character as in the other. Both kidneys conveyed to my mind quite the impression that they had undergone a process of repair.

The liver and spleen were both much smaller than usual, but healthy in structure.

In the same year that I treated this person for albuminuria, apparently in its advanced stage, with the result above related, Professor Christison delivered two clinical lectures at Edinburgh on Bright's Disease, in which, with his usual clearness and perspicuity, he gave a very complete general sketch of what was then known regarding the disease. Under the head of "Treatment," he alludes specially, and at some length, to the curability of the malady. He says, "Bright's disease was long considered a very deadly one, indeed, as generally fatal sooner or later. This denunciation is probably true of the stearotic form. But it is quite incorrect if extended to the more frequent form of inflammatory desquamation." He then gives a selection of cures, where he had the rare advantage of observing the results for a very considerable term of years after the first apparent recovery. To those who maintain that there is no such thing as Bright's disease without dropsy, and who will, of course, deny that some of the cases he quotes were really instances of that disease, but only simple chronic nephritis, he replies, that he has seen cases precisely similar without anasarca, but fatal, in which after death the kidneys presented the characteristic appearances of advanced Bright's disease.

One of the cases with dropsical symptoms, I shall briefly quote here. The patient was a retired merchant in the country, who had been attacked with universal and extensive anasarca. He was sixty-three years of age, totally blind, nearly deaf, very intemperate, cross-tempered, &c. His surgeon had failed to detect any disease in his heart, lungs, or liver, and had not thought of his kidneys. Dr. C. found his urine ot moderate density, rather pale, somewhat turbid, and pretty strongly coagulable. He directed digitalis, squill, and bitartrate of potass, to be given, to excite diuresis. These remedies had the desired effect; his urine acquired its healthy character, he perfectly recovered, and remained free of any urinary disease for seven years. He then again became Dr. Christison's patient for *gangrena senilis* of right foot, of which in about two months he died, without any return of anasarca, or of urinary symptoms of any sort. An examination of the body could not be obtained. Now, if any link was wanted to complete the chain of evidence as to a permanent cure having been obtained in this case, it would have been supplied by a post-mortem examination. To have seen and described the state of this man's kidneys, would have been most interesting, and probably conclusive. This link is, I think, supplied by the case which occurred to me, and which I have already detailed. The symptoms in my case were distinct and very severe, more so, I think, than in that of Dr. Christison; the general dropsy was very extensive indeed, and his urine was very highly coagulable; his distress was very great, and altogether he was considered, both by myself and the other medical gentlemen who saw him, to be in a most hopeless condition. Then the appearances on dissection, which were to me, after the lapse of so many years since his illness, intensely interesting—were to my mind quite conclusive that a process of repair had taken place in the kidneys. If, however, this be denied, it will, I think, be conceded that at all events the progress of the disease had been arrested, that atrophy of the organs as well as of the diseased structure had taken place, and that what was left of healthy structure had recovered its functions. To any one at all acquainted with the aspect of the granular kidney in its advanced stage, the appearances seen in the case described are quite different. The effects of the disease, it is true, were there—the organs were atrophied

—but they presented the natural pink color, quite distinct from the sandy, pale, blanched appearance of the granular degeneration; and, as the man's history proved after his recovery, they were perfectly competent to perform their functions.

It is very likely that Dr. Christison, or other observers, whose attention has been directed to the curability of Bright's disease, may, since the two very interesting lectures to which I have referred were delivered, have been able to verify by dissection cases where real recoveries of long standing had taken place. As, however, I have not been able to find any recorded cases, where, after the patients had presented unequivocal signs of the disease, they had recovered, remained well for a series of years, died of some other disease, and presented no signs of urinary disease during the last illness, and where the appearance of the kidneys was described after death—I have thought it right to record this as such a case. It affords additional confirmation of the reality of the fact that such recoveries do sometimes take place, and I beg to adduce it here in corroboration of the accuracy of the opinion ennunciated by the very distinguished observer, whose lectures I have taken the liberty of referring to.

The case is, however, deficient in one very important particular; there is no microscopical report of the structure of the kidneys. This I regret extremely, as it would have rendered the case complete: but as it did not occur in town, there was no means of having a microscopic examination conducted by a thoroughly competent observer. It would have been very interesting to have ascertained what appearance the atrophied kidney presented, as well as the nature of the intimate structure of the compressed granular matter. It is not likely that any fatty matter existed, as this form of the disease is by all observers considered to be incurable. It is possible, however, that some reparatory process might have been detected, perhaps that described by Mr. Simon in the 'Medico-Chirurgical Transactions' (1847), viz., a cystic arrangement by which vesicles are substituted for the normal tubularity of the gland, and new secretory apparatus organized.

This case was originally intended for insertion in the 'Clinical Record,' and but for its length it would have been placed there. It affords encouragement, and should lead us not to despair of any patient, however bad his symptoms may be; for if he be of temperate and regular habits, as in this instance, and the other organs be healthy, the disease in the kidneys may be arrested in its progress, and recovery may be the result. It should never be forgotten, that, as Dr. George Johnson remarks, "it is not precisely a disease *in* the kidney, but a constitutional disease manifesting itself *at* the kidney," which we have to contend with.—*Glasgow Med. Journal, Oct.* 1859, *p.* 263.

SURGERY.

AFFECTIONS OF THE BONES AND JOINTS, AMPUTATIONS, &c.

49.—ON EXCISION OF THE KNEE-JOINT.

By Oliver Pemberton, Esq., Surgeon to the General Hospital, Sydenham College, Birmingham.

[One great question with regard to the operation of excision of the knee is the utility of the limb preserved; for if in any class of cases it should prove that the limb, even if preserved, is perfectly useless, and even an encumbrance, the advisability of the operation in such class of cases falls to the ground. In December, 1853, Mr. Pemberton removed the left knee-joint of a pale strumous lad, aged 12, for extensive disease of the joint. The following is the report of the state of the patient in October, 1859, *i. e.*, six years after the operation :—]

The patient came to my house. He had grown in height, and had thickened considerably in his figure, but was somewhat diminutive for his age (18). The lower limbs presented a wonderful contrast in appearance. The one was strong, with the muscles, bones, and joints well defined; the other feeble and blighted. The sound limb, from the anterior superior spinous process of the ilium to the outer malleolus, measured thirty-four inches; the one subjected to operation, twenty-five! Thus showing a difference of nine inches; or a deficiency in growth as compared with the other of rather more than five inches since the operation. In the middle of the thigh, in the lower third, and round the calf of the leg, the measurements were sixteen, twelve, and twelve as opposed to thirteen, ten, and nine and a half. The foot of the limb operated on appeared to have expanded most, but even this was an inch and a half shorter than its fellow. The femur, the tibia, and the fibula of this limb were not larger than the same bones in a child of ten or twelve.

About the excised joint, the parts were sound, and free from pain on manipulation, a very moveable ligamentous or fibrous medium connecting the articular extremities. Notwithstanding the shortness and the flail-like joint, it was astonishing to see the power he possessed of extending the leg, and of bearing the entire weight of the body on it in walking, unaided by support of any kind; and it was quite clear that the disparity in length alone prevented him from realising all the advantages that he might under other circumstances have obtained from the operation. With all these drawbacks, he works hard as a boatbuilder, the limb being aided by a cork sole of some six or seven inches in height, and by a leather case at the knee.

The limb cannot, however, be deemed otherwise than an incumbrance, and with the best appliances to remedy the want of length, proving, after all, little better than a sad deformity.

In 1855, an authority on excision of the knee thus expresses his opinions on this question of subsequent growth.

"Another objection has been brought forward against the operation of excision, and which demands grave consideration. It has been asserted, that where the operation is performed in early life, the growth of the limb has been checked, and, consequently, that the lower extremities do not keep pace with each other, and hence one becomes so disproportioned to the other as to be ultimately useless." (Butcher, 'On Excision of the Knee-Joint,' First Memoir, p. 46).

In 1857, the same writer reiterates these sentiments. "In my former essay I proved, by several instances, that the growth of the limb was not checked by excision of the joint in childhood; and am happy now again to confirm this most important fact." (2nd Memoir, p. 60.)

Speaking of excision of the knee, Mr. Syme writes: "I tried the operation nearly twenty years ago, on a boy, who perfectly recovered from it, and seemed at first to possess a limb little inferior to its fellow, except in so far as it was stiff at the knee. But, in the course of time, it was found that the growth of the two limbs was not equal, and that the one which had been the subject of operation gradually diminished in respective length, until it wanted several inches of reaching the ground when the patient stood erect." (Pathology and Practice of Surgery, p. 225; 1848.)

I have quoted the preceding remarks word for word, as they commence and end, in the eighteenth article of Mr. Syme's work, in preference to alluding to his mere opinion on the matter; for the same paragraph, in all its identity, has been frequently referred to and cavilled at during the past nine years.

Unhappily, in the case I have narrated, I fear that another "existing proof" of the fact *is* added to the only one that Mr. Butcher (First Memoir, p. 46) could discover, when he wrote his first and second memoirs on excision of the knee, that the growth of the limb is at least sometimes checked when the operation is performed in early life. This most undaunted surgeon has, I fear, been led to adopt those conclusions on the question which his wishes and anticipations too clearly held in view. He does not write from his own experience, but strengthens the position he assumes by the communicative observations, amongst others, of Dr. Keith of Aberdeen, Mr. Page, of Carlisle, and Mr. Jones of Jersey.

Dr. Keith's case was a boy, aged 9, from whose knee two inches of bone in the whole, about equal portions from the femur and tibia, were removed; three years afterwards, the limb was reported as "plump, and growing as fast as the sound limb." There was firm bony anchylosis.

Mr. Page's patient was a young lad aged 17. Two inches and a half of the femur and tibia were removed. There was firm bony anchylosis. Reporting on his condition four years afterwards, he writes: "The growth of the stiff limb has quite kept pace with that of its fellow."

Mr. Jones's observations refer to the cases of three boys, aged respectively 7, 11, and 9. In the first instance, two inches and a quarter of bone were removed. There was bony anchylosis. In the second, rather more than four inches of the femur and tibia were removed. There was bony anchylosis. In the third, the exact measurement of bone removed is not mentioned; but there was bony anchylosis. This patient died at the end of twelve months; and Mr. Jones writes: "There cannot be a doubt that in this instance the limb from which the joint was excised, kept pace with the other in every respect." Whilst in regard to the first mentioned cases no disparity of growth is hinted at.

From Dr. Keith, the distinguished surgeon of Aberdeen, I have received the following most lucid report of the present state of the boy's limb, on which he operated nearly six years since. Dr. Keith's letter is dated October 12th, 1859; and he thus writes to me:

"Dear Sir,—I have pleasure in replying to your favour of the 7th current, but have not the additional pleasure of reporting so favourably of John Hay's limb, as I seem to have felt warranted in doing three years ago.

"Be the cause what it may—and I shall be delighted to have your opinion on the subject—I, and I presume every one who has had experience in resection of the knee, feel convinced that growth to the length is all but arrested in the lower limb after every such operation.

"I delayed my answer until I should have opportunity to see and to measure John Hay's lower extremities; this I accomplished yesterday, and annex the result.

"He is now a stout healthy boy, in his fifteenth year; his left or sound lower limb long and well developed. His right limb is really plump to look at, but seems a mere appendage to his body, when compared with the other limb; the right limb being now *seven inches* shorter than the left limb, from the anterior superior spinous process of the ilium to the heel.

"At the thick of the thigh the left measures 17 inches round; the right,

16. Around the knee and patella: left, 12½ inches; right, 12. Around the calf of the leg: left, 11½ inches; right, 10½.

"In length, the left limb is 17 inches from the anterior superior spinous process of the ilium to the centre of the patella, and 17 inches from the same spot to the sole of the foot, in the whole 34 inches; while the right was only 12½ to 14½; total, 27 inches.

"Yet, wearing a boot propped up by two steel rods seven inches long, he walks and runs with great ease and energy; and when I yesterday asked him, if he now wished the limb had been removed, he at once exclaimed: 'Oh, no! my leg is worth a thousand wooden ones; it is my own leg, sir, and I feel it to be so.' I daresay the poor fellow is right. The arrest of growth to the length is a mystery. Let me see that you solve it.

"To account for my statement made to Mr. Butcher three years ago, it is only necessary to state that exercise had developed the muscles so, that the limb was plump; he wore the same high-heeled boots that I had at first provided for him, so that no increase of disproportion in length had at that date taken place between the two limbs; but now my guess is, that the dwarfish little fellow had been standing still, and neither limb growing in length, it was only when he began to shoot out in earnest that the difference became apparent, and new boots with higher and higher props became the order of the day.

"Hay's right limb has an outward curve at the knee now, though anchylosis is perfect, and he thinks it is slowly increasing. That tendency from the hour of the operation has been often noted. How is it to be obviated?

"1. I think well of cutting the tendons forming the inner hamstring.

"2. Of slicing rather more off the inner than the outer condyle of the femur.

"3. Using a splinted knee-cap for life.

"The patella was spared in this case, and has strong union to both femur and tibia; and yet with all this the callus yields after five years."

The foregoing narrative of Dr. Keith will be attentively perused by practical surgeons. In two respects his case possessed an advantage over mine. There was anchylosis; and but half the quantity of bone was removed. Notwithstanding, at the end of nearly six years, the deficiency of growth is measured by five inches—the same amount that was recorded in my case, and extending over the same period of observation, within three months.

The circumstances which led Dr. Keith to state, in the first instance, that the limb was "growing as fast as the sound limb," are, in my judgment, sufficiently explained by the presumption that neither limb had then grown, or, if they had done so, it was only in a very limited degree. I had the same impression some two or three years after I had operated on E. F.; but I found subsequently that the real stride in development had not then commenced; and that, when it did, the limb subjected to operation did not partake in its advance.

The lateral yielding of the callus is not, in my experience, of frequent occurrence, where firm bony union has taken place. It may happen in young subjects, and, if so, forms another drawback to the proceeding being adopted in early years; but it certainly does not occur in adults. I should, however, always use in the ungrown limb a splinted knee-cap, as Dr. Keith suggests, to guard against the possibility of the event.

The next case, which was selected to illustrate the parity of growth, was under the care of Mr. Page. But the patient was 17 years of age; and thus being past puberty, his condition cannot fairly be adduced in support of the theory.

Then we have Mr. Jones's three boys. The third boy, aged 9, died twelve months after the proceeding. His limb was found to have "kept pace with the other in every respect"—a statement after what we have now ascertained, that cannot amount to any determination of the question at issue. The first and second boys, however, in whom large quantities of bone were removed, should now have their limbs reported on, as, from the length of time that has elapsed since the operations (eight and seven years), the evidence which their subsequent history could afford would be most important.

Being anxious to know about these cases I wrote to Mr. Jones concerning them; but my letter arrived at Jersey at the time of the calamitous fire in the General Hospital, or I doubt not that I should have received the information I sought. As it is, I trust that the profession, through some other channel, will ere long be in possession of all the facts concerning their present state, which this most able surgeon can afford.

On what, let us inquire, when the operation of excision of the knee is performed in early life, and the subsequent growth of the limb does not correspond with its fellow—on what does this depend? Mr. Humphry says, "In young persons, care should be taken to make the section through the epiphyses of the tibia and femur; so that a thin layer of the epiphysis, with the cartilaginous medium that unites it with the shaft, is left upon each bone. If this precaution be taken, there is every reason to believe that the limb will keep pace in its growth with the opposite member." ('Med.-Chir. Trans.,' vol, xli. p. 216). This opinion of so distinguished an authority on the development of the skeleton as Mr. Humphry, is entitled to every consideration, and, at first view, it appears plausible; but it is not borne out by facts.

Mr. Price writes: "Should the epiphysis of the bones be removed entirely, there can exist but slight doubt that the arrest of further development will take place." ('Med. Times and Gazette,' April 23, 1859). If this were so, there ought to have been an arrest in the growth of Mr. Jones's first and second instances, in which the bones must have been sawn through beyond their cartilaginous joinings, as well as in the boy operated on by Mr. Paige, at whose age (nine) an inch from either extremity could scarcely fail to go beyond the epiphysis.

On the other hand, Mr. Syme informs me (July 20, 1859), that the articulating surfaces only were removed in the instance from which he drew the conclusions already cited, the bulging part of the bone being left.

But supposing that the bones in the thigh and leg do not grow when deprived of their heads, how are we to explain the want of growth in the cases I have narrated, in the foot and the fibula, in the absence of the plea of want of use, inasmuch as the boy has worked his little limb for years with persevering activity?

That the theory of growth being arrested in young subjects by the removal of the heads of bones, is an untenable one, we have other facts in surgical experience to prove. Take, for illustration, amputation in the lower third of the thigh or leg, or at the ankle-joint. I have never found an arrest in the subsequent growth of the bones in my experience, in the first class of cases; and I am not aware of others having met with a different result. The complaint has been, rather that the bone or bones in the stump become troublesome, and grow too much, so as even to require the removal of some inches at a period of years very distant from the operation. At the ankle-joint, I have especially watched the parity of growth. I had occasion to amputate at this articulation in an infant only twelve months of age, nearly six years since. The malleolar epiphyses were removed. The subsequent growth of the limb has been of the most perfect character—a condition invariably observed in several other instances where I have been called on to perform the operation at tender years.

That the limb may grow in the young subject after excision of the knee, the evidence which has been given me on the subject by my friend Mr. Humphry goes some way to prove, but not, from the age of the patient at this time, quite far enough to establish an accomplished fact, even in his well observed cases; but we must at the same time acknowledge that it may not do so—a conclusion that cannot fail to make us desire a further inquiry into the causes of failure in this respect, ere we determine to recommend the adoption of the operation in children generally.

The late Mr. Mackenzie hazarded the opinion that the want of anchylosis might exercise an influence on the subsequent growth of the limb. ('Monthly Journal of Medical Science,' June 1856.) He based it on Syme's case, in which the limb allowed a slight degree of flexion and extension; and considering that my case afforded even greater mobility, the conjecture, but for Dr. Keith's illustration to the contrary, might have deemed to be not unfounded.

It will be seen, however, that on the two points—the section through the epiphyses, and the union by anchylosis—Mr. Humphry's case is very conclusive in an argument of results. This able surgeon thus writes to me, July 22nd, 1859:—

"The only case I know of in which sufficient time has elapsed to enable me to derive satisfactory information respecting the growth of the limb after excision of the knee, is that of William Child, related at p. 196 of the last volume of the 'Medico-Chirurgical Transactions.' The operation was performed in August 1855, when he was twelve years old; and in two months there was firm union between the bones. In consequence of your note, I drove over to examine the lad yesterday, and find that during the interval of four years, the growth of the limb has proceeded *pari passu* with that of the other limb and of the rest of the body. The bones of the foot are quite as long as those of the other side—the tibia and fibula very nearly; certainly showing no greater difference than is due to the removal of a thin slice of the upper articular surface of the former. The femur is about an inch shorter than the other femur. The difference in this respect between the femur and the tibia, is I suppose, owing to the greater thickness of the slice removed from the condyles of the femur, in order to obtain a flat surface for union with the tibia."

A fair bonsideration, then, of the foregoing facts, leads us to certain conclusions affecting the operation of excision of the knee in young subjects. These are:

Firstly. That the proceeding is liable to be attended by a want of corresponding growth in the limb subjected to operation, as compared with its fellow; the result being, that ultimately the member becomes not only useless, but is an incumbrance, as it has failed altogether to grow in proportion to the general expansion of the frame.

Secondly. That whilst we, at present, have no proof that the removal of the epiphyses, or the failure in obtaining anchylosis, exercise any material influence on the subsequent growth of the limb, it nevertheless appears probable that adequate growth is more likely to be attained where care has been taken to remove as small a portion of the articular extremities as possible, and anchylosis has resulted.

Having testified to the truth, so far as my own experience can guide me, on this deeply interesting question, as it affects the operation in early years, I shall now rest satisfied, trusting in the hope that we may yet reach the means whereby even this cause of failure may be removed, as time enables us to know more and more the results that may have been realised by others.

[Although the circumstances which necessitate amputation are also those which indicate excision, yet the particular operation cannot be chosen indiscriminately in all cases, for each of these operations possesses certain advantages over the other.]

In chronic cases of diseased articulations requiring an operation, the state of the bones and soft parts must be attentively considered.

Those cases will be found most eligible for excision where the mischief in the bone is limited to the articular extremity, and where the soft parts have not become adherent to the parts beneath, from the presence of sinuses, or from the general effects of long-continued disease.

Where the disease in the bone cannot be removed without the sacrifice of too great an amount, or where the soft parts around the joint have assumed an unhealthy character, amputation should be preferred; a vigorous condition of the latter structures being quite as essential to subsequent repair and well-doing as is a section through sound bone for the attainment of satisfactory osseous or fibrous union. Cases in which there has occurred the most complete alteration of the synovial membranes, accompanied by destruction of the cartilages, together with the ligamentous tissues, so long as the bones and soft parts are affected within the limits laid down, will be equally suitable to excision.

In forming our judgment, the constitutional power of the patient must not be disregarded. There are many instances of knee-diseases, requiring an operation, in which the pulmonary tubercular tendency is also manifest. The disposition to mischief in the lungs in these cases renders them, in my mind, inelegible for excision—in fact, unable to bear prolonged suppuration; whilst the removal of the disease by amputation, followed by the rapid recovery usual in such persons after such an operation, is well known to be attended by the happiest results.

In acute cases of diseased articulations, with suppuration, the selection of excision must be attended with great hazard. The occasions on which this has been done are very few. Free incisions into the cavity of the joint should be made, in the hope of thus relieving the disorder and obtaining anchylosis, in preference. The constitutional powers not admitting of this treatment, it will be better, then, to recommend amputation rather than resort to excision.

For deformity, and for accident, we may be also called on to recommend excision.

In the first class of cases there are many well suited to the proceeding. Among these are rectangular anchyloses, or where dislocation of the tibia backwards has occurred as the result of former diseases. Such cases afford every prospect of an useful limb being obtained in the place of one not only a deformity, but an incumbrance.

In the second, at present, our experience is very limited. Mr. Canton, in the Charing Cross Hospital, performed the operation ('Lancet,' August, 1858) in a boy aged 8, who had received an injury to the lower end of the femur, whereby the epiphysis had been separated from the shaft. The case did well. My friend, Mr. Lakin, ('Midland Quart. Jour.,' Jan. 1858, p. 37), late assistant-surgeon to the General Hospital in Camp, before Sebastopol, also resorted to the proceeding in the case of a private of the 77th Regiment, who was wounded by a musket-ball which had penetrated the inner condyle. The case did well for twenty-six days, and then sank from diarrhœa and exhaustion. This was the only case that occurred in the Crimea.

The advantages of this proceeding in cases of accident must be, therefore, at present ranked as conjectural; but there would appear to be no valid reason why a primary operation of this character should not succeed in removing the impending danger of a compound fracture or laceration of the knee-joint as thoroughly as an amputation through the thigh.

The character of the medium binding the sawn articular extremities together, in the most favourable cases, will be found to vary very much. In some cases bony union does not seem attainable by any amount of care and quietude; in others, it is as rapid as unexpected.

Whilst, therefore, we may devote all our attention to the securing anchylosis, if possible, as promising, under all circumstances, the most serviceable limb, we need not be disappointed at there being a limited movement allowed between the bones when the cure is complete, as, in many instances where such a condition has been observed, the limbs have been, in their utility and power of endurance of fatigue, everything that could be desired; the only precaution to be kept in view being the necessity there is for wearing, under such circumstances, a splinted knee-case.—*Brit. Med. Journal, Nov.* 26, *and Dec.* 10, 1859, *pp.* 959, 998.

51.—AMPUTATION AT THE KNEE-JOINT.

(Case under the care of Prof. FERGUSSON, King's College Hospital.)

[Although noticed by a few of our metropolitan surgeons, the operation of amputation at the knee-joint has been somewhat overlooked. In nearly all the cases in which the leg has been thus removed, the patients have recovered with stumps of the most useful and serviceable kind, much better adapted to support the weight of the body by proper appliances, than when the stump exists in the thigh itself. The manner in which the operation is best performed is matter for consideration.]

It was customary at one period to make a large posterior flap from the fleshy part of the gastrocnemius, and to saw off a slice of the inferior surface of the condyles; and although in some cases the stump healed kindly, and was a good one, as the operation came to be repeated, it was deemed preferable to dispense with the large posterior flap, and also to leave the femur untouched. Mr. Lane, of St. Mary's, makes a large anterior cutaneous flap in front of the joint right down to the insertion of the ligamentum patellæ, which is then reflected upwards, the knife being carried through the joint. A small posterior flap is then made, so that in the process of healing it shall draw the anterior flap over the articular surface. The patella is retained, and forms a round and smooth surface to rest upon, as in the flexed knee, the skin being likewise movable over it, as in the healthy joint. The advantages of this method of performing the operation we have already adverted to.

In the subject of the following case, the notes of which were kindly furnished by Mr. Francis Mason, house-surgeon to the hospital, Mr. Fergusson made flaps of equal length, which met below the condyles, the patella being removed, and none of the articular surface being taken away. However favourable the case seemed to be, we regret to say it ended badly, owing, no doubt, to the irritable condition of the man's nervous system, induced by anxiety and restlessness. Non-success in this instance, however, cannot detract from the merits of this special form of amputation.

A man, aged forty-eight, was admitted, in October last, with a large tumour on the right leg. It appears that he had disease of the fibula for two years, and about eight months ago he was a patient in the hospital, when a circumscribed growth, as large as a fist, was present over the fibula, and was not then thought to be malignant. Mr. Fergusson, at that time, proposed the removal of the fibula itself, with the tumour, which appeared to involve the whole thickness of the bone; but the patient would not consent, and he left the hospital. For the last year the man has been very anxious to have something done, but he was very undecided. Lately he showed himself with the tumour enormously increased in size, fully as large as an adult's head, and involving all the surrounding tissues, both in the front and back part of the leg, so much so that it became impossible to relieve him of it by any partial operation. The tibia still appeared to be healthy, yet the nature of the swelling was somewhat doubtful from its extreme hardness, as the muscles and aponeuroses were stretched over it, giving it the feel of an osteo-sarcomateous growth. The rapidity of its enlargement, however, seemed to show its malignant character, and there was no pain. The inguinal glands were slightly enlarged. Amputation was consented to; and, on the 15th of October, the leg was removed at the knee-joint, the man being fully under the influence of chloroform; and not an ounce of blood was lost.

In some observations made after the operation, Mr. Fergusson remarked that this was a case in which it was necessary to remove the whole of the leg. Amputation at the lower third of thigh would have been the usual operative proceeding, but he thought he would resort to one less formidable, namely, amputation *at* the knee-joint. In his early operations, he removed the leg at the knee, and then took away a slice from the condyles of the femur; and hitherto the posterior flap had been made from the calf of the leg. Some surgeons, he said, have made a long anterior flap from the skin alone to cover the ends of the bone. This he considered somewhat opposed to the usual rules of surgery; yet in all this, there is a certain amount of novelty which ought to be investigated by the profession at large. He has had occasion, during the last fifteen years, to admire the stumps made *at* the knee in preference to those in the thigh; and referred to the case of a man, having a stump of this kind, who walked ninety miles in three days without inconvenience.

In making his flaps, Mr. Fergusson found the anterior one to be rather short, and the posterior one somewhat retracted. This was owing to the presence of the patella, which he considered to be drawn upwards. It was, therefore, removed, and the flaps were well brought together beneath the articulating surface of the femur, none of the bone being removed.

The patient went on well immmediately after the operation. The wound, however, soon fell into an ashy condition, and subsequently a fatal result ensued, which may have been much influenced by the constant mental anxiety evinced by the patient for many months before the operation. The tumour was found to be medullary cancer, springing from the shaft of the fibula.—*Lancet, Dec.* 10, 1859, *p.* 586.

52.—ON EXCISION OF THE KNEE-JOINT.

By Eben Watson, M.D., Surgeon to the Royal Infirmary, Glasgow.'

[Dr. Watson commences his paper with some tables and details of the more important of the cases operated upon by him, or by other surgeons in Glasgow, so far as he has access to know them. It seems, that the knee-joint has been excised eleven times in Glasgow upon the living subject. Of these cases only four have been successful; of the remainder four died from the effects of the operation; two after secondary amputation; and one after secondary amputation recovered. The pathological condition in these cases were very different. In a large proportion the synovial membrane was in that pulpy state, so commonly the result of chronic inflammation in a scrofulous subject. In many, the cartilages were likewise affected. In some pus had formed in the joint, and in a few the extremities of the bones were carious. With regard to the selection of cases, the author observes:]

The one important rule in all excisions is, that the diseased parts be *wholly* removed; and this can obviously be accomplished in all such cases as above alluded to, unless the bones be very extensively affected. There is, indeed, an apparent exception to this general statement, inasmuch as the whole of the synovial membrane cannot be removed, when the knee-joint is excised. Everybody knows that the large bursa above the knee-joint is lined by a synovial membrane, which is continuous with that of the joint; and, doubtless, the former is sometimes affected with pulpy degeneration, as well as the latter. But it is fortunate for surgeons that it is very seldom so severely affected; and at all events, after excision of the joint, if other things go on well, the disease in the bursa will not long or seriously hinder the favourable termination of the case.

It is different with periodical abscesses in the thigh connected with bare bone. From all that I have seen of such abscesses, I should consider them a positive counter-indication to the performance of excision, should they unhappily be present in a case otherwise calling for the operation. It would seem, however, that the actual presence of pus in the joint should not be a bar to excision; for in several of our cases that did not prevent recovery.

In determining on the propriety of performing this operation in any case, the general health of the patient ought to be carefully considered; and if it is not pretty good excision should not be recommended. We should always bear in mind that patients subjected to this operation must be able to endure a long confinement to the recumbent posture; and their health must all the time be in such a state as will maintain a prompt action in the wound. The great danger to be feared during this period, would seem to arise from irritative fever, which, when it sets in, soon reduces the patient to an almost or altogether hopeless state. I need hardly say that if the tubercular diathesis is very strong at the time of the operation, it will co-operate with the surgical lesion to aggravate the danger referred to. This then, should be considered very disadvantageous to the operation, and looked upon as calling for delay, or the adoption of amputation at once. Again, if there is active disease going on of any kind whatever in any of the internal organs, that should be a cause of delay, or, perhaps, a complete counter-indication of the performance of excision.

There is little doubt that some derangement of the general health, manifested very generally by disorder of the digestive organs, precedes that change in the wound which accompanies, or, perhaps, proximately causes the irritative fever. A liability to diarrhœa, or to sickness and vomiting, would also, therefore, be unfavourable, and should, if possible, be corrected before the operation is attempted.

A doubt was at one time entertained by surgeons as to the propriety of undertaking this operation in young and growing subjects; from the fear that the limb operated on would not grow correspondingly with its fellow. This, however, has been disproved by experience, and need not any longer occasion hesitation.

Operation.—Many methods of operating have been used in the performance of excision of the knee-joint. The oldest, and that which many still prefer, is to make an H incision; the patella being included in the upper flap, and the sides of the H being close upon and parallel with the hamstrings. This answers very well, but the wound is unnecessarily large. Another mode of operating is, by a single semilunar incision, commencing on the one side of the joint, and terminating on the other. This incision is generally made to pass below the patella, but Dr. Andrew Buchanan has always drawn it above the patella; and certainly this latter form of incision has the advantage of freely opening the bursa above the knee, and of avoiding the patellar ligament. It seems to me, however, that this likewise, is an unnecessarily large wound.

I have lately operated by a single straight incision drawn right across from the one side to the other of the knee. Its middle corresponds with the middle of the patella, which I cut out, and open the joint. This is a much smaller wound than that occasioned by either of the other two modes of operating; and it is quite sufficient for the purpose required, namely, of fairly exposing the articular ends of the bones.

The removal of the patella is a recent improvement in the performance of this operation. I adopted it for the first time in one case before I noticed in the journals that others had done so before me. Mr. Fergusson of London has especially recommended this proceeding, because he has, in several cases, found it diseased long after the excision had been performed; and he had actually been obliged, in such cases, to cut out the bone, before the sinuses which it had caused would heal. In all cases it must retard the process of union between the femur and tibia; for, being placed directly above the uniting parts, it causes a constant dribbling of pus upon them, which must be disadvantageous. Lastly, should the patient get over these difficulties, and should the bone remain in front of the anchylosed joint, it cannot be of the least use; for it is required neither to strengthen the limb, nor to facilitate the action of its extensor muscles.

In cutting the bones I now use Graham's saw, and slant its blade so as to cut the end of the former convex, and thus save its length while I remove its entire surface. Again, I cut the head of the tibia, so as to make a concavity into which the convex end of the femur fits.

Any of the morbid structures which have not previously been removed, should now be cut off; and finally the knife should be drawn downwards at each extremity of the wound, so as to facilitate the escape of the discharge. The limb, in most cases, may now be straightened, and a few wire sutures inserted in the front of the incision. No bloodvessels of importance are cut in the performance of this operation, so that ligatures are seldom required, and almost no blood is lost.

One other point of importance belonging to the operation has occasioned some difference of opinion amongst surgeons; and that is, whether or not the hamstring tendons should be cut, when from long disease they have become contracted, or whether they should be gradually extended afterwards. Now, our experience at the Royal Infirmary shows, that the latter process gives far too much pain, and interferes too importantly with the granulation of the wound, to be received as the proper practice. In one case, the worst consequences were attributed by the attending surgeons to the adoption of this proceeding. On the other hand, the cutting of these tendons already exposed at each side of the wound, could not do any harm, but on the contrary, would make the future management of the limb a matter of comparative ease to both patient and surgeon.

Dressing and Management after Excision.—This brings us to the consideration of the future conduct of the case; and, first, as to local management:—

I think most surgeons have agreed in the propriety of fixing up the limb in the position which it is to retain, before the patient has been removed from the operating table, and before he has recovered from the anæsthetic action of the chloroform.

What kind of splint is to be used? Mr. Butcher employs what he calls a fracture box fitted to the limb and well padded. The sides of this box turn down, and permit the dressing of the wound without moving the limb to any great extent.

Mr. Fergusson seems to employ a posterior splint and a long straight Liston's splint outside. Others have been contented with Macintyre's splint alone, or combined with the straight Liston's thigh-splint.

I have found it best to have no dressings applied to the limb at all, except a bandage round the foot and footpiece, pads of cotton, and water-dressings on the wound; so that, with these exceptions, the limb lies naked in the splint. It may thus be kept quite clean, and has enough of support for the first three or four weeks. About that time the limb becomes flabby, and the granulations often prurient, so that the bandage may with advantage be continued up the thigh from the foot. By this time the discharge will have greatly diminished, and the patient will be quite able to bear the necessary moving of the limb in applying the bandage.

I have also found the splint above described better than any other in another respect: viz., that it can be very easily taken off and exchanged or washed. If the tapes are all loosed, the side bands may be pressed back, so that the limb is then on a flat suface, from which the surgeon may lift it, without such difficulty as if it were in a box or a Macintyre's splint.

It is of the greatest consequence that the back part of the knee be well supported after excision, otherwise dislocation is apt to occur from the ends of the bones falling backwards. Now, there is nothing to prevent this in Macintyre's splint, which, when straight, has a vacant space in the position referred to. But in the splint which I recommend there is a flat and even surface all the way down the limb, on which, when properly padded, it lies equally supported at all points; and I think that no practical surgeon will undervalue the circumstance of the lightness of the apparatus by which these results are accomplished; for he knows that the weight of an apparatus, applied for any length of time to a patient confined to bed, increases his discomfort, as well as the danger of bed-sores.

This apparatus should be kept applied till the wound has healed, and till the patient can bear the limb to be moved without pain. I would not recommend the bony union of the tibia and femur to be very severely tested at this date; but I would exchange the apparatus for a shorter posterior splint of gutta-percha, reaching from the calf of the leg to the middle of the thigh, and applied with a roller-bandage. By and bye the patient may be allowed to rise and walk on crutches; but he should be carefully assisted at first, lest he fall and injure the affected limb, which might produce even then a fatal relapse.—*Glasgow Med. Journal, Oct.* 1859, *p.* 276.

53.—ON A RECENT CASE OF RESECTION OF THE KNEE-JOINT.

By H. Grainger Earnshaw, Esq., M.R.C.S.E., Senior House Surgeon to the Liverpool Northern Hospital.

[The writer commences by stating his conviction that this case, though unsuccessful, offers much encouragement to the more frequent performance of the operation of excision of the knee-joint. The patient—an Irishman, forty-four years of age—was admitted a few minutes after receiving a severe injury to the knee, from a fall of ten or twelve feet.]

The immediate consequences of the fall to the knee were—1, a semi-elliptical wound, almost as if incised, commencing at the tubercle of the tibia, and continued upwards and backwards to corresponding points on the lateral surfaces of the joint, somewhat about its centre; 2, a jagged and total division of the ligamentum patellæ; 3, comminution of the lower part of the patellæ; and 4, when the limb was purposely flexed, exposure of the whole interior of the joint.

At a consultation held three hours afterwards, preference was unanimously given to resection of the joint, rather than to amputation above it. Mr. Chalmers, who had charge of the case, accordingly operated at once. The flap was already half formed, and only required lengthening backwards. About an inch was removed from the extremity of the femur, and a very thin slice was sawn off the apposed surface of the tibia; the patella, with its fragments, was dissected out, and the slightly-lacerated edges of the flap pared. The ends of the bones were then applied to one another, and the limb was laid and secured in a straight, hollow splint (Liston's Macintyre's); and after the flap had been united to the integument below by a single metallic suture, simple wet lint was placed over the knee-part, and the man was then carried to bed, and carefully adjusted on air and water-pillows.

Chloroform was administered for the operation. Very little blood was lost, and only three small arteries required tying. The shock of the operation was of the very slightest amount. A little while after the operation, the patient had a forty-minim dose of Battley's solution, and this was repeated in the course of the afternoon, and again during the night. Water was constantly dripped on the lint, and milk and water only allowed for drink.

[The case is reported daily—progressing very favourably up to the 17th day after operation—when it is observed that the appetite, hitherto excellent, failed. Next day, Nov. 1st, the report continues:]

Seems not so well, but makes no particular complaint; his countenance is less bright, and there is a sensible alteration in its expression—from what cause cannot be made out; the discharge from the wound continues unabated.

From this time until the following Tuesday (the 8th) he gradually changed for the worse. About the 4th, it was suspected, in consequence of the change, and of slight fluctuation thereabouts, that matter was accumulating in the upper third of the thigh; still, there was no swelling or tension, and no heat or pain.

8th. There is now no doubt about pus being collected in the upper part of the thigh. A deep opening was made and a pint and a half of thick yellow matter, containing lumps of disorganized cellular tissue, let out; and it is fully anticipated that there is a direct communication between this cavity and the knee.

Between the 8th of November and the 20th, the man's condition and prospect of recovery varied a good deal. At one time he was better in spirits and appetite; at another, he was worse again. All the time, the amount of discharge from the opening in the thigh diminished, while the amount and healthiness of that at the knee remained the same. On the 18th, the limb was for the first time taken out of the splint, there being evidently some tolerable union betwixt the femur and the tibia, and the whole length of it was placed on a water-pillow, secured in a straight position by cushions on either side and by a footboard at the foot. On the 20th, he was shivering, vomited frequently, and complained of intense pain in the opposite knee; upon examination of which there was no swelling, and but little pain on pressure. He now began to sink, and died on the night of the 23rd November, forty-one days after the operation.

Post-mortem Examination.—The whole flap had become soundly adherent, except for a small space at the outer and inner angles, whence the discharge escaped. The tendon of the rectus above the patella had also become firmly joined by fibro-cartilage to that below, and acted as a strong ligament between the femur and the tibia. After further dissection, the line of suture was seen to be concealed all around by a thin periosteal capsule. This was torn across by bending the leg, and in doing it the amount of direct union between the bones was measured by the reluctance with which they separated. For the eighth of an inch in depth from the circumference their tenacity was considerable; beyond this, to the centre, there was no adhesion. Also, on examining the two surfaces, around the borders was found fresh osseous matter, which could be scraped off and cut in fibres, and which grated against the knife; but it did not exist at all in the centre. The middle portions of both bones were of a deep-red colour, and easily penetrable by the point of the scalpel. The ossification commenced at the circumference, and was proceeding to the centre. The anchylosing process seemed to have been going on altogether prosperously. There was no swelling of the soft structures around the knee, nor had they a large suppurating surface underneath to explain the source of the discharge which had been continually, though moderately, given out from the outer angle of the flap since the eighth day of the operation. The cause of this was explained by laying open the thigh, and exposing its interior down to the knee. There was no abscess having defined walls; but an irregular cavity existed between the skin with its fat and fascia and the triceps muscle, which communicated with the outlet at the knee by one or two large sinuses. The muscles looked pale and shrunken, and as if their own investments had had most share in the profuse suppuration. In some respects the premonitory signs of death appeared to resemble so much those of pyæmia as to cause it to be suspected that this might be the cause of death. But this suspicion was not confirmed; for all the joints, including especially the left knee-joint, were quite healthy, and so were the lungs. The death was satisfactorily accounted for by the extensive suppuration amongst the muscles of the thigh, and by the irritative fever and exhaustion which followed in consequence.—*Lancet, Dec.* 17, 1859, *p.* 613.

54.—ON THE TREATMENT OF CONTRACTION OF THE KNEE

By Holmes Coote, Esq., F.R.C.S., Assistant-Surgeon to St. Bartholomew's Hospital.

[If cases the result of accident, or those proceeding from malignant disease, be omitted, undoubtedly the greater number of the remaining cases of anchylosis of the larger joints are owing to disease, either rheumatic or scrofulous in its nature. It is worthy of note that osseus anchylosis much more frequently follows rheumatic than scrofulous inflammation. The bones are of their normal weight and firmness in rheumatic, but in scrofulous diseases of joints these bones are light, and give every indication of want of power. In the museums, preparations of osseous anchylosis of the hip are much more common than those of a similar state of the knee.]

It is scarcely possible to attach too much importance to this fact respecting osseous anchylosis of the knee, for it serves us as a guide in most important points of treatment. In incipient cases of strumous disease, we may safely predicate that the contraction is chiefly due to muscular action; and we should, therefore, repudiate any such proceeding as that of the "*the forcible extension of the limb, the patient being under the influence of chloroform;*" well knowing that, under the full action of the anæsthetic, the muscles relax of their own accord, and the limb becomes straight without the necessity of any force.

In some cases there may be fibrous bands, which had, however, better be stretched than torn; or tendons may be so tight as to need subcutaneous division. The bones of the leg may be drawn backwards into the popliteal space, or the knee may be turned inwards, as in genu valgum; but there is still movement in the joint, except in cases where there has been an extraordinary amount of destructive disease: and it will be found that infinitely more good can be effected by steady, slow mechanical extension, than by any amount of sudden violence, however skilfully employed. But in cases where the joint has been destroyed by an attack of acute rheumatic inflammation, the opposed surfaces of the tibia and femur may become united by bone, the patella adhering to the outer condyle; and here mechanical extension often fails to act upon the limb. The resistance offered at the knee is such that the necessary counter-pressure bruises and excoriates the limb; and here we may with advantage forcibly break through the bony adhesions, which are rarely very strong; and then gradually extend by mechanical apparatus, as in the former case.

The question may be asked, Why not extend the limb at once, when the bony adhesions have been broken? The reply is, You wish to extend the limb without pain; without injury to the flexor muscles of the ham; without chance of stretching, tearing, or otherwise injuring the popliteal vessels and nerve. Some months ago I amputated the limb of a young man, at his urgent request, in whom the knee-joint had long been contracted, and various methods of treatment had been adopted without success. He declined any further trials in this hospital, saying that he could not follow his avocation with a diseased knee. Extension had been tried on several occasions, On examining the limb after its removal, ecchymoses were found extending along the hamstring tendons; the muscular fibres had been torn and bruised; the popliteal vessels were so thickened and changed in structure that violent and sudden extension might have been followed by injury to their walls

·The patient needs the support of the muscles and tendons surrounding a joint; it is they which contribute so much to the strength of the limb. If they are torn and injured, the patient loses the necessary command over the bones.

And here it may not be out of place to state why it is that in cases of disease of the knee the leg becomes flexed on the thigh. When the leg is fully extended, the ligaments of the knife, especially the two lateral and the crucial ligaments, become extremely tight, and hold the bones firmly pressed against one another. The lower extremity moves in extension as one piece. Under circumstances of disease, this pressure cannot be borne; hence the muscles flex the leg, and, by so doing, relax the ligaments. and set the bones free. This is another reason why forcible extension is unscientific and wrong in cases of progressive diseases of the knee-joint. We should always wait until active disease has subsided.

It is not difficult to ascertain the nature of the contraction, whether muscular, fibrous, or osseous. In the first instance, the tendons of the hamstring muscles are at once felt quivering in spasm on the least attempt being made to extend the leg. In the second, the patient complains of pain about the ligamentum patellæ when the leg is drawn forward; the hamstring muscles also contract, though not to the same extent as in a former state. When the union is osseous, the parts are flexed and immovable; extension seems to exert no influence on the muscles, and the patient complains but slightly of pain. There are, of course, other points which in each case will occupy attention; but these constitute the broad rule.

We may in very many cases promise to the patient, with safety, the advantage of an extended limb, which will be useful in progression; but we must not speak of "restored motion." When the tissues which are proper to a joint—namely, synovial membrane and articular cartilage, with the necessary ligaments—have been destroyed, free motion never can be regained. In order to effect such a phenomenon, we must supply our patient with a new joint. Let any one look around the different museums, and examine the specimens illustrating these diseases, and ask how free motion could have existed under the varied circumstances there presented for examination and reflection. But a moderate amount of movement may be very useful; and a stiff and extended limb is infinitely preferable to any artificial contrivance.—*British Med. Journal, Dec.* 17, 1859, *p.* 1015.

55—ON SOME COMPLICATED INJURIES ABOUT THE SHOULDER-JOINT.

By Frederick C. Skey, Esq., Surgeon to St. Bartholomew's Hospital.

[The following remarks are abstracts from a clinical lecture delivered at the Hospital by Mr. Skey. He commences with the most frequent form of injury to which the shoulder is liable, and proceeds subsequently to other injuries which, though not of infrequent occurrence, he thinks occasionally escape detection.]

If you inquire into the nature of the violence which has led to dislocation of the *humerus* (not of the *shoulder*, as it is commonly termed, for the shoulder, correctly speaking, is incapable of dislocation), you will generally make out that in the act of falling the man's arm has been extended for the purpose of breaking the fall; and that direct force has been applied on the under surface of the limb, whether hand fore or upper arm; the result of which is, that the head of the humerus is thrust out of the glenoid cavity, by a counter force rupturing the lower part of the fibrous capsule. The ultimate position of the head of the bone depends much on that of the arm at the moment of the fall; whether extended straight from the trunk,—the most common position. when the head falls into the axilla,—or forwards, or backwards, when it is found in either case in the opposite direction.

I shall, however, take as an example a case of dislocation into the axilla. The shoulder is flattened by the removal of the support, as it is termed, given by the head of the bone to the deltoid, which, being stretched, shows the clean outline of its origin. If you pinch it, you find it firm and tight—so tight, indeed, as to draw the elbow away to the extent of about four inches from the side of the body. If you attempt to approximate the arm to the trunk, the latter will recede, because the movement is a painful one, by drawing yet more positively on the already stretched deltoid and supra-spinatus muscles. If, on the contrary, the arm be raised, and the deltoid relaxed, pressure of the fingers upon it will detect the absence of the normal support it possesses in health, and the head of the bone will be felt in the axilla. If the hand be now applied in a similar manner to the opposite axilla, the different relative positions of the bones will be obvious; and, finally, it will be found on measurement that the affected arm is about an inch longer than the sound one. And this sign alone is conclusive as to the existence of dislocation, because the scapula being unchanged in its position, if the humerus extends an inch longer than the opposite bone, it is clear that its head cannot reach the socket. Add to the above the acknowledgment of a recent injury, and you have abundant evidence of simple dislocation of the humerus into the axilla.

The only variety of this accident to which I shall refer is that of dislocation forwards and inwards, in which the head of the bone is carried far upwards towards the clavicle, and when it is said to lie under the pectoralis minor or under the coracoid process, and which process obstructs its further progress in that direction. This is a rare variety of this dislocation, and I should not have alluded to it but for the purpose of marking two features peculiar to it: 1st, that the arm is rather shortened than lengthened; and 2nd, that it is usually accompanied by numbness of the arm, consequent on pressure of the head of the bone on the brachial plexus. The numbness accompanying ordinary dislocation is either absent or very partial; in this variety it is large and general. In both instances it subsides on the reduction of the displaced bone. I remember, some thirty-five years ago, a man being brought into the hospital with a dislocated humerus. While undergoing the necessary manipulation, distinct crepitus was felt in in the joint. Its nature and locality were uncertain. The injury was very deliberately examined by the entire surgical staff. Much discussion arose on the case, which was evidently one of dislocation. However, the bone was reduced by the ordinary method adopted at that era,—namely, by extension of the arm outwards,—and the crepitus was felt no more. The man recovered in the usual time. A second case presented itself within the year, the entire features of which were precisely similar to the former one.

The impression produced on the minds of many observers of these cases has ever been, that it is possible to have, as the accompaniment of dislocation of the humerus, a crepitus, not caused by a fractured bone, but arising from some unusual injury sustained by the joint; and for many years I was myself inclined to adopt the same explanation, but later experience has taught me otherwise. It appears to me more reasonable to infer in these cases an undetected fracture, than the presence of crepitus without fractured bone. I cannot comprehend the sounds or sensations of crepitus to be caused by the attrition or rubbing together of any two structures of which the shoulder-joint is composed; and under these circumstances I cannot doubt that the injury in both these cases consisted of fracture of the glenoid cavity superadded to dislocation; and when it is recollected that the mode of reduction adopted at that period consisted in drawing the humerus outwards, it is not surprising that the fracture should escape detection during the operation; and if it then escape observation, it is not probable that it would attract notice afterwards, because the simple agency of rest would be available to the recovery from both the fracture and the dislocation.

While employing the term "crepitus," I may observe that there are three distinct varieties of sensation to which the term may be applied, and which it would be well for you to observe. The first is the sensation conveyed by the rubbing together of the two fractured ends of the shaft of a long bone in a healthy subject; the second, the sensation peculiar to the rotation of the thigh in fractured neck of the bone in very old or debilitated persons, which resembles the cracking of an egg-shell; the third, the sensation occasionally conveyed to the touch by the movements of a diseased joint. The last of these is very distinct from the other two, and is the product of tissues less hard than bone. The term "crepitation" is limited to the breaking down of membranous tissues by the pressure of air confined within them. In the two cases I have above quoted the crepitus was clear and distinct, and, as I consider, could have been obtained only by the attrition of a broken bone.

The form of injury which I am about to describe is that of fracture of the glenoid cavity. A man sustains a fall on the shoulder, with the arm parallel to the body, but the force of the blow, whether the result of a fall or of direct violence, is thrown *on the deltoid*. The result is the fracture of some bone, indicated by palpable crepitus. Observation will teach you to judge tolerably correctly, by the sensation thus obtained, whether the crepitus indicates a large or a small fracture, and such knowledge will prepare you for a correct diagnosis of the real injury. Now, the diagnosis of fracture of the glenoid cavity is obtained chiefly by negative evidence, and in recalling to your recollection the bony structures immediately around, you subject each to a careful examination. You examine the clavicle, acromion, and the spine and coracoid process of the scapula. You find all bear forcible pressure, and may therefore be inferred to be sound, you next examine the head of the bone. Pressing your hand firmly on the deltoid, you rotate the arm freely, and you find the head to move as freely with the shaft, and the movement to be unattended with pain. You repeat this examination by raising the arm at right angles with the body, and pressing the thumb forcibly on the head of the humerus in the axilla, you again rotate, and the movement is both painless and inaudible, revealing no fracture. Nothing remains but the scapula, which, if you are correct in your diagnosis of crepitus, must be broken. It can be nothing else. If you now, while pressing the head of the humerus against the glenoid cavity, rotate the bone, the mystery will be solved, and crepitus will be evidently the result of fracture of a greater or less portion of the glenoid cavity. This evidence can only be obtained in one way—viz., by pressure of the head of the bone against the broken socket. The existence of fracture is borne out by the direction as well as the violence of the force which has caused it, and by the presence of local pain, due not to the broken bone merely, but to a corresponding injury done to the soft parts over and around it. If you have arrived at your conclusions hesitatingly, if a doubt remain on your mind as to the existence of fracture, the patient himself will often solve it on putting the question to him—Have you broken a bone?—and especially if a man of somewhat more than ordinary intelligence; because, however palpable may be that compound sensation of hearing and touch we term crepitus, it is still more palpable to himself when his mind has been directed to it by the probably protracted discussion that has taken place on the nature of his accident. When a patient is made cognizant of the nature of the accident, his evidence becomes valuable. These cases cannot be readily mistaken for dislocation, for in truth they have no sign in common with it, except the disinclination to move the arm, which is universal in its application to all injuries in this region; but especially is there no diminution in the rotundity of the shoulder, and no increase in the length of the arm.

Several of these examples of fracture of the glenoid cavity have come under my observation during the last year. Both the nature and the extent of the injury are readily detectable by the inductive method of examination I have described. The extent of the fracture is not always so easily determined; yet by carefully observing the character and amount of the crepitus, and noting whether it is readily produced by the act of elevating the arm, and pushing the head of the bone against the socket, we shall make a close approximation to the truth.

Fracture of the glenoid cavity may be complicated with dislocation of the humerus; but, so far as my own observation leads me, I believe it to be far more uncommon than simple fracture without dislocation. If the fracture as usually occurs, when superadded to dislocation, is confined to a small portion of the glenoid cavity, the signs of fracture merge in the apparently larger accident of dislocation, the evidence of which is uninfluenced by the presence of the injury to the bone. The shoulder is flattened, the arm is lengthened, the head of the bone revolves on the thumb pressed upon it in gentle rotation of the bone, while the fractured scapula lies behind the head, and escapes observation, unless the violence has been unusually great and the fracture unusually large, and comprising one-half or more of the socket from which the head is thrown. The evidence of dislocation is conclusive, while that of fracture is so imperfect and is so uncommon, that the injury escapes observation. The heel is applied, the bone is reduced, and although the return of the head into the socket may be accompanied with some unusual sensation, which is really that of crepitus, the reduction has been so satisfactory, the restoration of parts so complete, that the question of fracture may scarcely enter the thoughts of the surgeon. Nor does there arise in the after-history of the case any circumstance by which to determine the occurrence of fracture. The fragments are brought into contact by the act of reduction, and in the course of a few weeks are united.

A case of more than usual interest occurred to me lately in the person of a man, aged sixty, in Abernethy ward, who had sustained an injury to the shoulder three weeks prior to his admission. The head of the humerus was obviously thrown from its socket, and the flattening of the shoulder was unusually great. The deltoid was tight, and the elbow was drawn from the side. The arm was lengthened by one inch. The head of the bone rotated with the shaft; the coracoid process was unbroken, but there was distinct crepitus on moving the arm. The man was very positive in his assurance that the injury was caused by a direct fall on the shoulder. I made an attempt to reduce the dislocation, under chloroform, by the aid of pulleys. I brought the bone towards its natural relation to the acromion process, by extension downwards, with a padded fulcrum in the axilla. The natural rotundity of the shoulder was restored during the extension, but on its remission it disappeared. On the second day following I repeated the attempt, more critically, persevering in the extension for nearly half an hour, but with no better success. While under extension the head of the bone became prominent, but returned to its abnormal position on remitting the extending force.

On reviewing all the circumstances of the case, there can be little doubt that the glenoid cavity is separated from the rest of the scapula either in a large proportion or absolutely. A healthy man, possessing the entire power of movement of his arm, sustains a fall directly on his shoulder; the head of the bone is struck inwards with force against the socket, and extreme flattening of the shoulder and crepitus are the immediate consequences; the arm is lengthened, and the elbow projects from the side; the unbroken condition of the head is rendered obvious by its free rotation under pressure; the coracoid process is firm in its position; while two attempts at reduction fail to restore the head of the bone to the glenoid cavity. In what can this injury consist, if not in extensive fracture of the glenoid cavity—the socket of the joint? I believe the fracture to be a large one, because, having seen many cases of fracture of the lower portion of the glenoid cavity, I have never observed one example in which the symptoms were so strongly marked as in this case; while the slipping back of the head of the bone, after being brought on two occasions to its normal position, appears to infer the absence, and therefore the inability of the socket to retain it. The only treatment the case appeared to me to be susceptible of, was that of placing a large pad in the axilla, and leaving the rest to nature; and this plan was adopted.

In cases of fracture of a greater or less portion of the glenoid cavity, an easy position of the arm is all that is attainable, and indeed all that is required. The fragments are usually united in three weeks, during which the arm is placed in a sling.

I had recently another case of fractured scapula under my care in St. Bartholomew's Hospital. This fracture was the result of a fall with violence, but the precise direction of the blow I could not ascertain. The fracture extended vertically through the bone, either through or behind the notch. It was unattended by dislocation, or even by displacement. Its existence was ascertained by the presence of crepitus, which was well marked, and might be detected either by the pressure of the fingers, or by desiring the patient to put into action the trapezius muscle of that side. A large pad was placed under the scapula, which was firmly bandaged to the trunk. His recovery was both quick and complete. I have described elsewhere a case of fracture of the inferior angle of the scapula. The injury occurred to a lady, who was thrown from her horse. In this case I could only obtain crepitus by bringing into action the teres major muscle, which takes its origin from the surface of the scapula, through which the fracture had occurred.—*Lancet, April* 14, 1860, *p.* 368.

56.—*On Removal of Articular Cartilage in Amputation.*—[At a meeting of the Medical Society of London, Mr. Haynes Walton stated that he believed the usual impression among surgeons, as to the propriety and necessity of removing the articular cartilages when amputating at a joint, was fallacious.]

He had lately watched two amputations of the knee, and several about the large joints of the foot and arm, all of which had united by the first intention. He had lately removed a hand, shattered by the bursting of a gun, at the carpo-metacarpal articulation, leaving the carpus entire, bringing the flaps together over the irregular edge formed by the second row of the carpal bones covered by their cartilage, and a more rapid union he had never seen. The patient could move the carpus, and the radio-ulnar action was natural—no small advantages for the adaptation of an artificial hand. He thought the great advantage of not interfering with the cartilage was the less likelihood of necrosis of the end of the bone, or of suppuration, and other evils often attendant on cutting through a bone, or exposing its extremity.—*Lancet, Nov.* 5, 1859, *p.* 469.

57.—SCOOPING OF BONE SUBSTITUTED FOR RESECTION OR AMPUTATION.

M. Sedillot, of Strasburg has, for the last two years, seized every opportunity, in cases of diseased bone, of scooping out the affected parts, and leaving the cortical portion, rather than have recourse to resection or amputation. In April 1858, he brought this mode of operating before the Academy of Sciences at Paris, and then stated that he was led to adopt the method from observing the remarkable osteogenic powers of the periosteum, pointed out by M. Flourens and M. Ollier. Instead, however, of dissecting the periosteum from the bone, and removing the latter, (a proceeding which offers some difficulty), M. Sedillot leaves the whole cortical portion of the bone, as above stated, and removes the carious parts.

On the 31st of October last, the author brought before the same Academy an account of the cases operated upon in that manner. Ten patients recovered, and three died. Amongst the former, M. Sedillot mentions the case of a young girl, in whom he had scooped out the lower third of the femur and the condyles: she now walks very well. Another case is that of a young man suffering from caries of the lower part of the left tibia; the scooping here included the whole of the articular extremity, and the inside of the malleolus: the patient now works hard, and can walk fifteen or sixteen miles. The fatal cases may not be charged to the operation: one died of epidemic sloughing phagedena six weeks after the scooping, and the others several months after submitting to operative procedures. M. Marmy, of Lyons, and M. Ehrmann, a military surgeon in Algeria, have both sent to the author a successful case of this operation.

If we are not much mistaken, M. Sedillot's operation has much analogy to the gouging in caries so often successfully practised in the hospitals of this metropolis. But a real improvement in the operation of resection of joints is, the careful preservation of as much periosteum from the extremity of the articular surfaces some distance up the shafts as has not been destroyed by the progress of disease. This should be borne in mind by those surgeons who frequently perform resection of joints; nor are the practical proofs of the utility of these precautions wanting. M. Verneuil, of Paris, for instance, has placed several cases of resection of the elbow before the Academy of Sciences, and shows that, by dissecting very carefully whatever periosteum is left, he had, by regeneration of bone, in one of his cases, only two inches' shortening, after having removed altogether four inches of osseous texture from the humerus, radius, and ulna. In another case, M. Verneuil was able to leave a regular cylinder of periosteum at the lower extremity of the shaft of the humerus; and in this instance, also, the results were extremely satisfactory.—*Lancet, Dec.* 10, 1859, *p.* 591.

58.—*Forcible Flexure in Anchylosis.*—Since the attention of the profession has been drawn to the subject of the treatment of false anchylosis by forcible rupture of the uniting medium, through the labours of Mr. Brodhurst in this interesting field of research, we have noticed very many attempts to treat such cases at the different hospitals, and mostly with success, when the union has not been osseous. Several examples of the kind have been recorded in our 'Mirror.' Two lately occurred at St. George's Hospital, in which this plan of treatment has been followed by the most satisfactory results. The first was a boy who had broken off the outer condyle of his right arm four months ago. This united, with the arm remaining in a faulty position from a false anchylosis; pronation and supination were perfect, but a stiff joint remained. Under chloroform, Mr. Cæsar Hawkins readily broke up the uniting medium, and the fullest flexion and extension were obtained, and this has continued without an untoward symptom. The other case was that of a woman aged fifty-five years, who fell on her hand three months and a half ago; fractured the lower part of the radius, and injured the elbow, partially dislocating the ulna backwards. The result was union of the fracture and anchylosis of the elbow-joint, without the slightest motion, the arm being, moreover, almost straight, and to all intents useless. She was seen by Mr. Prescott Hewett three weeks before admission, and it seemed a case suitable for forcible flexion. She was given chloroform, on the 12th of November, and on firmly laying hold of the forearm, Mr. Hewett readily broke up the adhesions, and flexed the arm with the greatest ease, restoring the normal position, without being accompanied by the slightest rigidity. This patient is now doing remarkably well.

The success which has attended this plan of treatment in various other joints besides the elbow, in the hands of Mr. Brodhurst and numerous other surgeons, entitles it to every consideration.—*Lancet, Nov.* 26, 1859, *p.* 536.

59.—*Manufactured Pasteboard Splints.*—[At a meeting of the Royal Medico-Chirurgical Society, Mr. Acton exhibited some pasteboard splints, such as were used extensively by the Russians at Sebastopol. Their inventor is M. Merchie, chief surgeon to the Military Hospital at Brussels.]

They are exceedingly light, firm, and inexpensive, being manufactured in pasteboard or *papier maché.* When wet, the substance is moulded to the form of the thigh, leg, arm, or forearm. These portable dry splints retain the proper shape of the limb. In case of fracture occurring, half a splint is applied to the limb, a little cotton wool only intervening between the skin and the pasteboard. The other half-splint is then placed on the opposite side of the fractured limb, and the whole enveloped in a bandage. The inventor had demonstrated, on the field of battle, that, as soon as these splints were applied, a man could be removed to the rear, and transported long distances, without danger or pain, in the usual conveyances.

[These splints must not be confounded with splints made by moulding cardboard to the injured limb; they are made by moulding pulped paper to shapes suitable to every size of limb, then dried, and kept ready made for use.]—*Lancet, Dec.* 3, 1859, *p.* 564.

ORGANS OF CIRCULATION.

60.—ON ACUPRESSURE—A NEW METHOD OF ARRESTING SURGICAL HEMORRHAGE.

By J. Y. Simpson, M.D., Professor of Midwifery in the University of Edinburgh, &c.

[At the first winter meeting of the Royal Society at Edinburgh, in December, 1859, Professor Simpson made a lengthened communication on acupressure, a new mode of arresting surgical hemorrhage. After passing in review the various means employed in arresting surgical hemorrhage, and observing that ligatures, however employed (even metallic) always act as foreign bodies, and excite higher stages of inflammation than the adhesive, Dr. Simpson said, that what is wanted in plastic surgical operations in order to ensure union by the first intention, is, to arrest the hemorrhage effectually, and yet not leave any foreign body whatever in the wound. This end is attained by acupressure.]

Dr. Simpson stated that he had tested the effects of acupressure as a means of effectually closing arteries and staunching hemorrhage first upon the lower animals, and lately on two or three operations on the human subject. The instruments which he proposed should be used for the purpose were slender needles or pins of passive iron, headed with wax or glass, and in other respects also like the hare-lip needles commonly used by surgeons at the present day, but longer when circumstances require it. They might be coated with silver or zinc on the surface, if such protection were deemed requisite.

At first, Dr. Simpson believed that in using acupressure as a hemostatic means, it would be necessary to compress the tube of the bleeding artery between two needles, one placed on either side of it. But in his later experiments upon the living as well as the dead body (as in amputations on the latter, and subsequently injecting tepid water through the arteries, in imitation of the flow of blood), he had found, that the compression of one needle was usually perfectly sufficient to shut up an artery, and that even sometimes, when two or more bleeding points were near, they could be closed simultaneously by the action of one needle or pin. The whole process consists in passing the needle *twice* through the substance of the wound, so as to compress together and close, by the middle portion of the needle, the tube of the bleeding artery a line or two, or more, on the cardiac side of the bleeding point. The only part of the needle necessarily left exposed in the fresh surface of the wound is the small middle portion of it, which passes over and compresses the arterial tube; and the whole needle is withdrawn on the second or third day, or as soon as the artery is supposed to be adequately closed, thus leaving *nothing* whatever in the shape of a foreign body within the wound, or in the tissues composing its sides or flaps. To produce adequate closing pressure upon any arterial tube which it is desired to constrict, the needle must be passed over it so as to compress the tube with sufficient power and force against some resisting body. Such a resisting body will be most frequently found, 1st. in the cutaneous walls and component tissues of the wound; 2nd, sometimes in a neighbouring bone, against which the artery may be pinned and compressed by the acupressure needle; and, 3rd, in a few rare cases it may possibly be found in practice, that a second needle may require to be introduced to serve as a point against which the required compression is to be made. Most commonly the first of these three plans seems perfectly sufficient, and that even in amputation of the thigh. In acting upon this mode, the surgeon may place the tip of the fore-finger of his left hand upon the bleeding mouth of the artery which he intends to compress and close; holding the needle in his right hand, he passes it through the *cutaneous* surface of the flap, and pushes it inwards till its point project out to the extent of a few lines, on the raw surface of the wound, a little to the right of, and anterior to his finger-tip; he then, by the actions of his right hand upon the head of the needle, turns and directs the needle, so that it makes a bridge as it were *across* the site of the tube of the bleeding artery immediately in front of the point of the finger, with which he is shutting up its orifice; he next, either with this same fore-finger of the left hand, or with the side of the end of the needle itself, compresses the locality of the bleeding arterial orifice and tube, and then pushes on the needle with his right hand so as to make it *re-enter* the surface of the wound a little to the left side of the artery; and lastly, by pressing the needle further on in this direction, its point re-emerges through the *cutaneous* surface of the flap—and the site of the tube of the bleeding artery is in this way left pinned down in a compressed state by the arc or bridge of steel that is passed over it. The needle thus passes first from and through the skin of the flap *inwards* to the raw surface of the wound, and after bridging over the site of the artery, it passes secondly from the raw surface of the wound *outwards* again to and through the skin. Sometimes the needle will be best passed by the aid of the eye alone, and without guiding its course by the finger-tip applied to the bleeding orifice. It compresses not the arterial tube alone, but the structures also placed over and around the *site* of the tube. When the needle is completely adjusted, all of it that is seen on the surface of the raw wound, and that not necessarily so, is the small portion of it passing over the site of the artery, while externally, upon the cutaneous surface of the flap, we have remaining exposed more or less of its two extremities, namely, its point and its head. The rest of it is hidden in the structures of the flap or side of the wound. The degree of pressure required to close effectually the tube of an artery is certainly much less than medical practitioners generally imagine; but in the above proceeding the amount of pressure can be regulated and increased, when required, by the acuteness of the angle at which the needle is introduced and again passed out,—the cutaneous and other structures of the flap serving as the resisting medium against which the needle compresses the arterial tube. But if it were ever, perchance, necessary to produce greater compression than can be thus accomplished by the needle alone, this increased pressure could be readily obtained by throwing around the two extremities of the needle exposed cutaneously a figure-of-eight ligature, as in hare-lip, with or without a small compress placed between the arc of the ligature and the skin. The process of the adjustment of the needle is difficult to describe shortly by words, but the whole of it is readily seen and imitated when repeated upon a piece of cloth or leather. We fasten the stalk of a flower in the lapelle of our coat by a pin passed exactly in this manner. To compress a bleeding artery against a bone is somewhat more complicated, but not much so. In accomplishing it, we have to introduce from the cutaneous surface a long needle through the flap of the wound obliquely to near the site of the artery, and then compressing, with the fingers of the other hand, or with the end of the needle, the part containing the artery against the bone, we make the needle, after passing over this compressed part, and after testing whether it has closed the vessel or not, enter into the tissues beyond, and if necessary even emerge from, the cutaneous surface on the other side at an angle somewhat oblique to that at which it entered; thus taking advantage of the resiliency and resistance of the soft textures to make them push the needle with the necessary degree of compression against the artery and bone. Arteries in particular parts require special adjustments and modifications to compress them against the neighbouring bone, which only experience can point out. There is always sufficient soft tissue on either side of the artery for the needle to get a purchase upon, to compress the arterial tube against the bone or other resistant point. In two cases, Dr. S. had found that branch of the internal mammary artery which so frequently bleeds in the bottom of the wound after excision of the mamma, easily and perfectly closed by a needle passed through the flap to near the artery, then lifted over it (and after compressing it so as to stop the flow of blood) pushed onwards into the tissues beyond. Possibly, in some amputations, an acupressure needle or needles may yet be passed, immediately before the operation, half an inch or so above the proposed site of the amputation line, so as to shut the principal artery or arteries, and render the operation comparatively bloodless. If so, these needles would serve, at one and the same time, the present use of both tourniquet and arterial ligatures. Perhaps this will be found, in some cases, a simple and effectual means of compressing and closing the artery leading to an aneurism—as the femoral artery, for example, in popliteal aneurism,—changing the operation for that disease into a simple process of acupuncture instead of a process of delicate dissection and deligation, when in any case the milder methods of compression, manipulation, and continuous flexion of the limbs fail. It has been hitherto a difficult problem to obstruct the vessels of the ovarian ligament in ovariotomy, without leaving a foreign body, whether clamp or ligature, upon the stalk of the tumour, to ulcerate and slough through it. If the stalk be transfixed and pinned in its whole breadth to the interior of the relaxed abdominal walls, by one or more acupressure needles passed through these abdominal walls from without, this difficulty may possibly be overcome.

That needles used for the purpose of acupressure, and passed freely through the walls and flaps of wounds will not be attended by any great degree of disturbance or irritation, is rendered in the highest degree probable by all that we know of the tolerance of living animal tissues to the contact of metallic bodies. Long ago, John Hunter pointed out that small-shot, needles, pins, &c., when passed into and imbedded in the living body, seldom or never produced any inflammatory action, or none at least beyond the stage of adhesive inflammation, even when lodged for years. Some time ago, when the subject of acupuncture specially attracted the attention of medical men, Cloquet, Pelletan, Pouillet, and others, showed that the passage and retention of long acupuncture needles was attended with little or no irritation in the implicated living tissues. The Reviewer of their works and experiments in the 'Edinburgh Medical Journal' for 1827, observes: "It is a remarkable circumstance that the acupuncture needles never cause inflammation in their neighbourhood. If they are rudely handled or ruffled by the clothes of the patient, they may produce a little irritation; but if they are properly secured and protected, they may be left in the body for an *indefinite* length of time without causing any of the effects which usually arise on account of the presence of foreign bodies. In one of M. Cloquet's patients they were left in the temples for eighteen days; and in cases in which needles have been swallowed, they have remained without causing inflammation for a much longer period. It appears probable, from the facts collected on the subject, that metallic bodies of every kind may remain imbedded in the animal tissues without being productive of injuries." All the late observations and experiments upon metallic sutures are confirmatory of the same great pathological law of the tolerance of living tissues for the contact of metallic bodies imbedded within their substance. In the operation for hare-lip, where the whole success or failure of the operation depends on the establishment or not of union by the first intention, surgeons use needles to keep the lips of the wound approximated, often compressing these needles strongly with their figure-of-eight ligatures, and find this measure the most successful means which they can adopt for accomplishing primary adhesion.

The acupressure of arteries, when compared with the ligature of them, appears, as a means of arresting hemorrhage, to present various important advantages:—1st. It will be found more easy, simple, and expeditious in its application than the ligature. 2nd. The needles in acupressure can scarcely be considered as foreign bodies in the wound, and may always be entirely removed in two or three days, or as soon as the artery is considered closed; whilst the ligatures are true foreign bodies, and cannot be removed till they have ulcerated through the tied vessels. 3rd. The ligature inevitably produces ulceration, suppuration, and gangrene at each arterial point at which it is applied; whilst the closure of arterial tubes by acupressure is not attended by any such severe consequences. 4th. The chances, therefore, of the union of wounds by the first intention should be greater under the arrestment of surgical hemorrhage by acupressure than the ligature. 5th. Pyæmia and surgical fever seem not unfrequently to be excited by the unhealthy suppuration, &c., in wounds which are liable to be set up by the presence and irritation of ligatures. 6th. These dangerous and fatal complications are less likely to be excited by the employment of acupressure, seeing the presence of a metallic needle has not the tendency to create local suppurations and sloughs in the wound, such as occur at the seats of arterial ligatures. And 7th. Hence, under the use of acupressure, we are entitled to expect both, *first*, that surgical wounds will heal more kindly and close more speedily; and *secondly*, that surgical operations and injuries will be less frequently attended than at present by surgical fever and pyæmia.—*Edinburgh Med. Journal, Jan.* 1860, *p.* 647.

[In a subsequent communication in the 'Medical Times and Gazette, on the subject of Acupressure, Dr. Simpson continues :—]

The great practical question, however, is—granting that acupressure by metallic needles is simpler and safer than the silken ligature—Is it actually sufficient for the closure of the arteries usually divided in surgical operations? Before publishing on the matter I had experimented on the subject in the lower animals, and had shut up easily by it the carotid artery of the horse—the largest living vessel which it was within my power to experiment upon. I had made various experiments with it, in the way of amputation and other experiments, on the dead human subject—imitating the flow of its head. The rest of it is hidden in the structures of the flap or side of the wound. The degree of pressure required to close effectually the tube of an artery is certainly much less than medical practitioners generally imagine; but in the above proceeding the amount of pressure can be regulated and increased, when required, by the acuteness of the angle at which the needle is introduced and again passed out,—the cutaneous and other structures of the flap serving as the resisting medium against which the needle compresses the arterial tube. But if it were ever, perchance, necessary to produce greater compression than can be thus accomplished by the needle alone, this increased pressure could be readily obtained by throwing around the two extremities of the needle exposed cutaneously a figure-of-eight ligature, as in hare-lip, with or without a small compress placed between the arc of the ligature and the skin. The process of the adjustment of the needle is difficult to describe shortly by words, but the whole of it is readily seen and imitated when repeated upon a piece of cloth or leather. We fasten the stalk of a flower in the lapelle of our coat by a pin passed exactly in this manner. To compress a bleeding artery against a bone is somewhat more complicated, but not much so. In accomplishing it, we have to introduce from the cutaneous surface a long needle through the flap of the wound obliquely to near the site of the artery, and then compressing, with the fingers of the other hand, or with the end of the needle, the part containing the artery against the bone, we make the needle, after passing over this compressed part, and after testing whether it has closed the vessel or not, enter into the tissues beyond, and if necessary even emerge from, the cutaneous surface on the other side at an angle somewhat oblique to that at which it entered; thus taking advantage of the resiliency and resistance of the soft textures to make them push the needle with the necessary degree of compression against the artery and bone. Arteries in particular parts require special adjustments and modifications to compress them against the neighbouring bone, which only experience can point out. There is always sufficient soft tissue on either side of the artery for the needle to get a purchase upon, to compress the arterial tube against the bone or other resistant point. In two cases, Dr. S. had found that branch of the internal mammary artery which so frequently bleeds in the bottom of the wound after excision of the mamma, easily and perfectly closed by a needle passed through the flap to near the artery, then lifted over it (and after compressing it so as to stop the flow of blood) pushed onwards into the tissues beyond. Possibly, in some amputations, an acupressure needle or needles may yet be passed, immediately before the operation, half an inch or so above the proposed site of the amputation line, so as to shut the principal artery or arteries, and render the operation comparatively bloodless. If so, these needles would serve, at one and the same time, the present use of both tourniquet and arterial ligatures. Perhaps this will be found, in some cases, a simple and effectual means of compressing and closing the artery leading to an aneurism—as the femoral artery, for example, in popliteal aneurism,—changing the operation for that disease into a simple process of acupuncture instead of a process of delicate dissection and deligation, when in any case the milder methods of compression, manipulation, and continuous flexion of the limbs fail. It has been hitherto a difficult problem to obstruct the vessels of the ovarian ligament in ovariotomy, without leaving a foreign body, whether clamp or ligature, upon the stalk of the tumour, to ulcerate and slough through it. If the stalk be transfixed and pinned in its whole breadth to the interior of the relaxed abdominal walls, by one or more acupressure needles passed through these abdominal walls from without, this difficulty may possibly be overcome.

That needles used for the purpose of acupressure, and passed freely through the walls and flaps of wounds will not be attended by any great degree of disturbance or irritation, is rendered in the highest degree probable by all that we know of the tolerance of living animal tissues to the contact of metallic bodies. Long ago, John Hunter pointed out that small-shot, needles, pins, &c., when passed into and imbedded in the living body, seldom or never produced any inflammatory action, or none at least beyond the stage of adhesive inflammation, even when lodged for years. Some time ago, when the subject of acupuncture specially attracted the attention of medical men, Cloquet, Pelletan, Pouillet, and others, showed that the passage and retention of long acupuncture needles was attended with little or no irritation in the implicated living tissues. The Reviewer of their works and experiments in the 'Edinburgh Medical Journal' for 1827, observes: "It is a remarkable circumstance that the acupuncture needles never cause inflammation in their neighbourhood. If they are rudely handled or ruffled by the clothes of the patient, they may produce a little irritation; but if they are properly secured and protected, they may be left in the body for an *indefinite* length of time without causing any of the effects which usually arise on account of the presence of foreign bodies. In one of M. Cloquet's patients they were left in the temples for eighteen days; and in cases in which needles have been swallowed, they have remained without causing inflammation for a much longer period. It appears probable, from the facts collected on the subject, that metallic bodies of every kind may remain imbedded in the animal tissues without being productive of injuries." All the late observations and experiments upon metallic sutures are confirmatory of the same great pathological law of the tolerance of living tissues for the contact of metallic bodies imbedded within their substance. In the operation for hare-lip, where the whole success or failure of the operation depends on the establishment or not of union by the first intention, surgeons use needles to keep the lips of the wound approximated, often compressing these needles strongly with their figure-of-eight ligatures, and find this measure the most successful means which they can adopt for accomplishing primary adhesion.

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For the first application of acupressure to the arrestment of hemorrhage after amputation of the limbs, I am indebted to one of the most accomplished and advanced surgeons in this country—my friend Dr. Greig, of Dundee. Formerly as surgeon to our army in the East, and latterly as surgeon to the large hospital at Dundee, Dr. Greig has enjoyed great opportunities as an operator. His very interesting letters to me on the subject of acupressure indicate the change and struggle which every earnest and ingenious mind has in setting aside old-established and cherished practices for the adoption of what is new. When I first took the liberty of directing his attention to the subject, as contained in an abstract of a paper on it in the January number of the 'Edinburgh Medical Journal,' and asked him to be so good as to test the plan, he wrote me, January 8, 1860, that he could "see no great difficulty in giving the thing a fair trial." But he adds:—"Of its general adoption I have great doubts. We have been always taught to look upon the ligature as the only true means of arresting hemorrhage. and this feeling is somewhat difficult to get out of one's mind. Your illustration of fixing a flower in the lapel of the coat by means of a pin, explains the whole thing."

Case 1.—Two days afterwards, January 10, Dr. Greig wrote me:—"I performed amputation at the forearm this afternoon in a case of laceration of the hand from the bursting of a gun, and I used the needle instead of ligatures for arresting hemorrhage. Both the radial and ulnar arteries bled freely, but were easily controlled by a needle placed on each, almost half-an-inch above the cut end. Both needles were, of course, in the palmar or anterior flap, and were applied *quite as easily as a ligature.*" These last words are underscored in Dr. Greig's letter, and show that thus the very first trial of acupressure proved as easy as deligation in the hands of a surgeon who for years had been in the constant practice and habit of applying ligature to arteries for the staunching of hemorrhage in his operations.

Case 2.—Three days afterwards, January 13, Dr. Greig again wrote as follows:—"I have had another amputation at the middle of the forearm today, and used acupressure with ease and success. The process, so far as I have tried it is *the simplest* one can imagine; and, unless I see some good reason for changing my mind, it must ultimately come into universal adoption. It is really surprising how very little pressure is required to stop bleeding from an artery. In fact, I had no idea of it till I tried acupressure."

On January 20, Dr. Greig writes:—"Both the cases of amputation in which I used acupressure have done remarkably well. There has been less irritation and less suppuration, and the wounds are healing more kindly than had ligatures been employed. The first case did not close by the first intention, owing to part of the anterior flap having been lacerated by the explosion. The second has gone on as well as could be wished—no fever, no irritation—and the wound is healing by the first intention. What surprises me more than anything else is, the very small amount of pressure which is required to stop arterial hemorrhage. In passing the needle over an artery I do not think it will be found necessary to turn it sharply over the vessel, thereby binding it very tightly to the flap. Such a degree of pressure is by no means required. Less irritation is caused by passing the needle more lightly across the artery, and taking in more tissue along with it."

"It is a great comfort also," Dr. Greig adds, "to both patient and surgeon, that by acupressure the artery is closed in about forty-eight hours (a large artery may, of course, require a longer time), and all cause of irritation at once removed. In my first case I allowed the needles to remain in for three days; but in future I will consider two days long enough; and, for all I know, perhaps it is longer than is required."

"I have now the greatest faith in acupressure. I intend employing it in all kinds of cases that may come under my care, and I will have no fear whatever to use it in my first thigh amputation."

"In giving directions for securing the vessel, you advise the surgeon to place the forefinger over its bleeding mouth, &c. Now you will find it much better when you have a flap to keep the finger of the left hand on the skin side and use the thumb. You feel the vessel beating between the thumb and forefinger, and you can introduce the needle in the dark."

I heard again from Dr. Greig on January 23. "The amputations," he states, "are doing well, and both patients are walking about the wards." "Yesterday," he continues, "at a case of removal of mamma, I again used the needles, and easily arrested the hemorrhage from two arterial branches which were spouting freely in the upper or axiliary flap. A small branch of an intercostal was the only other bleeding vessel, and torsion was used for it. Nothing could have been easier or more beautiful than acupressure applied in this case, as the procedure was seen in its simplest form—more so than in a flap." "I see," Dr. Greig adds, "that in France M. Foucher has tried acupressure on the dead subject, and also on a dog. I wonder why he did not try it in an amputation. Nothing can be easier, and if a surgeon uses it once I am sure he will do so again.—*Edin. Med. Journal, Jan.* 1860, *p.* 647, *and Med. Times and Gazette, Feb.* 11, 1860, *p.* 137.

61.—ON ACUPRESSURE.

[A writer in the 'Med. Times and Gazette,' singing himself A. M., observes that in none of the cases related by Dr. Simpson, in support of the use of acupressure in amputations, did complete reunion by the first intention take place. In other words, the object of these appliances has not been fully attained.]

Some of the causes of this failure seem to me clear.

There is, first, the application of towels, lint, sponges, &c., to the raw surfaces, by which they are covered with myriads of small filaments or foreign bodies, each acting as a centre of irritation, and demanding ejection from the wound, and often, in the aggregate, constituting a more formidable evil than two or three silk or cotton ligatures. The experience of every housemaid in cleaning and drying crystal, will illustrate and prove what I say; but direct proof can be found in the microscopic examination of the early discharge from a wound so handled.

The saw is a further source of dead or foreign bodies, the flaps being frequently, if not generally, dusted over with powdered bone.

The second cause of failure, I think, arises from the application of warm or cold water to the bleeding surfaces. In this I may be in error, though I believe I have had proof of the soundness of the conclusion. Water thus applied causes the death, if I may so speak, of a certain amount of blood, which then lies in the wound as a thing to be removed; or, stating it differently, the use of the water induces such changes in the blood oozing from the cut surfaces, as renders it unfit to undergo those changes through which it should pass in the process of healing or union, or, in other words, unfit to serve those purposes which it is intended to serve.

The employment of the needles in arresting the hemorrhage makes the use of towels, sponges, and water, all but unnecessary, as the bleeding mouths of vessels have not to be sought for, as is the case when ligatures are used.—*Med. Times and Gazette, March* 3, 1860, *p.* 225.

62.—*On Acupressure.*—Acupressure, is fast coming into use among surgeons. In Mr. ADAMS' case, which we noticed last week, the superiority of the needle over the ligature has been made most apparent. The needle was removed forty-eight hours after the operation, and there was not a sign of bleeding or suppuration; while the ligature has remained as usual, acting as a seaton during the sloughing process it sets up. It should be observed that no part of the needle was left exposed on the raw surface of the wound; it was introduced half-an-inch above the cut point. This is a great advantage in amputation; for the cut surfaces can be brought into apposition free at every point from the contact of any foreign body. Our provincial hospital surgeons are taking up the method in a most creditable manner. At Dundee, Carlisle, Greenock, and Liverpool, it has been applied with great success; and now that Mr Adams has led the way in the metropolis, at a small hospital, his example will be followed sooner or later in the larger establishments where the surgery partakes more of the character of stereotype. Mr. Bickersteth, of Liverpool, writes most warmly as to the facility of acupressure and its probable great results.—*Med. Times and Gazette, Feb.* 25, 1860, *p.* 194.

63.—*On Acupressure.* By HENRY T. TAYLOR, Esq., Guildford. Having observed with much interest the success of Dr. Simpson's plan of acupressure in securing the vessels in amputations, I am led to think that the same principle might be applied to the deligation of arteries for aneurism or secondary hemorrhage, some modification being made in the form of the compressor, so that it should act exclusively on the vessel, and admit of easy withdrawal. The following simple means have occurred to me as likely to effect this purpose.

Having laid bare the carotid of a sheep, I passed under it a common bodkin of the kind used by sempstresses; into the long eye of this I inserted a hare-lip pin, having first removed the point, and bending it down over the artery, I tied the ends of the pin and bodkin firmly together with a few turns of waxed thread. The pressure thus made arrested all pulsation in the vessel above the point included. After forty-eight hours I removed the compressor easily, cutting the thread and drawing the pin out first. On the next day, when the animal was killed, I found the artery firmly plugged with a coagulum of the usual form.

This operation will remind many of your readers of old attempts to obtain closure of the arterial tube by means of Assalini's compressor and the *presse-artère* of Dessault and Deschamps; but it is less open to the objections urged against the use of those instruments, as it can be performed with less disturbance of the vessel, while the presence of so small a foreign body excites so much less irritation in the wound. It is worthy of consideration whether these plans were not too hastily abandoned, which possessed this advantage over the ligature—security from the dangers of secondary hemorrhage, where the coats of the vessel are unsound, or where the proximity of a collateral branch prevents the formation of a clot on the distal side of the ligature.—*Med. Times and Gazette, March* 24, 1860, *p.* 301.

64.—*Hemorrhage from the Tonsils after Incision.—Arrest by the Perchloride of Iron.* (Case under the care of Mr. HENRY THOMPSON, Surgeon to the Marylebone Infirmary.)—[A charwoman, aged 35, was admitted into the Marylebone Infirmary, suffering from acute tonsilitis. Two days previously a surgeon had punctured one tonsil in the hope of letting out pus, and on the following day had punctured the other tonsil; but in neither case was any pus evacuated, though there was free bleeding from both, which, however, soon ceased. On the evening of her admission, however, hemorrhage commenced from the right side, and continued without intermission for nearly two hours, by which time she had altogether lost about four pints, and the patient was pale and almost pulseless. The small opening in the right gland could be felt distinctly with the finger.]

The woman's condition being such as to admit of no delay, Mr. Thompson at once prepared to place a ligature on the carotid, and the needful instruments were got in readiness. Before doing so, however, he determined, though without much hope of being successful, to make a trial of the perchloride of iron. This was done by carefully mopping the part dry by means of lint wrapped on the left forefinger, and then pushing a glass-brush dipped in the fluid into the incision. After a few applications the bleeding was entirely stopped; in about a quarter of an hour, however, a slight recurrence necessitated a repetition of them, which were again successful. No return took place afterwards. The woman slowly regained strength, and made ultimately a complete recovery.

Mr. Thompson stated that, although he had on several previous occasions easily arrested very formidable hemorrhages by resort to the perchloride solution, yet that on the present occasion he had not at all expected to be able to do so. He spoke most strongly as to the value of this agent when properly prepared according to the French codex.

[Of course, from the success of the treatment. it is all but certain that the carotid could not have been wounded.]—*Med Times and Gazette, Dec.* 24, 1860, *p.* 631.

65.—*The Effects of the Perchloride of Iron.*—The influence of this powerful styptic was well shown in the arrest of dangerous hemorrhage from the tonsils, in a case which we noticed in our 'Mirror' of the 19th November last, under Mr. Henry Thompson's care at the Marylebone Infirmary. We have seen it used at University College Hospital for the coagulation of the blood in varicose veins, but in one instance it produced suppuration over the vein and much constitutional disturbance. It has been employed by Mr, Prescott Hewett, at St. George's Hospital, upon a mass of veins situated on the inner ankle of a young woman, with partial benefit; but the veins were large, and difficult to reduce either in number or bulk. In the same hospital we saw, on the 22nd ult., an infant who had a large congenital nævus of the left cheek. This had been treated by the injection of the perchloride of iron by a general practitioner out of the hospital, but with the effect of producing the most intense inflammation, and sloughing of the central portion of the nævus right down to the mucous membrane. Mr. Hewett has completely obliterated all the vessels of this nævus by a series of operations, which consisted in the passage of needles through its various parts, for the purpose of pressure and strangulation. The cure has been a tedious one, but nevertheless satisfactory.

The perchloride of iron would seem to be beneficial in some cases when it is introduced subcutaneously, but in others it sets up some amount of inflammation, which may so impair the vitality of the parts as to produce actual destruction of tissue, as occurred in the last-mentioned instance. It requires to be used with care.—*Lancet, Jan.* 21, 1860, *p.* 63.

66.—*Collodion in Arrest of Hemorrhage from Leech-bites.*—M. Stanislas Martin observes that a few layers of collodion promptly arrests the bleeding from leech-bites.—*Med. Times and Gazette, April* 14. 1860, *p.* 377.

67.—ON THE TREATMENT OF VARICOSE VEINS OF THE LEGS, AND OF VARICOCELE.

By John Erichsen, Esq., Professor of Surgery and of Clinical Surgery in University College, &c.

[The danger of operating in varix is slight, if the veins are not opened. If the veins are opened, air is admitted into the wound, and the chances of suppuration, instead of adhesive inflammation are much increased.]

There are many modes of treatment in varix, all having one object in view—viz., that of causing occlusion of the vein by the adhesive inflammation. But, in my opinion, all measures should be avoided which include opening the vein, whether this be done by caustic or by the knife, as they are extremely dangerous, by leading to suppuration within; so, also, are those plans of treatment by which we break down the exuded lymph and coagula, or open up the vein even when occluded.

I shall not enumerate all the methods before the profession for the radical cure of varix, but proceed to describe those which I have for many years successfully employed. The plan I ordinarily pursue is as follows:—A hare-lip pin is passed underneath the vein on one side, and its point brought out on the other, a piece of elastic bougie, about an inch in length, is then laid over the vein parallel to its course. Then, by means of a silken thread twisted over the bougie and under the two ends of the pin, the vein is compressed between the pin and bougie. In performing this operation, there are one or two little points to be observed. The first is to be careful not to transfix the vein with the pin: if the vein be opened, and the pin lie across it, there is danger of suppuration, as the pin acts as a kind of seton in the vein. This inadvertence is avoided by dipping the pin deeply, whilst passing it under the vein. In this way there is no risk of piercing the vessel If a drop or two of venous blood exude, by the side of the pin, through the puncture, the vein has been perforated, and the instrument should be withdrawn, and passed again. If the vein is unharmed, the operation is bloodless. The second point is, that the ligature should not be so tight as to cause ulceration by strangulation of the part compressed. If the thread be moderately tight only, and the pin made, as it generally now is, of unoxidisable iron, it is quite passive, and does not rust, hence excessive irritation is avoided. The bougie and pin should remain about ten days, at the end of which time the vein is converted into an impervious cord of plastic matter and coagulum. When this is attained, the pin may be removed, the limb bandaged, and the patient may leave his bed.

It has been objected to this and to all other operations for varix, that the cure is not permanent; that the varicose condition is apt to return; that the same veins perhaps are not affected again in this manner, but that others speedily assume a dilated and tortuous state. No doubt this is the result of some instances, but in many cases which I have had an opportunity of examining years afterwards, the cure has been permanent; and, in the meantime, the object for which the operation was undertaken is served.

Another method which is much used in France, and which I have occasionally employed myself in this hospital for some years past, is that of injecting a small quantity of a solution of the perchloride of iron, of a specific strength, into the veins, by means of Pravaz's screw-syringe. In this way the blood contained in the dilated vessel is made to coagulate, and thus the passage through the vessel is occluded. The adhesive inflammation at the same time being excited, permanent obstruction is attained, and a cure effected. This, though a valuable means when the vessels are knotted and sacculated, is not, I believe, so good a one as the pin and ligature, because I have seen it followed, in two or three of the cases in which I have had occasion to use it, by circumscribed abscesses, and even sloughing of the adjacent parts, though no fatal result has, as yet, occurred in my practice. On this account, I consider this mode of treatment undoubtedly more dangerous; and I think it ought to be confined to the cure of those cases only where the knots are so large, and so closely matted together, that the pin cannot be passed underneath them.

The next distribution of veins liable to varix is that formed by the spermatic plexus. Dilatation of these vessels—varicocele—is often met with in young men; and much benefit can be afforded by palliative means, such as supporting or compressing the tumour in various ways; for instance, by raising the scrotum in a suspensory bandage, or by wearing a mocmain truss, &c., and one or other of these contrivances generally gives sufficient relief for the patient's comfort. But I have found it necessary to have recourse to more active measures than these, and to adopt operative treatment in three of the cases which have presented themselves during the present session. The circumstances for which operation may be and has been practised, in these and similar cases, can be arranged in the four following categories:

1. The existence of a varicocele disqualifies the sufferer from admission into the public services. This, in my opinion, is a perfectly legitimate reason for operating. One of the cases on whom I recently effected a radical cure, was that of a man in the prime of life, who, wishing to enlist in the Marines, was refused solely on the ground of having a small varicocele. This I cured by operation, and the man afterwards entered the service.

2. In cases in which the presence of a varicocele of inordinate size causes a distressing sense of weight and pain in the loins and groins, and often inability to stand or walk for any length of time, in these cases, when the patient is in continual discomfort, or more or less prevented from pursuing his ordinary avocations, in fact, quite crippled, it is perfectly justifiable to resort to operation.

3. When atrophy of the testicle is a consequence of the pressure of the blood in the veins.

4. In cases, also, where the pressure of the enlarged veins on the spermatic nerves produces repeated attacks of spermatorrhœa; and these cases are by no means uncommon. These are, however, more frequently met with out of the hospital than in individuals of the class who apply to such institutions for relief. In fact, young men of the more highly-educated classes are very subject to it, especially those who habitually lead a sedentary and studious life, as, for instance, young clergymen and lawyers. In these persons a peculiarly hypochondriacal state is brought on by the tendency of the mind to dwell on the condition of the genital organs, and the patient is constantly fidgetting about the local and tangible disease he observes in them. This was the case in the patient on whom I operated a short time ago for double varicocele, and who has received a better education than most hospital patients, for he belongs, in some degree, to the medical profession. His anxiety with regard to this disease, though the veins affected have been perfectly occluded by the method presently to be described, is still so great that, at his earnest importunity, I removed, last week, a portion of the scrotum which happened to be rather more pendulous than is usual, partly in order better to support the testis, and partly that his morbid feeling on this subject might in some degree be assuaged.

Now, how should the radical cure of this condition be produced? To this I would answer—By exciting adhesive inflammation of the spermatic veins, through an application of the same principle which sets up that process in the veins of the lower extremity. There are several different ways of doing this; some are very objectionable. The twisted suture, as applied to the veins of the leg, induces too great irritation in the scrotum, and there its introduction is often followed by violent inflammation or sloughing, or by an opening up of the cellular tissue of the scrotum with œdema, and even purulent infiltration. It is better, I think, not to use this method here, indeed I have twice, in the practice of others, seen it followed by death. The plan I have adopted for some years, is that suggested and practised by Vidal, a distinguished French surgeon, and is as follows:—The vas deferens, readily distinguished by its round cord-like feel, is first separated from the veins, and entrusted to an assistant; next an iron pin, bored with a hole at each end, is passed between the vas and the veins, and brought out, first notching the scrotum with a scalpel at the point of perforation; then a silver-wire, threaded on a needle so constructed that the wire shall follow it without catching, is passed in at the aperture of entry of the needle, and then carried between the integument of the scrotum and the veins, the wire is brought out at the second puncture. Each end of the wire is now passed through the corresponding hole of the pin, which is twisted round and round repeatedly, each turn causing the wire to be rolled around the pin, and so tightened till the veins are firmly compressed between the pin behind and the loop of wire in front. By this means the scrotum is quite free and uncompressed, and there is no danger of arousing inflammation or œdema. The wire should be tightened from day to day, as it causes ulceration in the veins until it has completely cut through, which results, usually, in about a week or ten days. Meanwhile there is much plastic matter thrown out around the veins, this finally contracts, and obliterates their channels. This method is an effectual and permanent cure, as we had an opportunity of seeing in the case of a porter at this hospital, on whom I performed this operation with perfect success, for he remained here for three years after the operation, during which time he was perfectly free from any return of his disease. Of late, I have been in the habit of employing a simpler method. I separate the vas in the usual way, and then make a small incision, about half an inch long, in the front and back of the scrotum, afterwards passing a needle armed with silver-wire, as before described, between the vas and the veins, bringing it out behind, then returning the needle, but this time carrying it in front, between the veins and the skin, and so including the veins in a loop of wire, without implicating the scrotum. This is then tightly twisted together, so as to constrict the inclosed vessels. This plan had a similar effect to that of the wire and pin combined; by repeated tightening the wire gradually effected a passage, by ulceration, through the veins, which were obliterated by the same process.

It has been objected to this and similar operations, that atrophy of the testis may take place from its arterial branches being included together with the veins, but as the spermatic artery runs near to the vas deferens, and is held out of the way with that duct, it escapes, and the chance of that mischief is avoided. Nevertheless, atrophy of the testis may coexist, as the result of long-continued pressure of the blood in the vessels of the gland before the operation was performed.—*British Med. Journal*, *Feb.* 25, 1860, *p.* 141.

ORGANS OF RESPIRATION.

68.—ON A CONVENIENT INSTRUMENT FOR EXAMINING THE BASE OF THE TONGUE AND EPIGLOTTIS.

By P. C. Price, Esq., M.R.C.S., Surgeon to the Great Northern Hospital.

In cases of disease of the base of the tongue and parts immediately in relation with the epiglottis, much difficulty is often experienced in endeavouring to obtain a clear view of the parts involved, while it is often impossible even to catch a glimpse of the morbid condition. When such is the case, the surgeon is compelled to remain content with the conclusion at which he may arrive by the symptoms exhibited. To facilitate the examination of the upper part of the throat, I am in the habit of using a little instrument, which consists of a small disc of hard steel, very highly polished, and attached to a slender rod of soft, bendable, metal, by means of a ball-and-socket or screw joint, which gives great freedom of movement, and allows the plate of metal to be placed at any angle with its stem. The polished disc acts as a mirror of the first class, and when raised somewhat above the temperature of the breath (by being dipped into hot water), enables the surgeon to detect at the base of the tongue, and about the epiglottis, such morbid conditions as excoriations, ulcerations, small tumours, &c.

This simple contrivance was made for me by Mr. Matthews, of Portugal-street, and is much used, I believe, on the continent. In two instances of syphilitic ulceration of the throat, lately under my care in the Great Northern Hospital, it proved of considerable advantage; and its general utility will, I have but little doubt, be duly appreciated on many occasions. —*Lancet*, *Dec.* 24, 1859, *p.* 643.

69.—*On Tracheotomy.* By E. Lawford, M.D., Leighton Buzzard.—[One great difficulty in performing tracheotomy is that experienced in preventing the trachea from rolling under the knife. This is particularly experienced in young subjects, where the adipose tissue is abundant, and the trachea more deeply situated.]

Instruments have from time to time been introduced to the profession, calculated to overcome this difficulty. One surgeon has advised a tenaculum to be used to elevate the tube. Another directs that the patient should be requested to perform the act of swallowing; and at the moment when the tube is elevated and fixed by this action of the muscles, the favourable moment should be seized, and the scalpel immediately introduced. This would be all very well, provided the patient will obey directions; but children and half-asphyxiated people will not or cannot obey such directions, and we must do that which is necessary to be done for them. Now, it strikes me that there are three requisites in an instrument for the safety and expedition of this operation:—1. A transfixer, to steady the larynx; 2. A guard against the effects of antero-posterior pressure; and 3. A director for the knife. I say a guard against antero-posterior pressure; for I maintain that, in young subjects, the pressure necessary to carry the knife through the elastic rings would bring its point in dangerous proximity to the posterior wall; so much so that I think it is positively unsafe to open it without a director.

[The cassigna hook in common use in the French hospitals transfixes and elevates the trachea; but, inasmuch as the groove for the knife is on the convexity of the curve, it does not guard it, neither does it support the ring to be divided. Dr. Lawford recommends an instrument essentially similar to this, but with the groove for the knife on the concavity. This instrument answers the objects to be held in view, viz., transfixing the larynx, preventing antero-posterior pressure, and guarding the knife. This instrument may be obtained of Mr. Coxeter, of London.]—*British Med. Journal, Nov.* 19, 1859, *p.* 943.

ALIMENTARY CANAL.

70.—ON THE RADICAL CURE OF REDUCIBLE INGUINAL HERNIA.

By John Wood, Esq., F.R.C.S., Assistant-Surgeon to King's College Hospital. Read before the Royal Medical and Chirurgical Society.

[The plan followed by Wutzer and other operators failed because of their not obtaining a hold upon the posterior wall of the canal, and their securing only the anterior portion of the fold produced by invagination. Moreover, the principle of plugging up a dilatable orifice, like that of the inguinal opening, is a false one.]

The principle of Mr. Wood's operation is directly opposite to that of dilatation,—namely, that of drawing together and compressing the anterior and posterior walls of the canal in its whole length, and their union by the adhesive process with the invaginated fascia of the scrotum, which is detached from the skin and transplanted into the canal, the skin being left to adhere below to the approximated margins of the external abdominal ring. By this means the posterior wall of the inguinal canal is made to act as a valve to prevent any future descent of the bowel, shutting up the superior opening by becoming united to the anterior wall through the medium of the scrotal fascia, which thus affords a very highly organized and vascular connective tissue between the tendinous surfaces, which it would be very difficult to cause to adhere otherwise. The fascial vagination becomes likewise firmly adherent to the spermatic chord. This continues to be effective, even when the temporary effusion of lymph is reabsorbed.

The operation.—This consists, 1st, in detaching the scrotal fascia from the skin over the lowest part of the hernial protrusion, with a tenotomy knife, and then invaginating the fascia into the canal with the forefinger; 2dly, in passing a strong well-curved needle, fixed in a handle, armed with a stout, thick thread, and guided by the finger through three points in the canal—

viz., the conjoined tendon and the triangular fascia (forming the posterior wall), and the external pillar of the ring, close to Poupart's ligament, (forming the anterior wall of the canal). The ends of the ligature are left in the two former punctures, and a central loop in the latter, passing through the pillars of the external ring, and through the same aperture in the skin of the groin. This may readily be done by sliding upon the subjacent aponeurosis. 3dly, a cylindrical or flattened compress of glass or box-wood, two inches and a half long by one inch wide, is tied firmly upon the axis of the canal by passing the ends of the ligature through the loop, and tying over the compress. Before tightening the ligature, the surgeon should satisfy himself, by passing the forefinger through the external ring, that the ligatures draw upon the posterior wall. The opening in the scrotum should be tucked well up to, but not within, the external ring.

In recent cases of hernia, in which the sac is small and possesses an intimate vascular connexion with the peritoneum, and a very slight one with the cord, it may be pushed back into the superior opening, and the ligature applied altogether external to and without puncturing the sac, thus diminishing very much the chances of peritoneal inflammation. But in old and large herniæ, the sac has a more intimate vascular connexion with the scrotum and cord, and constitutes, as it were, a separate structure, distinct from the peritoneum. In these cases the sac is necessarily invaginated with the fascia, and the ligatures pass through it. In these the inflammation set up in the sac is much less liable to spread into the abdominal cavity, especially when the upper orifice is closed by the ligature. In a large sac the adhesive process is necessary to complete obliteration of the canal, and to prevent future complications.

The compress is removed from the fourth to the seventh day, according to the degree of action set up. The ligatures may be left in a week or two longer to act as conductors for the discharges, and to keep up consolidating action as long as may be desirable. When the sac is punctured, serous fluid flows from the wound in greater or less quantity during the first three or four days.

The author called attention to the action of the rectus muscle upon the inguinal canal, through the conjoined tendon, in drawing backward the posterior wall of the hernial canal, thus aiding the dilating action of the protruding bowel in the production and growth of the hernia. The effect of the ligatures and consequent adhesions in his operation directly counteracts this action of the rectus. He considers that the first tendency to oblique inguinal hernia, so often hereditary, is owing to deficient development of the lower fibres of the internal oblique producing an imperfect covering to the internal ring. In some of the cases operated on, he has succeeded in supplementing this deficiency by passing the scrotal fascia well up in front of the internal abdominal ring, and securing it to Poupart's ligament in that position.

He considers that the chief source of failure in the performance of his operation, especially in large and old cases, is in not securing a hold upon the posterior wall. By simply attaching the fascia to the pillars of the external ring, and drawing the latter together, the hernia, though prevented for a time from descending into the scrotum, still occupies the canal, and will, sooner or later, again dilate the external ring, unless constantly bolstered up by a truss. The closing of the external ring by the lower ligatures, in this operation, contributes much, however, to secure in its new position in the canal, the transplanted fascia.

In small cases of direct hernia, the closure or obliteration of the external ring only may be effective in producing a cure, if care be taken to obtain a hold with the inner end of the ligature, upon the triangular fascia covering the border of the rectus, immediately behind the opening of the external ring.

In noticing the objections to the plan, the author showed that by properly protecting the point of the needle with the finger, and keeping in front of the fascia transversalis, all danger of wounding the epigastric and circumflex iliac vessels or the bowel, was guarded against.

The fear of peritonitis is avoided in recent cases (in which it is most to be dreaded), by not puncturing the sac at all, closing up the tendinous opening external to it. In old cases, adhesive action may be set up in the sac without fear of its spreading to the peritoneum, as the results of numerous cases have shown. The objections made to the limited incision into the skin of the scrotum (which is little more than a puncture) he considers to be puerile. Its advantages in permitting the escape of discharges are evident.

Full reports of fifteen cases of hernia (all inguinal,) were appended to the paper. One of the cases was a boy of eight years of age; the ages of the others ranged from fifteen to fifty-four and fifty-eight years. One was a female with bubonocele; the rest were males. Three were cases of direct, the rest of oblique hernia. Thirteen were scrotal; four of large size, and three with very large and lax internal openings. Two were congenital, and two complicated with varicocele (cured also by the operation). In only one case were the symptoms at all severe, or gave suspicion of peritonitis. In this case, the patient was in King's College Hospital eight weeks; the symptoms were produced by burrowing of matter between the oblique muscles, following a diarrhœa then prevalent in the hospital (in July last). This patient made an excellent cure, was treated entirely without truss, and was one of the cases shown to the Society. The hernia had a very large internal opening, and the subject was cachectic and ill nourished before the operation. In one other case, the patient was in bed a month; in another, there was partial sloughing of the sac, which was a large and long one, with a very pendulous scrotum and a large varicocele. This case was treated also entirely without truss, and both hernia and varicocele were cured in eighteen days. The duration of treatment in the rest of the cases varied from nine to twenty-one days. Eight were treated entirely without truss. Thirteen are good and persistent cures, and have remained firm ever since, extending over the following periods of time: one (the first), very nearly two years,—this case was published in the 'Lancet' of the 29th of May, 1858; another, one year; two, ten months; four, nine months; three, eight months; one, two months. Three of the cases had been before operated on by Wutzer's and Ragg's methods; one case was operated on twice; one is doubtful; one was reruptured by indiscreet and early hard lifting without truss.

Six cases of cure were exhibited by the author to the fellows of the Society. Of these, four had been treated entirely without truss, and all had been well, and some severely tested by lifting and heavy labour. The first case (operated on nearly two years ago) was amongst those exhibited. No difference whatever was apparent between the groins of the two sides. One had been cured a year, three nine months, and one eight months. One of those treated without truss was congenital, in a young man aged twenty years; another was of five years' standing, in a man aged fifty-eight. The rest were of eighteen, sixteen, and three months' standing, respectively. All were scrotal herniæ, and two direct. Two had chronic bronchitis (at times severe) after the operation, and one during the progress of the cure.

[In the discussion which followed the reading of this paper, Mr. HENRY LEE suggested another mode of operating which he believed would attain the same results by less complicated means.]

The operation which he suggested consisted in invaginating the skin of the scrotum into the inguinal canal with the forefinger of the left hand. Using, then, the finger as a guide, the needle, armed with a ligature, was passed through the external wall of the inguinal canal, and out through the skin of the abdomen; one end of the ligature was then left free. The needle, being withdrawn, was then passed through the same aperture in the scrotum, and through the internal wall of the inguinal canal out through the skin. The needle then being taken away, the two ends of the ligature were tied together. The ligature was then allowed to remain for several days, or to ulcerate its way out. He considered that this plan had several advantages. It dispensed with the necessity of any dissection of parts, and of the dangers arising therefrom. The skin of the scrotum descended into its natural position two or three days after the operation, and there was less consequent deformity. Although the sides of the inguinal canal were brought together by the ligatures, yet the permanent adhesion depended upon the lymph effusion around the ligature, which was allowed to remain for that purpose. Another advantage was, that if any matter formed in the course of the punctures made, the presence of the ligature would ensure a ready outlet for the fluid. He (Mr. Lee) related the particulars of a case in which he had performed this operation in King's College Hospital, in the middle of last September. The patient, a butcher, accustomed to heavy work, had continued his occupation for several months, without any truss, and with the inguinal canal quite closed.

Mr. **Curling** had no doubt that a more simple mode of procedure than that described by Mr. Wood might be adopted in many cases with success. A case of omental hernia had lately been published in the journals, in which he had employed the principle practised by Mr. Wood, but in a manner somewhat similar to that followed by Mr. Lee—by passing the needle through the anterior and posterior walls of the canal. The proceeding was quite successful. Mr. Wood was entitled to great credit for the research and originality he had exhibited in his treatment of the subject before the Society. Surgeons for a long time past had been endeavouring to find means of overcoming the difficulty of obtaining a radical cure for hernia. In no case, however, had they gone to the root of the evil as Mr. Wood had done—viz., the contraction of the opening through which the intestine descended. He (Mr. Curling), however, did not see the necessity of separating and invaginating the superficial fascia. He thought Mr. Wood's object would be obtained in the passing of the needle by cutting through the superficial fascia, and carrying the finger onward into the canal. If Mr. Wood's operation were ever resorted to extensively, it must be for the purpose of dispensing with the truss, which was an object of great importance, especially to poor persons. In the cases before the Society an operation of a very difficult character had been skilfully performed, and it would be hardly fair to expect that in other hands the results would be so favorable Previous to the performance of the operation which he (Mr. Curling) had alluded to, he thought it desirable to practise it upon the dead subject. He accordingly did so on that of a phthisical subject, in which the inguinal canal was constricted. Now, in passing the needle under the conjoined tendons, he included a small portion of peritoneum. Such an occurrence, even in skilful hands might possibly happen when the operation was performed upon the living body. From what he had heard from Mr. Wood, he understood that in many of his cases the sac was invaginated; the ligature, therefore, passing through the sac must pass also through the membrane continuous with the peritoneum—a proceeding which would not be altogether without danger.

Mr. **Holthouse** thought, although the operation performed by Mr. Wood was a valuable one, that too much importance had been attached to the drawing together of the canal and the orifices. He considered that the main part of the cure must be attributed to the adhesion of the opposed surfaces of the canal. If this were really the case, it might be questioned how far an operation was applicable in every instance of the affection. Hernia presented itself to us in great variety, and the operative proceeding, in order to ensure success, should be adapted to the peculiarities of each case. In Gerdy's operation cure had resulted, not as the effect of plugging up the canal, but of the adhesion of its sides. In some cases which had come under his observation, the plug had come down very shortly after the performance of the operation, but a radical cure was, nevertheless, effected. This could only have resulted from the adhesion of the opposite walls of the canal. With respect to the occurrence of peritonitis after Gerdy's operation, he had examined sixty-two cases in which it had been performed. Amongst these, four deaths had occurred, but not one of these was from peritonitis. One death took place from pyæmia, one was accidental, one from suppuration in the abdominal walls, and in one the cause was unknown. In a case which had come under his (Mr. Holthouse's) care, in which Mr. Wood's operation was performed, great pain was suffered in the abdomen for the first three or four days, and fears of peritonitis were entertained. On removing the ligatures, however, at the end of the fifth day, the pain gradually subsided, and the patient made a good recovery. He died of bronchitis about four months afterwards, when no traces of peritonitis could be detected.

[Mr. SPENCER WELLS observed that Wützer's operation was in many cases preferable to either Mr. Wood's or Mr. Lee's.]

The danger of passing the needle through the sac was small in comparison with that of introducing vegetable threads, which had the effect of strangulating and killing the portions of tissues which they encircled. In one case, at Chatham, he believed, death had resulted from the performance of Mr. Wood's operation; another fatal case had occurred at King's College Hospital; and one case, in which Mr. Wood had himself operated, had been attended by symptoms of a very serious character. In cases in which the canal was long and narrow, and where the plug could be introduced with facility, the simpler and safer operation of Wützer was to be preferred Where, however, the canal was short, the abdominal rings wide, and it was only possible to apply constriction upon one point of the canal, the operation of Mr. Wood, or some modification of it, such as that suggested by Mr. Curling, might be employed with advantage.—*Lancet, March* 10, 1860, *p.* 246.

71.—RADICAL CURE OF HERNIA; A NEW MODIFICATION OF WUTZER'S OPERATION.

In the following case, in which a new modification of Wützer's operation was performed, his instrument was wholly dispensed with, and the sides of the canal were brought together by a simple ligature. The ligature was allowed to remain and to ulcerate its way out, in order that any matter that might form should have a free exit. The consolidation of parts is produced not by the invaginated tissues so much as by the lymph effused around the ligature in the inguinal canal. Mr. Lee considered this modification of Wützer's operation safer than some others lately introduced, inasmuch as it involved no separation of the skin from the subjacent tissues, and was consequently less likely to be followed by cellular inflammation. The plan of allowing the ligature to ulcerate its way out Mr. Lee also thought was attended with a twofold advantage,—namely, 1st, it ensured an escape of any matter that might form in its course; 2nd, its prolonged presence in the inguinal canal rendered the obliteration of that canal by the effusion of lymph more certain.

Benjamin F., aged sixteen, was admitted into the hospital on the 17th of September, 1859, with an oblique inguinal hernia of the right side. The hernia descended every time the patient stood up, but was again easily reduced. Below the external ring there was a permanent enlargement, caused by a hydrocele of the cord. This was quite distinct from the swelling produced by the hernia. On the day of admission, the skin of the scrotum was invaginated into the inguinal canal, and a ligature was passed, by means of a curved needle, along the invaginated portion of skin, through the internal wall of the inguinal canal, and out through the skin of the abdomen. The needle was then withdrawn, leaving one end of the ligature protruding through the opening. The needle was then again introduced, by the same opening into the invaginated skin, through the external wall of the inguinal canal and through the skin of the abdomen. One extremity of the ligature was left free, projecting through this opening, and the needle was withdrawn. The two ends of the ligature were now tightly tied together, bringing into conjunction the opposite sides of the inguinal canal, and embracing a portion of the superjacent skin. The needle, in being introduced, was purposely made to pierce the walls of the hydrocele of the cord, and some transparent fluid escaped. No disturbance of any kind followed the operation.

Sept. 22nd. The invaginated portion of skin had descended to its natural position.

24th. The ligature was tightened.

25th. A considerable amount of pain produced by the ligature; general health very good.

26th. Pain relieved; there is now no impulse on coughing in the inguinal canal; the wound discharges feeely.

Oct. 4th. There was a recurrence of the pain in the situation of the ligature during the night.

6th. The pain had subsided; no constitutional irritation.

17th. The ligature, being held by a piece of tendon or fascia, was removed; there is no impulse on coughing.

Nov. 4th. Left the hospital a day or two after the last report, and has been at work since that period, wearing a truss; there is no impulse upon coughing; the external wound is healed; there is no mark on the scrotum where the ligature passed. The hydrocele of the cord had been apparently quite cured. He was directed to discontinue his truss.

Ten days later he again presented himself, having continued his work without a truss. The pillars of the external ring could be felt separated, but above this the inguinal canal appeared quite closed.

Dec. 5th. This patient again presented himself, having continued his work without a truss. The hernia appeared perfectly cured—*Lancet, Jan.* 14, 1860, *p.* 35.

72.—ON A NEW MODIFICATION OF WUTZER'S INSTRUMENT FOR THE RADICAL CURE OF REDUCIBLE HERNIA.

By Furneaux Jordan, Esq., Assistant-Surgeon to the Queen's Hospital, Birmingham.

In Wutzer's operation, the difficulty which all surgeons have experienced and described, is in obtaining sufficient adhesion between the invaginated cylinder of integuments and the posterior wall of the inguinal canal, and in efficiently blocking up the inner inguinal aperture. In two cases which were recently under my care at the Queen's Hospital I used the following instrument with the best results.

The instrument consists of a hollow metal plug. In the interior, and along the upper wall, is a small canal for the needle; beneath the needle, and occupying the remainder of the hollow space, is a vulcanised india-rubber apparatus of a pyriform shape, the wider extremity being at the round end of the plug. At the under aspect of the tubular plug is an aperture, oval in form—one extremity being a little wider than the other—seated close to the end of the plug, and slightly encroaching on its rounded extremity. Through the aperture dilatation may be effected with air, or water, or glycerine—fluid is perhaps preferable, because however accurate the stop-cock, probably a little air may escape—by means of a small syringe, which can be easily applied to a tube and stop-cock connected with the smaller extremity of the vulcanised tube, and fixed to the flat end of the instrument. The tube must be introduced into the hernial canal, the needle passed, and the skin-piece fixed, *before* dilatation is effected. The attempt to dilate before fixing the plug would result in the repulsion of the instrument to an extent corresponding with the degree of dilatation. The amount of fluid dilatation of the instrument, of which it is desirable to have three sizes, admits of easy measurement before it is used. The instrument may be adapted to almost every form of hernia (the cases in which I used it were direct inguinal), by bending or otherwise altering the form of the tube, or by directing the fluid dilatation at will by an aperture in any part of the plug, or by uniting both these possible methods of modification.

Its advantages are these:—The fluid dilatation posteriorly presses upon the posterior wall of the inguinal canal, and thus induces adhesion between the approximated sides of that canal and the posterial aspect of the invaginated mass of cutaneous structures. But it does more than this: *the air or* *fluid pressure, from its peculiar character, is exerted chiefly at and fills up the internal ring or other aperture—whatever its position, or shape, or size.*

I would suggest here that—as *continued* pressure is the most effective agent in the absorption of inflammatory exudations and the atrophy of normal structures—the pad, spica bandage, and subsequent truss, might perhaps be omitted with advantage in the after treatment of the radical cure of hernia, the patient simply wearing an efficient suspensory bandage. We want as much new and old tissue as possible in the inguinal apertures. To take great pains to excite inflammatory exudation, and then to take equal pains to disperse it, is surely a surgical paradox.—*Med. Times and Gazette, Jan.* 21, 1860, *p.* 58.

73.—*Injections of Iodine into the Tract of Fistulæ in Ano.*—Many patients refuse the knife in this complaint. Hence many contrivances have been devised; amongst which Mr. Luke's instrument for gradually cutting through the parts by means of a thread and rack has been found very useful. No means but the knife, or such substitutes as Mr. Luke's instrument, can be of any avail when the tract has become callous and insensible to stimulating or caustic injections; but, in recent cases, various injections have proved of great service. Of late, pure tincture of iodine has been tried; and we find several successful cases related in 'L'Union Médicale' of the 26th ult., by M. Henry, assistant to M. Bonnafont, at the Infirmary of the Invalides (the analogue to our Chelsea Hospital) in Paris. To prevent the injections irritating the rectum, in complete fistula, M. Bonnafont places a tent in the bowel, reaching about an inch above the intestinal orifice of the fistula. This tent is also very useful in respect of diagnosis, when we are in doubt whether the fistula is blind or complete. Put the tent into the rectum pretty high up, and inject iodine; if it comes out unstained, the fistula is blind; if stained, it is complete.—*Lancet, Dec.* 17, 1859, *p.* 621.

74.—*Severe Prolapsus Ani; Sloughing of the Mass; Cure.*—In old prolapse of the rectum, where considerable hypertrophy and induration of the protruded part have occurred, it is commonly necessary, when great inconvenience arises, and the return is not to be effected by supine posture, cold applications, and equal pressure, that the prolapsed part be cut off at the base, or treated with nitric acid. In a case of severe prolapsus of the rectum admitted into St. Mary's Hospital, under the care of Mr. COULSON, any such proceeding was superseded by the operation of nature. The patient, an old man of sixty-two, was the subject of an old prolapse which had been irreducible, and was very painful during the week prior to his admission. When admitted, it existed as a circular tumour, turgid, hard, and inflamed, of the size of a small orange. It was uncomplicated with hemorrhoids; and there was no retention of urine. The patient suffered considerable dragging pain, and defecation was highly inconvenient. Emollient applications, laxative medicine, and the supine position were employed to relieve his pain. Sloughing of the tumour, however, quickly ensued after his admission, and in the course of a week the whole plug of membrane sloughed away, and a complete and spontaneous cure took place. The cure in such a case results from a precisely similar process to that which the surgeon imitatively excites by the employment of the ligature. Artificial strangulation of the base of a hemorrhoidal tumour has, however, this advantage for the patient over the natural process seen in the above case, that in the latter instance the strangulation is necessarily preceded by a considerable amount of inflammation and engorgement, which are productive of great pain and suffering during the preliminary stages.—*Lancet, Nov.* 26, 1859, *p.* 535.

ORGANS OF URINE AND GENERATION.

75.—ON LITHOTOMY CONSIDERED AS A CAUSE OF DEATH.

By Professor A. Buchanan, M.D., University of Glasgow.

[To simplify the subject as much as possible, Dr. Buchanan limits the present paper to a consideration of the causes of death in cases where the organs are sound and healthy, and the general health good.]

There are, according to my views, four causes which render the operation for stone in itself dangerous and fatal. These are—1. Infiltration of urine into the subserous cellular tissue of the peritoneum, and the passage of it through that membrane, by osmosis or simple percolation, into the interior of the peritoneal cavity. 2. Infiltration of urine into the subfacial cellular tissue of the pelvis, while all access to the peritoneal cavity is debarred by the dense fibrous membrane. 3. Mechanical contusion of the bladder and parts adjacent, of which what is called "shock" is the most severe form. And 4. Hemorrhage.

1. It may be objected to this arrangement that the two first causes are comprehended under the general expression, infiltration of urine. But it is very generally of that expression, as employed to indicate a cause of death after lithotomy, that renders it objectionable, from its being ambiguous and devoid of definite meaning. Infiltration of urine is not in itself a cause of death, for it occurs after every operation for stone, but only infiltration in certain situations; and unless these situations are specified, no accurate idea has been expressed. That mere filtration of urine is not a cause of death, we have evidence in what we see occur every day in urinary abscesses, or effusion of urine under the superficial fascia of the perineum. That the urine must be largely absorbed after such effusion is certain, but we never see any bad effect thence resulting; nor apprehend anything more than the local mischief arising from the irritant action of the urine, which occasions sloughing of the cellular tissue, and that may be so severe and extensive as to cause death, especially if not mitigated by incisions. Such external effusions warn us what must be the extreme danger of effusion of urine proceeding inwardly into situations where there is no possibility of its escaping, and where it comes into contact with parts more susceptible of irritation. Still, however, the affections induced, whether by external or internal effusions, proceed alike from a local irritation. But such affections are so totally dissimilar in respect to the symptoms which they exhibit and the course which they run, according to the seat of that irritation, that no man who has any pretension to scientific accuracy in his discrimination of diseases can possibly confound them together; which is clearly done when they are referred indiscriminately to infiltration of urine.

The two forms of urinary infiltration, which become causes of death after lithotomy, take place in consequence of special errors in the mode of cutting for stone, which it is most important to signalize. The first kind of infiltration has been observed chiefly after the rectangular operation, and was, indeed, never discriminated as a distinct affection till that operation came into use. It may, however, occur readily after the common lateral operation; after which, however, the second form of infiltration frequently presents itself. These specialities depend upon the particular errors into which the operator is most apt to fall in adopting these respective modes of operating.

Six cases which I have described present no illustration of the second kind of infiltration, and all of them but one seem to me to be instances of the first. Of these cases, I have described minutely the appearances that presented themselves after death in the second, third, and fourth. The two first of them, notwithstanding the care with which I examined and reflected over them, completely baffled me, and left my mind in a state of utter perplexity and darkness with respect to their true nature: the only point which admitted of no doubt as to both, being that the patients had been, in some unascertained and mysterious way, stabbed in the lower belly. It was only on examining the third case that daylight broke in upon me, and gave me a glimmering idea of the probable nature of them all. An accidental circumstance, which I have mentioned above, first put me upon the right path. The ecchymosis, described above, terminated in a well-defined line on the sigmoid flexure of the colon, attracted my attention. The blood forming it had evidently come from below. I traced it all round the rectum, where that intestine is covered with peritoneum, and beyond the reflection of the peritoneum I found the blood extending downward, between the rectum and the bladder, as far as the external wound, The source of the blood was thus obvious. From the external wound it had made its way into the cellular tissue between the bladder and rectum, till it reached the reflection of the peritoneum (recto-vesical pouch). and thence, following the course of that membrane, it had been diffused alike over the bladder on the one side, and the rectum and colon on the other.

But granting this to be the true origin of the blood, it will be said that blood is an innocuous fluid; and that the effusion of blood under the peritoneum never could give rise to the grave symptoms under which those patients laboured. It must, however, be considered that wherever blood passes, a more subtle and penetrating fluid will pass more readily and extensively. The urine, therefore, with which the surface of the wound after lithotomy is continually bathed, entering by the same aperture as the blood, will diffuse itself in the cellular substance between the bladder and rectum, and will thence make its way into the subserous tissue behind the peritoneum covering the parietes and viscera of the abdomen. But will it stop behind the peritoneum? Assuredly not. It will pass through that membrane by osmosis, or by simple percolation; and will enter the peritoneal cavity. That the sanguinolent fluid, which was in four out of five of these cases found in the cavity of the abdomen, consisted of urine holding the colouring matter of the blood in solution, I entertain no doubt; although it was not proved to be so by chemical tests. But, if urine be infused into the cavity of the peritoneum, it will produce the same effects, whether it enter by the circuitous route just described, or be directly effused from a rupture of the urinary bladder. Now the known effect of the effusion of urine, of bile, or of the contents of the alimentary canal into the peritoneal cavity, is death in about twenty-four hours; and I refer to the same cause the fearfully-rapid death that takes place after a badly-performed rectangular operation for stone.

Such is my view of these rapidly fatal cases, and there seems to me to be two causes of the rapidity of the fatal event. First, the rapidity of the infiltration, taking place from a wound situated immediately beneath the open urethra, along which the urine secreted into the bladder continually passes, to be diffused laterally over the cut surface and the orifice of the open wound beneath the prostate; and, second, the circumstance that the urine passing inwardly by a wound beneath the prostate is not confined by any fascial covering. but comes into immediate contact with the peritoneum.

This view of the subject is, to my mind at least, much more probable, as being more consonant with facts and with physiology, than the theory which ascribes to shocks all deaths that occur rapidly after lithotomy. How, it may be asked, can such a cause be assigned in such cases as No. 3, in which the whole duration of the operation was less than a minute, and the wound was of smsll size?

2. In the absence of cases illustrative of the mode in which death takes place after the second kind of urinary infiltration, we must, for the purpose of comparison, borrow the descriptions which have been given of them. The patients, instead of being seriously affected immediately after the operation and dying in twenty-four hours, are described as sometimes appearing to do well for several days, when they rolled round upon their bellies, and died in great agony; or, as sometimes surviving for a longer period, with gradually increasing emaciation, or hectic symptoms: while after death there is found purulent infiltration into the cellular tissue of the pelvis, or circumscribed abscesses. There is, therefore, a total dissimilarity in these

cases to the former, whether we look at the symptoms during life, or the pathological lesions after death. We may infer also from the kind of operation performed, that the wound extending beyond the rim or base of the prostate had divided the deep perineal fascia, or levator ani muscle, in such a way as to permit the infiltration of urine under the recto-vesicle fascia where it lines the sides of the pelvic cavity.

The views stated above as to the cause of the infiltration of urine, have a most important bearing upon the operative process in lithotomy. If it be true that the greatest danger of lithotomy arises from urine getting into the cellular space beneath the prostate, or rather between the bladder, prostate and urethra on the one side, and the rectum on the other, then the great rule for performing lithotomy safely is to avoid carefully this hypoprostatic space, as I shall take the liberty of calling it. This space is enclosed by strong and well defined boundaries. It lies just beneath the recto-vesical pouch of the peritoneum, and is therefore conterminous with the abdominal cavity, from which it is separated by a thin membrane. It has the bladder, prostate, and a small portion of the membranous part of the urethra, in front; the rectum behind; and on each side the recto-vesical fascia, or lateral ligament of the bladder. The vas deferens and the vesiculæ lie within it, and it is filled with a delicate cellular tissue free from fat. When the bladder is folded forward over the pubes, the hypoprostatic space has very much the shape of a four-sided pyramid, of which the apex is truncated, and lies a little below the apex of the prostate. The boundary forming this apex consists of the deep perineal fascia where it intervenes between the urethra and rectum, and forms there the only fibrous septum for the protection of the abdominal viscera. If this septum be divided while the bladder is at the same time opened, infiltration behind the prostate must necessarily ensue.

It is of great importance, therefore, to study the boundaries of the hypoprostatic space, with the view of determining at what points they are most apt to be violated by the knife of the lithotomist. I am satisfied that the great source of the danger of lithotomy has been generally misunderstood. We may see this in the admonition invariably given to the student and young operator never to extend his incision beyond the base of the prostate. This admonition, in so far as it represents such an incision as the great source of danger in lithotomy, involves a double error; for there are certain directions which the base of the prostate may be both safely and advantageously cut; and there are other directions in which a fatal error will be committed, although the gland is not divided half way to the base. The just and true admonition, which I would substitute for the above, is never to do the slightest violence to the fibrous tissues, which form the three planes at the apex and the two sides of the pyramidal space beneath the urethra, prostate, and bladder.

The mode and place in which the hypoprostatic space is laid open, differ in the old operation and in the new. In the latter the lateralized knife, penetrating too low, divides the deep perineal fascia behind the urethra; and being thought to be in the groove of the staff, it is carried deep under or through the substance of the prostate into the lax cellular tissue beyond. The lateral incision to the left side completes the violence inflicted on these delicate parts. The operator now ascertains with his finger that he has not entered the bladder, and rectifies, as he supposes, his mistake, by carrying his knife higher up and dividing the prostate; but it would be much wiser in such circumstances to send the patient to bed after the first unsuccessful thrust: for the wound, though a severe one, is not yet necessarily fatal, but by the second incision, infiltration of urine into the subserous tissue of the peritoneum, and death in twenty-four hours becomes inevitable. This is, I believe, the true explanation of the frightful rapidity of these fatal cases. They constitute a new phase in the history of lithotomy, altogether peculiar to this new operation. Five out of six fatal cases that have occurred in our hospital have been of this kind, and cases in every respect similar have occurred in private practice. This is now so generally known in this city, that an operator, if his patient is not dead or obviously dying at the end of twenty-four hours, is congratulated on the success of his operation.

There are two other modes in which the hypoprostatic space may be laid open in peforming the rectangular operation for stone, which I think it important to signalise. It is well known that in whatever part of the urethra or prostate the angle of the staff be made to rest, that part is protruded in front of the orifice of the groove projecting in the perineum. If, therefore, the operator be not careful to ascertain that the angle of the staff is sufficiently far forward, if he allow it to rest at the back of the membraneous portion of the urethra, the deep perineal fascia is folded over the mouth of the groove, and must be divided in entering the groove with the knife; and if the angle rest on the prostatic portion on the urethra, on the body of the gland, or farther back, a still deeper wound will be inflicted on entering the groove, as it is easy to verify on the dead body.

The third mode in which I have seen the accident happen, is the result of a mode of operating, which some operators have tried, but which, as appears to me, is not to be recommended. They make the external incision first, that they may enter the groove of the staff with more facility than by a second incision. But in thrusting the knife into the middle of the perineum for the purpose of commencing the first incision, the deep perineal fascia, which is only a few lines from the surface, must be very liable to injury, as the knife can enter the middle of the perineum in no way safely unless lodged in the groove of the staff. The wound thus produced may not be very deep, but I have seen the most formidable symptoms result from it—long-continued shivering, profuse sweating and diarrhœa, lasting for several days, and causing great anxiety for the patient's safety, although he ultimately did well.

In the lateral operation, from the direction of the incisions they are less apt to do injury to the hypoprostatic space, and cannot do it to the same formidable extent; but that space must be laid open more or less by those misdirected incisions that penetrate into the rectum and injure the vesiculæ seminales. It seems to me at least not improbable, that in many of the rapidly fatal cases, death may be owing to this cause rather than to shock, to which it is usually ascribed. The more the incision of the prostate is directed downward, the accident must be the more likely to happen. Perhaps, therefore, we may thus explain the increased mortality after the lateral operation of lithotomy, which seems to me beyond all doubt to have taken place in our own day. Cheselden cut the prostate outward, keeping the edge of his knife turned up. But we have since been recommended, upon high surgical authority, to divide the prostate downward and outward, in the line of the external incision. By following this precept, I cut the rectum in one of my first lateral operations, and I abandoned it ever after, cutting directly outward; now any precept that tends to endanger the rectum will also render the space under the prostate more liable to be laid open.

The importance of the subject must be my excuse for one further reference to anatomy. The hypoprostatic space, as above defined, is divided diagonally from before to behind by that portion of the recto-vesical fascia, which, to avoid the confusion of names, I shall call the fascia interposita, from its lying between the rectum and bladder. It comes off from the deep perineal fascia behind the urethra, forms the proper tunic of the lower part of the prostate, and stretches from the base of that gland towards the rectum, in front of which it runs upwards, gradually assuming the characters of mere cellular tissue. When, therefore, the deep perineal fascia has been divided behind the urethra, there are two routes by which the infiltrated urine may reach the peritoneum; the one between the rectum and fascia interposita, which is circuitous and probably less fraught with danger; the other between the fascia interposita and bladder, which to lay it open requires a deeper wound, dividing either the fascia interposita or the substance of the prostate.

With respect to the best mode of extending the ordinary incisions of the Rectangular Operation for the purpose of extracting stones of large size, we must take for our guide the preceding considerations derived from anatomy, and the observed results of different modes of incision. We have seen that there is danger in extending the ordinary incisions laterally, and still more downward. In the former way we run the risk of subfascial infiltration of urine, and, in the latter, of the more dangerous form of infiltration into the subserous tissue of the peritoneum. The incision upward is, therefore, all that remains to us. It is clear that, if a moderate lateral incision has been first made, above the level of which the urine in the bladder can never rise, there will be no risk of urinary infiltration from an incision upward, even though carried beyond the base of the prostate, for the urine can never come into contact with such a wound. Even without the precaution of a previous lateral incision, Dr. John Thompson recommended the incision upward, and Dupuytren found it to be safe in practice.

The mode in which I perform the incision upward is by means of an instrument, which I name a *finger-director*, which I have for many years past used in cutting for anal fistula. It is a metallic cylinder, tapering from the rounded point to the base, open in front, and fitting like the finger of a glove to the index finger, while the handle attached to the back part of it lies flat upon the back of the hand; unlike the clumsy rectangular handle of the speculum ani, which, in imitation of the above instrument, has been more recently introduced to answer similar purposes. For these its structure shows that it was never intended; and those who know the history of the operation in question are aware that it was never till of late years generally so applied. This tapering cylinder, fitted upon the index finger, enters the wound with the same facility as the index itself; and the part of it, at which its further progress is arrested, gives an accurate measurement of the circumference of the wound of the prostate. The handle of the instrument being now securely held, while the open side of the cylinder is turned upward, the forefinger feels with much nicety the tense fibres stretched over the open groove which require to be divided; and this is done by one or more incisions with a curbed bistoury. The director can now be pushed further in, while an exact measurement of the size of the enlarged internal aperture is given as before. If need be, this incision can be carried beyond the base of the prostate, dividing the bladder to whatever extent may be deemed necessary; and this, with the lateral incision first made, will allow any stone to be extracted that can pass between the bones and the soft parts over them. After making a sufficient incision upward it is, I believe, never necessary to cut the right side of the prostate, and much safer not to do it. But a bilateral section of the external parts may be required for a large stone, and even a third incision directly upward, or a little to the left of the raphé of the perineum; thus giving the enlarged external incision a tricuspid form, not unlike a leech-bite on a great scale.

[The *Rectangular Operation* is now universally the one followed in Glasgow and the neighbouring parts of Scotland; but with the exception of Mr. Spence, Dr. Buchanan states that not a single surgeon in Edinburgh has ever performed it; and in other parts of the country it has been "opposed, misrepresented, and ignored." Dr. Buchanan says:]

Notwithstanding these unfavorable appearances, I am fully satisfied that the seed which I have committed to the ground is not destined to remain unproductive, but is even now in vigorous growth, and will ere long yield an ample harvest. The signs of this result are to me unmistakeable. Twelve years ago, when I first threw a pebble upon its placid surface, the opinion of all medical men in this country on the subject of lithotomy was tranquil and undisturbed. The lateral operation of Cheselden was accepted by all as an ultimate result of surgical ingenuity, and the boldest innovators for half-a-century before had never ventured farther than to modify the shape of the cutting instrument, or alter slightly the direction and the extent of the incisions. How different is the case now! Throughout all England the medical mind is in a ferment on the subject of lithotomy. Every periodical teems with observations and cases innumerable, all indicative of some great change which the public mind is in process of undergoing. And what is that change?

It is exactly what I first advocated. All are now thoroughly determined, as determined as I then professed myself to be, to abandon for ever the old homicidal lateral operation for stone. Still further, they are all, without any exception, equally unanimous as to the direction in which the new movement is to be made. It is exactly the direction which I recommended to them. They all try to penetrate into the bladder, directly in the mesial or middle line of the perineum. To be sure, they do not do this exactly in the way in which I recommend that it should be done. But that would have been an honour too great for me to expect from metropolitan surgeons, more accustomed to give than to receive instruction. They prefer, not unnaturally, using their old instruments to my new ones. Some of them, however, have gone so far as to take my staff into their hands, and they express their surprise at the facility with which it slips into the bladder. But they do not call it by my name, or by the name which, as the inventor I was entitled to give it. They prefer calling it, I am afraid less in compliment than in derision, "the Glasgow staff." Some have gone further, and have actually used the Glasgow staff in their operations. But do not suppose that they have followed my directions as to the mode of using it. They have more spirit. They hold the groove of the staff in the middle of the perineum, or right up against the pubes, as if to deviate as far as possible from the leading maxim of holding it under the level of the bulb of the urethra, which any tyro at Glasgow could prove to them to be indispensable. Last of all, there are surgeons more independent still. They do not use the Glasgow staff. They get a small bit added to their end of it; or better still, they drill a hole right through it, and then they consecrate the new staff by giving to it their own name, so that thenceforward no one can justly say of them that they use any other staff than their own. If, in fine, they wish to attain the highest distinction—to add the praise of ingenuousness to that of ingenuity—they publish a description of the new staff, while they admit candidly that some man, somewhere in Scotland, did previously devise and use a staff of a somewhat similar description.

The name of Rectangular Operation has been an unfortunate one, as tending to produce confusion in the public mind between the operation and the instrument used in performing it. It is impossible to dispel from the minds of many surgeons, more especially of those who have invented rectangular instruments and wish to use them in their own way, that "rectangular operation" can mean anything else than an operation performed with a staff of a rectangular shape, in whatever position that staff may be held. But to perform the rectangular operation, such as I have described it, the staff must be held in one invariable position, viz., with the groove under the level of the bulb of the urethra; and if the groove be placed in any other position, it is no longer the operation described by me that is performed. It is only when held in this position that the staff will permit the knife to penetrate into the bladder by the shortest possible route, and to cut outward, making a safe and sufficient opening for the extraction of the stone. Whoever operates with a rectangular staff held in the position just described, performs essentially the operation which I recommend, whatever subordinate modification may be given to the staff. I owe, indeed, and beg to express my thanks to many ingenious gentlemen who have introduced such modifications, while they retain the essential points of the rectangular shape of the staff, the true position of the groove, and the direction of the section outward. If I have not myself adopted any of the proposed modifications, it is because I have not been able to persuade myself that the simple staff in the hands of a man of ordinary dexterity, duly instructed in the way of using it, is not a superior instrument to any of the more complicated staffs which have yet been constructed, so far as I have seen them. As to those gentlemen again who employ rectangular staffs, whether simple or complicated, held in any other position than that described above, I can only say that I cannot perceive the use which such instruments so held are intended to serve, and that personally I owe them no thanks, but the reverse, for the confusion which they have occasioned in the public mind as to the true nature of the operation which I have named "rectangular."

I must advert more particularly to two operations, in some respects very much akin to my own, which have obtained popularity in England. These are Mr. Allarton's median operation, and Mr. Hutchinson's operation with his rectangular catheter-staff. I may be allowed, without undervaluing the labours of those gentlemen, to urge the similarity and priority of my own.

Mr. Allarton's operation was first given to the public six years after mine—at a period therefore when the practical success of the latter had been fully ascertained. The object of both operations, in so far as incision is concerned, is the same—to penetrate into the bladder in the middle line of the perineum. I first applied the term *mesial*, in contradistinction to *lateral*, as a generic name for all such operations. Mr. Allarton, unwilling apparently to borrow anything from me, with much ingenuity changes two of the letters of this name, and calls his operation the "median operation." Now, as the one word is just as good as the other, and there is no difference in their signification, little objection could have been made to this change had he contented himself with calling his operation *a* median operation, but to call it *the* median operation, as if no other median operation were in existence, is to arrogate for it a degree of importance not warranted by its priority in point of time, or, as it appears to me at least, by its demonstrated excellence.

While mesial, or median if it be preferred, is the generic name according to the nomenclature which I have proposed, the different modes of operating in the middle line are denoted by specific names indicating in what way the prostate is divided in each of them. If we apply this principle to Mr. Allarton's operation, it ought to be named the *medio-lacerant*, for I believe this term to express truly the mode in which for the most part, the prostate is actually divided; but if any objection be made to that assumption, the operation might then be called the *medio-dilatory*, which would express the double idea of the process of dilatation and the tediousness of that process.

To represent Mr. Allarton's section as a revival of the "sectio Mariana" is manifestly erroneous. Had Mr. Allarton imitated Marianus, he wonld not have cut in the middle line of the perineum at all, but to the left of it, and would have gone right through the corpus spungiosum and bulb into the canal of the urethra, leaving the membraneous part untouched by the knife. Mr. Allarton's section is a much more scientific one, and the offspring of a much later age. So far as I know the history of lithotomy—if we pass over the crude attempts of the Celsian operators, and come down to the time when anatomy had become a science—Dupuytren is the first surgeon who ever attempted to reach the bladder by cutting from the middle line of the perineum through the membraneous and prostatic portions of the urethra. To that great surgeon of the last generation—to whom I owe the respect and gratitude of a pupil to his teacher—I willingly professed my own obligations, assuming his bilateral operation as the starting-point of my own researches. But the bilateral operation was not the first mesial operation devised by Dupuytren. At an earlier period of his career he performed the very section now recommended by Mr. Allarton, but he abandoned it on finding that it was only fitted, even when accompanied with a free section of the prostate, for extracting stones of a very small size (très-peu volumineux). There can be no doubt that this is the source from which Mr. Allarton's operation has been actually derived, although he is not himself aware of it. His own account is, however, quite reconcilable with this view. He says he derived it indirectly from Bresciani di Borsa, through the medium of the 'Medico-Chirurgical Review.' But the Italian surgeon obviously adopted his section from Dupuytren's first mesial operation, just as he adopted the double lithotome from the second, in all cases where the size of the stone required it. I have thus shown that in object, in name, and in origin, Mr. Allarton's section is very similar to my own—the great difference being that Mr. Allarton operates with the old staff, as I originally did, to the great danger of the rectum and bulb of the urethra, while I operate with a staff especially devised to guard against these dangers.

There are only two steps in Mr. Allarton's section which he claims as altogether his own. These are keeping his finger in the rectum to guide his knife, and using the knife by stabbing as with a dagger instead of by cutting inward. Now these are the well-known steps of a rectangular operation. From these numerous coincidences, I am satisfied that Mr. Allarton has directly or indirectly profited by my labours more than if in the former way he has thought fit to acknowledge, or if in the latter than he may himself be aware of.

I hope I have said enough to show that Mr. Allarton has no title to the credit which is so generally accorded to him all over England of having introduced the mesial section for stone into this country. Mr, Allarton, as I have above stated, introduced his operation six years after mine, at a period when the practical success of the latter was no longer doubtful. Both operations are taken from Dupuytren—mine directly from his last and most perfect effort, which I have so modified as to render it, I believe, more safe and easy of execution, and more successful in its results: Mr. Allarton's, from an earlier operation which had been abandoned by Dupuytren himself on account of its imperfection, but which was revived by an Italian surgeon who wrote more than twenty years afterwards, and from whom Mr. Allarton indirectly takes it. The subject may be illustrated thus:—The city of Glasgow has just received a supply of pure water from Loch Katrine, the source of the River Forth; now if, six years after this, it were proposed to bring in another supply of water into the city, taking it not from the fountain-head, but much further down from the turbid stream, which meanders beneath Stirling Castle, the attempt would be quite parallel to Mr. Allarton's attempt to reintroduce into this country the mesial section for stone.

Although it is only with Mr. Allarton's section, or the first part of his operation, that I have properly anything to do, I cannot avoid offering a few remarks in passing upon the second part of it, in which unquestionably he shows himself a genuine disciple of Marianus. One great cause of the slow progress of the human intellect seems to be the tendency which it has, not to advance in a straight line, but to move onward in a kind of spiral curve, which brings it frequently back after a wide circuit to some point which it occupied long before. It is impossible to avoid this reflection when we see sensible men emulating each other in devising instruments, for the purpose of renewing, in the last half of the nineteenth century, the barbarities which were the disgrace of Surgery three hundred years before. It must be owned, however, that these proceedings are a genuine result of a principle which has long passed, and passes at the present day unquestioned as an axiom among lithotomists, viz., that the prostate gland is more safely torn than cut. How a proposition so repugnant to common sense, and so inconsistent with our experience of every other tissue of the human body, should have met with a general acceptance with respect to tissue so dense and unyielding as the prostate and its capsule, is difficult to conceive. With whom it originated I do not know; but it has since passed as one of the arcana of lithotomy from one operator to another, and being screened from all general investigation, from the belief that none but lithotomists were capable of judging of it. I think, however, it most probably originated in the following way:—If a knife be pushed through an orange, or any similar solid, dividing it in the middle, but leaving it entire at the sides; and if thereafter the finger be forced through the opening, it will be found that a great deal of boring and apparent dilating or tearing is required to allow the finger to pass; but it is merely the effort necessary to change two plane and parallel surfaces into a cylindrical surface capable of adapting itself to the surface of the finger. Now, every operator, who thrusts his finger into the bladder, after cutting the prostate gland, experiences similar sensations, and is apt to imagine that he is dilating or tearing the substance of the prostate, when he is doing no more than adapting the two plane and parallel cut surfaces of the gland to the curvilinear surface of his own finger. Tearing the prostate is something very different; few tissues of the body resist it more powerfully, or would, I believe be more injured by it. While the prostate ought never to be cut a single hairbreadth beyond what is necessary for the extraction of the stone, it ought, according to my view, to be cut, and not torn to the necessary extent.

Mr. Hutchinson, of London, has invented what he calls a rectangular catheter-staff, which he directs to be held in the middle of the perineum. What end can be proposed in holding such an instrument in such a position I cannot conceive, unless it be to render absolutely certain the division of the spongy body and bulb of the urethra, which are doubled over the angle of the staff, and cannot possibly escape the knife. I do not speak of what I have not seen. An eminent surgeon of this city took up at one time the same idea as Mr. Hutchinson, and insisted that his rectangular staff should be held up under the arch of the pubes. I held it myself, reluctantly indeed, but without one word of remonstrance, which I knew would have been vain. Such a gush of blood followed the introduction of the knife as I never before witnessed in lithotomy. It would have been called a flooding in the other sex; but most fortunately, although quite unlooked for by either of us, the bleeding stopped almost completely as soon as the patient was unbound and the staff withdrawn from the urethra.

With respect to the future of this operation, it might, perhaps, be most prudent to allow the future to speak for itself; but having once succeeded in foretelling its success, I am tempted to hazard another prediction. When the operation was yet a mere speculation in the dissecting-room, I had the presumption to anticipate, and say openly, that in ten years the lateral operation for stone would be a matter of history; and in less than half that time the new operation had completely superseded the old one on the only field where they have hitherto come into competition—in this city and around it. I now venture to predict that in less than the period above mentioned, it will be the dominant operation throughout England, counting from the day when the operation shall be first performed in the theatre of a London hospital by a man who has carefully studied it, and is duly qualified to perform it. In saying this I am quite aware that while there are prophecies that bring about their own fulfilment, there are others which have an opposite tendency, and may even prevent the natural course of events by the opposition and hostility which they excite. But I know well what I speak, and have calculated the chances upon both sides. The petty prejudices of individuals by which the operation is now opposed will become every day more feeble, and at length pass away; while what is conformable to truth and to nature is enduring, and will gather every day new strength from every fresh manifestation of its power.

I must, however, be understood to speak solely of England. To those who know Scotland it would not seem strange that a Glasgow invention should find little favour, and should not even be tolerated within the bounds of Mid-Lothian. The example of Mr. Spence, although his operation was successful, never met with a single imitator in Edinburgh; and what is excluded from Edinburgh is excluded virtually from a large part of the ancient kingdom of Scotland. No! I prophecy only of things possible. Dr. Livingstone—a pupil of the Glasgow school—will plant the new operation on the Zambesi and Shirwa; it will pass the famed Hydaspes, and penetrate into China and Japan—and perhaps, thereafter a traveller from those remote regions will re-import it under a barbarous name as the invention of some Eastern sage, and then, but not till then, it will find a ready welcome, and become the dominant operation in the capital of Scotland.—*Med. Times and Gazette, April 7 and* 14, 1860, *pp.* 339, 364.

76.—ON MEDIAN LITHOTOMY.

By Thomas P. Teale, Esq., F.R.C.S., Surgeon to the Leeds General Infirmary.

[Mr. Teale considers median lithotomy "not unlikely to supersede both lateral lithotomy and lithotrity." Mr. Teale observes, that difficulty is often experienced in children in consequence of the membranous portion of the urethra not being perfectly divided; he then continues:]

In children, the left forefinger of the operator is the best dilator, and generally the only one necessary. In adults the question of dilatation demands more serious attention, as the finger alone will rarely be found sufficient, and some further mechanical aid will be required.

The instrument which I have until recently employed has been Weiss's three-branched dilator for the female urethra. The branches of this instrument, which expand by means of a screw, are slender, smooth, tapering somewhat towards their free extremity, at which part they measure, when closed, one-fourth of an inch in diameter, and at their base half an inch. They are four inches in length. With this instrument I have operated six times in adults; and in the majority of these cases found it to answer perfectly, but in some it appeared to be rather too slender and too short, although my object was attained by it. Anticipating the inconvenience that might arise from the use of this instrument in a subject extremely fat, or of large size, or with hypertrophied prostate, I consulted with Messrs. Weiss as to the best mode of remedying its defects. It was thought by us that two instruments, in extreme cases, should be used; one precisely similar to the old one, but with somewhat stronger blades, five inches in length, and a second instrument with broader blades to carry on the dilatation commenced by the first. These instruments were used, immediately after I received them, by Mr. Seaton, of Leeds, and I had the satisfaction of observing that they performed their duty perfectly. Shortly afterwards I used them at the Leeds Infirmary, in a case of unusual difficulty, which I must enter upon at some length.

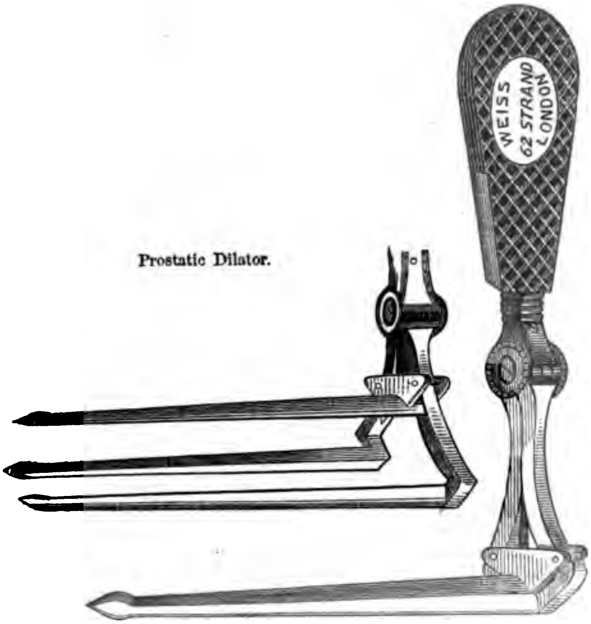
The patient, aged forty-seven, was of very large size and enormously loaded with fat. My finger (which, however, is not a very long one) when introduced into the rectum could not reach the apex of the prostate, but could only feel the staff in the urethra anterior to it. Evidently the bladder was very remote from the perineum, but I quite expected that the new instruments would be long enough to reach it easily. The incision was freely made. The long, narrow-bladed dilator was introduced; and, when it was pressed deeply, so that its base was imbedded in the incision, I felt the stone. After dilatation had proceeded sufficiently, the second dilator, with broader but somewhat shorter blades, was introduced within the branches of the former, which was then withdrawn. The blades of the second instrument were gradually expanded, so as to allow of the full-sized lithotomy forceps being passed within them. Contrary to my expectation, I was now unable to pass the forceps onwards into the bladder, on account of an unusual solid resistance to their progress. I imagined this to be owing to the back part of the prostate not being sufficiently dilated, in consequence of the branches of the second dilator not having reached fairly through the prostate. The staff was again introduced, and the long, narrow-bladed dilator was passed along its groove, and pressed deeply inwards, and expanded. The parts now become sufficiently dilated to allow of the ready passage of the forceps into the bladder, and the stone—not without difficulty, on account of its remoteness from the surface—was seized; and was extracted through the prostate and external parts without any resistance. The stone was of lithic acid, and weighed six drachms. So deeply was it seated, that when it was grasped by the forceps, this instrument, although of the largest size, was buried in the wound to within a finger's breadth of the rings of its handle.

On the third day after the operation the patient died from inflammation of the cellular tissue of the pelvis. The examination after death, conducted by Mr. R. G. Hardwick, house-surgeon of the infirmary, affords so instructive a lesson in reference to median lithotomy, that I cannot too strongly invite attention to it. The body of the patient was extremely fat, the abdominal layer of fat being two inches in thickness. The omentum and the subperitoneal cellular tissue of the abdomen and pelvis were in like manner extremely loaded with it, and a layer of fat one inch in thickness existed between the prostate and pubes. The bowels were distended with air; their peritoneal surface highly vascular, and there was a very slight quantity of lymph upon it. These appearances were more strongly marked towards the lower part of the abdomen. The cellular tissue of the pelvis, about the prostate and back part of the bladder, was evidently torn open, and was ecchymosed, softened, and much infiltrated with serum. The prostate was of natural size and structure. Its urethral surface exhibited towards its vesical extremity, three wounds corresponding with the free extremities of the dilator, which, however, had not been long enough to reach fairly beyond the prostate. An instrument passed through the external wound into the bladder, showed that the distance from the perineum to the cavity of the bladder was five and a-half inches. The facts thus disclosed enable me to give the following explanation of the case. Owing to the extreme distance of the cavity of the bladder from the surface, namely, five inches and a-half, the long, narrow-bladed instrument, five inches in length, could only just reach the bladder when imbedded in the wound. As expansion proceeded, the instrument unavoidably receded to a small extent; the free ends of its blades thus became lodged within the prostate, and bore forcibly upon its mucous membrane. Three wounds were thus produced, which, however, in the aggregate, were no deeper than the incision made in lateral lithotomy. It further appears that the second and shorter dilator, introduced into the track its predecessor, as soon as expansion was commenced, receded from the grasp of the prostate; and in its subsequent expansion, acted upon and tore up the cellular tissue between the prostate and rectum.

On reflecting upon this case, I feel convinced that, if I had been so fortunate as not to be possessed of the second broad-bladed dilator, and had thus been compelled to continue the dilatation to its full extent with the longer narrow-bladed instrument, the operation, even in this unfavorable subject, would have been completed without material difficulty; and that the wounds of the prostate would not have subjected the patient to greater risk than the wound of this part inflicted by lateral lithotomy.

Availing myself of the information thus obtained, I have decided upon using, in future, one dilator only, and for this purpose have had another narrow-bladed instrument constructed by Messrs. Weiss, stronger and longer than the former one. The blades of this instrument are five and a-half inches in length, terminating in a slight bulb, which would check the tendency they might otherwise have to recede from the bladder. The dimensions of the instrument, of which the engraving gives a good representation, are the following: The blades five and a-half inches long, their conjoint diameter when closed, three-quarters of an inch at the base, and half-an-inch at the bulb.

The Act of Dilatation next requires careful attention, as upon its efficient performance the safety of the patient mainly depends. The dilator being introduced, the staff withdrawn, and the handle of the dilator turned up towards the abdomen, the surgeon, or an assistant, slowly, and at short intervals, expands the blades of the instrument. The prostate generally offers only moderate resistance, while the chief obstacle to expansion exists in the parts external to it, namely, the fibrous structures of the perineum. The chief pain in this part of the operation is caused by the stretching of these fibres. To facilitate, therefore, their yielding, and to diminish their pain, they should be gently touched with the edge of the knife, either inferiorly or laterally. When the dilation of the parts external to the prostate has been carried far enough it will be found that the prostate itself has not been dilated quite to the same extent, on account of a slight yielding of the free extremities of the instrument; but it will generally have yielded enough to



Prostatic Dilator.

allow the large lithotomy-forceps to be introduced into the bladder, between the blades of the dilator, which may then be withdrawn. Should the stone be found to be very large, the blades of the forceps may be used for still further dilating the prostate. This is easily effected by withdrawing the forceps a little, and then expanding its blades by repeated gentle efforts. The stone being seized, the process of extraction is effected by a moderate rotatory and tractile force, aided when necessary, by slight touches of the bistoury applied to resisting bands of fibrous structure. It is better thus to overcome the resistance than by violent traction.

In this operation, as in the lateral, it is important to use forceps of rather large size relatively to the subject, as the stone is more easily seized by a large than by a small forceps. The blades, being smooth and broad, greatly protect the parts transversed from the rough surface of the stone.

Concluding Remarks.—It is an interesting question by what process the prostate, in median lithotomy, is widened so as to allow of the extraction of a stone through it? Is it by dilation, or by laceration, or by both? At present, I presume, this question cannot be answered. Extensive observation, after death, of fatal cases can alone solve it. Experiments on the prostate in the dead body would prove little, as there is, I doubt not, a wide difference in the dilatability, and in the proneness to laceration, of the living and of the dead prostate. It is, however, probable that in the living body the process which suffices for the extraction of a stone of moderate size is one of simple dilatation; and that, in the case of a large stone, it is attended with some solution of continuity both of the mucous membrane and of the proper structure of the prostate. But, in this respect, it will only be analagous to that which must take place, from the forcible stretching of these parts, when a large stone is removed by lateral lithotomy; and moreover, it is unattended with the casualties incident to a deep thrust of the knife.

As to the relative fatality of the two operations, which alone can determine their respective merits, it is too early to speak. Prolonged and varied experience of the median is required. But, when the lateral operation in adults is known to be attended with a sad fatality, it is the clear duty of surgeons to try fairly other methods which afford a reasonable hope of better results. If the median should hereafter prove to be less fatal than the lateral, it must of necessity supersede it. If, on the contrary, the results of the median should be less favourable, it will become our bounden duty to relinquish the median operation.

As far as my own experience goes, the result of six recoveries in seven operations on the adult is favourable to the further testing of its merits. Difficulties may and will arise; but we must ascertain whether such difficulties are or are not insuperable.

It has been supposed that the median operation is applicable only to cases of small stones. My own impression is, that the largest stones, admitting of being extracted through the perineum, may be removed by the median quite as readily as by the lateral operation, provided that the resistance afforded by tense perineal fibres be relieved by gently touching them with the knife. In one of my cases, there were two stones, conjointly resembling in form an egg cut in two. These were seized and extracted as one stone, and were removed with perfect ease. They conjointly weighed 1⅝ ounce, and measured four and a quarter inches in their smallest circumference. The stone which I saw my son extract weighed 3¼ ounces, and measured five and a-half inches in its smallest circumference. This stone was removed with about the same degree of extracting force as is usually required in the lateral mode.

It is stated by some surgeons that the operation is nearly bloodless. I have seen cases in which scarcely any blood was lost; but in most instances the bleeding at the time of operation was about the same as in lateral lithotomy The subsequent draining, however, was much less. But, if bleeding of a more formidable character should occur after the median operation, we may feel assured that it does not proceed from large or from deep-seated vessels; and, moreover, the bleeding vessels may be rendered accessible to ligature or compression, by the aid of the three-branched dilator. In some cases I have thought that the stone was not seized so readily in the median as in the lateral operation; but on this point I would reserve an opinion until I have had further experience.

In conclusion I will take a brief survey of the changes that have occurred in my own views and practice, as to the treatment of stone in adult males, since my first operation in 1826. These successive phases may be thus arranged:—

1. Lateral lithotomy in all cases.
2. Lateral lithotomy the rule, lithotrity the exception.
3. Lithotrity the rule, lateral lithotomy the exception.
4. Median lithotomy the rule, lithotrity the exception.

In the early part of my practice lateral lithotomy was alone thought of. As soon, however, as lithotrity received its last great improvement from the introduction of the screw-lithotrite, I adopted the operation; first in select cases where the stone was small: afterwards it was extended to stones of larger size, until it became, in my estimation, the operation to be generally preferred, leaving, however, a large number of exceptional cases, in which the stone was great, or the prostate much enlarged, or the bladder highly irritable, for lateral lithotomy. The next change was caused by the revival of median lithotomy in an improved form; and from the limited experience I have yet had of this operation, I feel a strong conviction that it will be the one which I shall generally adopt, leaving lithotrity to the few exceptional cases suitable for it, in which the patient has an insuperable objection to the knife.

It may be asked, Why was lateral lithotomy superseded in part by lithotrity? In reply, it may be said that the acknowledged fatality of the former operation induced surgeons to test the value of any other proposal which held out a better hope of success. In my own practice, thirty-five operations of lateral lithotomy in adults were attended with thirteen deaths, or one death in two and two-thirds of the cases. By the introduction of lithotrity, the result is improved by the addition of fifteen cases, with only one death, giving a result from lateral lithotomy and lithotrity conjointly, of fifty operations in adults, with fourteen deaths, or one death in three and a half cases.

If the question were now asked, Why I would resort to any other operation when lithotrity has furnished me with a result of fifteen cases with only one death, I would reply, that in each case so treated, the patient has generally had to submit to several operations or "sittings," and that any one of these sittings might have been, as some of them were, followed by symptoms severe enough to cause serious anxiety.

In the hope of diminishing the risks to which, from the plurality of operations, the patients subjected to lithotrity are exposed, and of obtaining better results than lateral lithotomy affords, I have been desirous of giving a fair trial to the median operation.

I have now operated on adults seven times, three of these operations being performed on the same individual. The result of these has been one death in seven operations. If such a portion of success can be maintained, it would be a great gain, as far as my own experience goes, upon lateral lithotomy, or even upon lateral lithotomy and lithotrity conjoined. I have great expectation that such will be the result; for, if we except the fatal case already described, presenting extraordinary difficulties, which might probably be overcome hereafter, the remaining six recovered so favourably that it was difficult to believe the patients had been subjected to a grave operation. They formed a marked contrast with the adult patient who had undergone lateral lithotomy, and who subsequently recovered.

I have no hesitation in extending the median operation to children, although lateral lithotomy in children has presented me a result of eighteen operations without a single death. Yet a considerable number of these patients were seriously ill, and caused me much greater anxiety than has been felt for the few young subjects whom I have seen under the median operation.

[Mr. Teale very properly brings forward the following fatal case, which occurred subsequently to writing the preceding paper. The patient was a gentleman fifty-two years of age.]

The perineal incision being made, the urethra opened, and the resisting bands formed by the deep fascia divided, the three-branched prostatic dilator, was passed along the groove of the staff into the bladder, which it entered without the least resistance. The staff was then withdrawn and the process of dilatation commenced. Immediately after dilatation was fairly begun, there was the usual gush of urine from the bladder. The process was then continued very gradually, to such an extent only as to admit the forceps—a straight pair—which passed readily into the bladder. On exploring the bladder very carefully with the forceps, I was unable, for a considerable length of time, to feel the stone, but at length perceived a rub against its surface high up towards the pubes. I was, however, unable after several attempts, to succeed in getting it within the grasp of the instrument. The straight forceps was now withdrawn, and a curved pair introduced. With this instrument, the blades being directed upwards towards the pubes, the stone was struck, seized, and extracted. It weighed two drachms, and was of oval form, very much flattened. Scarcely any blood was lost. The patient on being placed in bed was free from pain, and appeared in a most favourable condition.

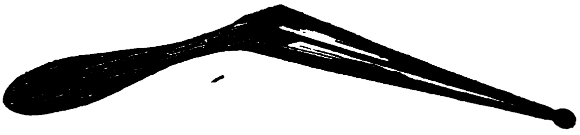
Two hours after the operation, before leaving by train, I visited him along with Dr. Wright and Mr. Hartley, and found him suffering from severe pain in the pelvis. The urine had not begun to flow. A few days afterwards I received a letter from Mr. Hartely informing me that the "pain" of which the patient complained at our visit on the afternoon of February 9th, "never left him; the urine came freely before ten o'clock in the evening, but there was no relief of pain. Peritonitis was evident next morning. He died at 10·15 on the evening of February 10th." No examination after death was obtained.

Remarks.—By this fatal case, the result of median operations in the adult performed by myself is two deaths after eight operations, and of such operations in the adult performed by Leeds surgeons, three deaths after sixteen. The very slight local or constitutional disturbance in my first six cases, led me to prefer median lithotomy to lithotrity in this instance. In my paper of December 10th it is stated, "In some cases I have thought the stone was not seized so readily in the median as in the lateral operation; but on this point I would reserve my opinion until I have had further experience." In the present case it was impracticable to seize the stone with the ordinary straight forceps. That there would have been greater facility of seizing it under the lateral operation, I do not presume to say; but the difficulty on this occasion was undoubtedly great. Probably the difficulty may have been increased by the peculiar form of the stone,—an oval much flattened.—*Med. Times and Gazette, Dec.* 10, 1859, *and March* 10, 1860, *pp.* 573, 232.

77.—ON A PROBE-POINTED GORGET FOR MEDIAN LITHOTOMY IN CHILDREN.

By Thomas P. Teale, Esq., F.R.C.S., Surgeon to the Leeds General Infirmary.

[Mr. Teale describes this instrument in relating the following case. The patient was nearly three years old, admitted into the Leeds Infirmary on Dec. 10th.]



Probe-Gorget.—The proportion is exactly two-fifths of real size.

Before operating I requested Mr. T. England, of Leeds, to make me a probe-gorget, as a guide for the finger into the prostatic urethra. The blade of this instrument is three inches in length, and may be described as a hollow half-cone, three-fourths of an inch in diameter at its base, terminating at the apex in a bulbed probe half-an-inch long.

I operated on this child on December 17, and removed a stone the size of a chestnut, or, to speak more definitely, two and a-half inches in its smaller circumference, and three and a-half in the larger.

The instrument easily glided along the groove of the staff so far as to allow the tip of my left forefinger to be lodged within the prostate. The finger was then pushed onwards while the guide was withdrawn. The tip of the finger readily touched the stone, which was seized and extracted, with perfect ease, the operation being completed in little more than one minute. The child has not ailed anything since the operation, and at the end of a week is playing cheerfully in the ward.

As it is evident that the median operation will receive extensive trial, it is important to come to an early decision as to its details. In the hope of promoting this object, I venture to add another to the various instruments already submitted to the profession by Mr. Bowman, Mr. H. Thompson, and Mr. Armstrong Todd.

The instrument, which I have now described, has been found practically efficient in the removal of a large stone from a young subject. My colleagues and others who were present at the operation have kindly expressed their full approval of it. It thus appears that a probe-gorget has been employed in median lithotomy both by Mr. H. Thompson and myself, quite independently of each other, and with a satisfactory result in both instances.

In conclusion, I beg to offer the two following remarks :—

1st. That the "probe-gorget" here recommended, as well as the instruments recently described by Mr. Bowman and Mr. H. Thompson for use in children, should be regarded less as dilators than as guides for the finger, which in young subjects is the real dilator.

2nd. That the name of "prostatic dilator" is more strictly applicable to the instrument described in my paper of December 10, and I presume also to Mr. Armstrong Todd's instrument. [See Article 81 of this vol.]—*Med. Times and Gazette, Dec.* 31, 1859, *p.* 663.

78.—ON THE MEDIAN AND LATERAL OPERATIONS OF LITHOTOMY.

By John Erichsen, Esq., F.R.C.S., Surgeon to University College Hospital.

[It is a curious fact that the operation of median lithotomy was introduced some three or four hundred years ago, and continued to be practised up to the middle of the last century, when it fell into disuse upon the introduction of the "safer and simpler" plan of Cheselden. Mr. Erichsen prefers the usual lateral operation to the median one now again brought into practice by Mr. Allarton. He says:]

I have found it advantageous to modify Mr. Allarton's operation in one or two points. The first, I think of some utility, is to use—which I did in the last case in which I cut by the median operation, about a week ago—instead of the ordinary curved staff, a rectangular one, grooved from about one inch above the elbow nearly to the point, corresponding to the "Glasgow staff" introduced by Dr. Buchanan. Its use and advantages are, that, when in the bladder, the angle rests against the apex of the prostate and can be felt in the perineum, and you can judge of the exact point where to enter the knife (directing it so as to open the groove just below the angle), which you cannot do with the curved staff; the incision upwards also is limited, and there is, besides, less danger of wounding the rectum, the urethra being drawn upwards away from it, and not pushed down against it as with the ordinary staff. The knife should be straight backed, having the blade not more than two inches long; you can then tell to what depth you have entered it. I have found it advantageous in practice to carry a beaked director, shaped like a large hernia director, along the groove, after the incision has been made in the urethra, so as to open up the canal and thus to clear the passage for the finger. After having opened the urethra, I think it is better to dilate the prostate before withdrawing the staff; by pushing the finger slowly, with a rotatory movement, along its side, you get into the bladder with more ease and certainty; whereas, if you only use the probe, you may find it not stiff enough, and you are apt to push the bladder before you. It has been proposed to employ mechanical means of dilatation, instead of the finger, to open up the prostate and neck of the bladder; and I have had some dilators constructed by Mr. Coxeter on the principle of the two-bladed dilator of the female urethra. I have used them on the dead subject, as some of you have seen, but I have not ventured to use them on the living, lest, by the employment of screw-power, the same deep lacerations of the neck of the bladder should result that were so fatal in the old Marian operation. Indeed, I believe that their use would be fraught with danger from their liability to occasion rupture of the neck of the bladder, and if persisted in, I cannot but fear that they will bring discredit on the operation, reducing it to the condition of the old Marian, and repeating the dangers of that procedure. No safe dilatation can be effected except by the finger, with which no harm can be done, whilst it appears to me that the greatest possible mischief may be done with screw dilators.

In order to make a proper comparison between these two operations—the median and the lateral—we must take them seriatim. Now what are the dangers of the lateral operation? 1st, the difficulty in some cases of getting into the bladder; 2nd, hemorrhage; 3rd, the risk of wounding the bulb; 4th, of wounding the rectum; 5th, of too extensive an incision in the prostate, and opening up of the pelvic fascia; 6th, the difficulty in extracting the stone.

With respect to the general ease and simplicity of the operation, there is no doubt the surgeon will be more skilful in that operation which he has more often performed: so far as ease and complicity are concerned, there is no great difference between them. In the lateral operation there is very seldom any difficulty in getting into the bladder, though surgeons have sometimes been foiled in this; but surgeons of the greatest skill have also had great difficulty in getting into the bladder in the median operation. The bladder tends to get pushed upwards and backwards before the finger, especially in children in whom the prostate is not developed; and unless the neck of the bladder be well opened, there appears to me to be a danger of tearing across the membraneous part of the urethra, and of pushing the separated bladder on before you. So far as facility in entering the bladder then is concerned, the two operations may be placed much in the same category.

As to the second danger—hemorrhage. In this respect the median has decidedly the advantage. If the incision be made in the middle line, without wounding the bulb, although there may be tolerably free bleeding at the time, yet there is no vessel that can furnish a dangerous consecutive hemorrhage; whilst in the lateral there are the dangers of arterial and of profuse venous hemorrhage, the knife coming into close relation with the artery of the bulb and others of some size. If the object were, therefore, simply to save blood, the median is so far better than the lateral. But after all, the danger of excessive hemorrhage in the lateral operation is but very small. With care, it will rarely happen that the patient loses a dangerous amount of blood.

Point third—wound of the bulb. This may occur in both, but is more difficult to avoid, and, indeed, very likely to happen, in the median, as the bulb sometimes so overlaps the membranous part of the urethra that it is difficult not to cut it; whilst in the lateral operation, by cutting low down, and entering the groove of the staff well back, and from below upwards, this may always be avoided. It is true that division of the bulb in the mesial line seldom gives rise to much hemorrhage, but cases have occurred to my knowlege, though not in my practice, in which patients have died from this cause after the perineal section, the blood regurgitating back into the bladder, and filling that viscus.

With respect to the fourth point of comparison. The rectum is not in much danger in the lateral operation, unless it be distended. In the median, on the other hand, the rectum is in considerable danger. If you perform this operation on the dead body, you will find the back of the bistoury very, I may say uncomfortably, close to the finger in the rectum; and if you place another finger in the wound, you will find them come into very close apposition just anterior to the prostate. In the old Marian operation, the rectum used to be very frequently cut, air and fæces issuing from the wound.

Firstly, as to the treatment of the prostate. I think this is very nearly the same in both operations. All are agreed that in the lateral operation but a limited incision should be made in the prostate and neck of the bladder, the opening being dilated with the finger, so as to avoid wounding the capsule of the prostate, and the opening up of the pelvic fascia. The difference between an incision that opens the capsule of the prostate, and dilating this structure by the finger, is very great. The great object in lateral lithotomy is not to open up the pelvic fascia, and it is difficult, if not impossible, to tear this with the finger. Take an aponeurosis out of the body; you will find it very difficult to tear; but touch it ever so lightly with the knife, it separates at once. So, in the median operation, you may dilate the prostate to a considerable extent without opening its capsule. I have used the word "dilate," but dilatation is an erroneous term. If you look upon the prostate, which is one taken from a dead subject on which I have performed this operation, you will find it to be torn—not simply dilated, but lacerated. As you see in this preparation, their is an actual laceration of the substance of the prostate, but not extending into or through its capsule. A laceration of the substance of the prostate is of no consequence, and only becomes dangerous when it amounts to rupture of the capsule, when it exposes the patient to the fatal accident of extravasation of urine and diffuse inflammation of the pelvic fascia. Now, in the lateral operation, in running the knife down the groove of the staff, you may, certainly, unless care be taken, go beyond the limits of the prostate, and thus expose the patient to all these dangers. In the median, this cannot be done, if the knife be not used after the urethra is opened, the prostate being dilated solely with the finger. So far as this point, then, is concerned, I think that the median may be regarded as safer than the lateral operation, it being *impossible* to open up the pelvic fascia with the finger in the median, whilst they *may* be opened by the knife in the lateral.

Now for the last and most important part of the operation—the getting the stone out of the bladder. In lithotomy, your work is only half completed when you have entered the bladder; the most important part, that for which the operation is undertaken—the removal of the stone—has yet to be done. Here, I think, the lateral presents a decided and unquestionable superiority; so much so, that it must ever prevent the median becoming *the* operation for stone.

In performing the median operation you will find three points, or rather planes of obstruction, between the surface and the interior of the bladder. The first is occasioned by the transverse muscles of the perineum, and, perhaps also by the under portion of the deep perineal fascia. In the lateral operation you cut across this plane, and lay open the ischio-rectal fossa, giving abundance of room for the manipulation of the forceps and the extraction of the stone, along the base of the triangle formed by the rami of the ischium and pubic bones. But in the median you have to extract towards the summit of this space, at the apex of a narrow triangle, having the transverse muscles forming a tense bar along its base, and offering a material obstacle to the introduction of the forceps and the extraction of the stone. The second obstacle lies in the prostate; but as it is easily removable by dilatation, it cannot be considered a serious one.

The third, the deepest and most important, is situated at the neck of the bladder. In this preparation you find here a narrow, tense ring at the neck of the bladder beyond the prostate, and this bar remains intact in spite of the dilatation and laceration to which the prostate has been subjected. On introducing the finger, you will find it grasped tightly by this ring. This inner ring of the neck of the bladder cannot be dilated beyond a certain point. I have found by experiments on the dead subject, that it cannot be expanded to a size more than sufficient to extract a calculus of one inch in diameter without laceration or incision. The existence of this ring is the greatest barrier to the extraction of the stone, and its laceration or rupture is well known as one of the most dangerous and fatal accidents in lithotomy. It is in consequence of the obstacle offered by this that the median operation is not available for the extraction of large calculi. Such a calculus as that you saw me remove by the lateral the other day, two inches in diameter, could not have been extracted by the median operation without the employment of a serious, probably fatal, degree of violence, because it could not be got out without laceration or rupture of the ring of the bladder. But it may be said, what is easier, when the finger is in the bladder, than to push a probe-pointed bistoury along it, and cut downwards and outwards, through these structures into the ischio-rectal fossa, if the stone be large? I answer, nothing could be easier or more simple; but what would be the consequence? Why you at once reduce the median to the condition of the lateral operation. A free incision in the neck of the bladder and prostate increases the tendency to hemorrhage, opens up the fascia, and exposes the patient, in fact, to all the dangers of an ill-contrived lateral operation, destroying at once and altogther the principle of the median operation—viz., dilatation and not incision; and if you do not gain space by incision, but attempt to extract a moderately large stone by dilatation of the parts, you will certainly not succeed, but your dilatation will end in a laceration, not only of the substance of the prostate, which is safe, but of the neck, and, perhaps, the base, of the bladder, which will be fatal.

In conclusion, then, it appears to me that the median operation, when performed in suitable cases, has the advantage over the lateral of being attended by less risk of hemorrhage, and with less danger of injury to the pelvic fascia; but that in consequence of the very small size of the opening that can be made in the bladder by it, *provided these advantages are maintained*, it is only applicable to certain classes of exceptional cases, and that it cannot be substituted for lateral lithotomy as the general operation for stone in cases not admitting of lithotrity.

My opinion as to the particular cases in which the median operation may with propriety be performed is this: In cases of foreign bodies, such as pieces of bougie, bits of tobacco-pipe, &c., being lodged in the bladder, the median is preferable, the body being small and easily extracted; so also for stones not exceeding one inch in diameter. But then, it may be said, calculi of this size can generally be safely subjected to lithotrity. That is true; but, in certain cases, lithotrity is not admissible; so that the median operation becomes limited to cases of small calculi in which lithotrity cannot be practised in consequence of irritability of the bladder, or of the patient being too weak to bear it. If a small calculus be lodged just behind the prostate, in a pouch which occasionally forms at the lower fundus of the bladder, you may come down on it at once by the median incision. In cases also in which lithotrity has been performed, and the patient is unable to expel the fragments, you may perform the median operation, and readily extract the detritus by the scoop, as it lies behind the prostate. But where you have to do with a stone of large size, the median is not, in my opinion, safe; such an amount of traction must be used as will infallibly bruise and lacerate the neck and base of the bladder, and expose the patient to infiltration of urine and deep pelvic inflammation—to all the dangers, in fact, of the old Marian operation; dangers which were so great that more than half the patients subjected to it perished, and which caused it to be abandoned for the lateral.

Finally, in cases in which the patient is so anæmic that the loss of an additional ounce or two of blood might turn the scale against him, the median is preferable to the lateral, as although it is by no means almost a bloodless operation, as is supposed by some, yet there is less hemorrhage during the performance, or rather, perhaps, less oozing after its completion, than in the lateral, and there is certainly not the danger of the profuse bleeding that is sometimes seen in that operation. It was for this reason that I performed the median in the last case that was under my care in private, and I am glad to say with success.

I have said nothing special about the performance of the median operation in children. Under the age of puberty, lateral lithotomy is a very successful operation; certainly the most successful of all the *great* operations in surgery. The median can scarcely be superior to it in this respect; and unless it can be shown to possess decided advantages over the lateral in ease of execution, it does not appear to me to be desirable to abandon an operation of proved safety for one that is still on its trial.—*Lancet, Dec.* 17, 1859, *p.* 607.

79.—ON MEDIAN LITHOTOMY.

By Wm. Bowman, Esq., F.R.C.S., Surgeon to King's College Hospital.

[Describing a prostatic dilator prepared for him by Mr. Weiss, Mr. Bowman observes:]

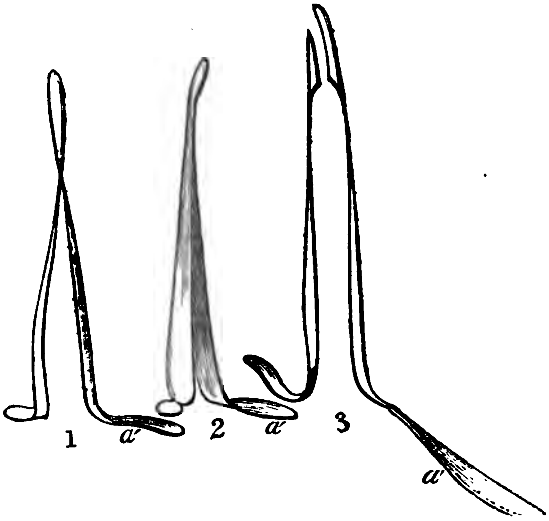
It consists simply of two blades, either united together at one extremity, or fitting one into the other there, and terminating in a slightly-curved probe-point, adapted to enter the bladder readily from the perineal wound along the groove of the staff. The blades being of thin steel, form a sufficiently rigid stem when closed, and admit of separation with flexion by the finger run in between them; thus, of course, dilating the prostatic canal transversely without any forcing forwards of the neck of the bladder. The principle involved is the same as that of Mr. Holt's excellent stricture-dilator, which probably gave me the idea. The blades are of such a length as to extend from the external wound a little way within the prostate, and the probed extremity reaches from half an inch to an inch beyond. The blades in diverging from the point of union, become gradually broader, and are a little hollowed on their opposed surfaces, so as to fit the finger. At their free ends they are curved outwards at an angle, and one of them is extended into a handle, which is to be firmly held by the right hand, while the left forefinger is being introduced as a dilator between the blades.

It is well known to lithotomists that in young children there is some risk of thrusting the neck of the bladder before the finger, as the latter seeks to enter its cavity, after the deep incision, and that operators deficient in coolness or dexterity have on rare occasions failed in consequence to enter the bladder at all, or have done serious, and even fatal, damage to its attachments. In such a difficulty, the dilator now devised might be of use. What is customary in such a case is to introduce the knife afresh along the staff, and extend the incision in the prostatic region—a proceeding of some hazard in these young subjects, and probably much less safe than to dilate.

Recent opinions have more and more concurred in prescribing, with Brodie and Liston, a very limited incision of the prostate, and an extension of the opening by means of the finger, the blunt-gorget, or the blades of the forceps in withdrawing the stone. It is admitted on all hands that the prostatic canal is highly dilatable, or at least that it may be split open with little danger when partially divided by the knife; and the safety of the modern lateral operation is acknowledged to depend mainly on the limited extent of the deep incision. Where a cutting instrument is run along the staff into the prostatic canal, it is in the nature of things that in the best hands it may sometimes wound more deeply than is intended, either by falling too much away from the groove of the staff, or by going too far into the bladder, and by having an inclination a little different from the intended one. Hence, occasionally, cellular infiltrations or hemorrhages, adding heavily to the anxieties of the surgeon in the after treatment, and perhaps proving fatal.

I cannot but think that the median operation, aiming as it does only at an incision in the middle line, where no damage can be done, and carried only up to—not into—the prostate, must be highly worthy of adoption, unless experience should prove that it is attended by insuperable dangers more than counteracting its obvious advantages, and greater than those of the lateral operation. Dangers there undoubtedly are, as experience has proved, in attempts to dilate the neck of the bladder. I believe the dangers of slow dilatation are necessarily very great, modifying, as that process must do, the circulation of this important region, inducing inflammation in the neighbouring tissues, infiltrations, or venous contaminations. There may be dangers also in rapid dilatation as proposed by Mr. Allarton; but I confess it appears to me that the efforts of surgeons should be now directed to test these dangers, to avert them, as it seems quite possible to do, and thus to secure the removal of the stone with the least risk to life. There is no subject in surgery which requires to be considered from so many independent points of view as that of stone, for one case differs from another in the most important and essential particulars. A deep perineum, a large prostate, a bulky stone, no less than the varieties of age, are conditions materially altering the whole character of the operation; and I am inclined to think that further study of the method of dilatation following an incision of the urethra in the median line up to the prostate, will cause that mode of operating to gain ground in the estimation of surgeons.

To revert to the median operation of Mr. Allarton in young children, I lately found the same difficulty which Mr. Teale speaks of in carrying the finger along the staff or probe into the bladder after the incision of the membraneous part of the urethra. In those subjects the prostate can hardly be felt by the finger in the rectum, and as the finger so placed runs nearly parallel to the membraneous part of the urethra and neck of the bladder, it must be a little uncertain at what exact point the knife thrust in between the urethra and rectum enters the former channel. If it does not enter quite close to the prostate in cutting forward, so as to divide the lower wall of the urethra, it must leave a little of the membraneous part of the canal undivided, as well as an important part of the deep fascia, and it becomes desirable, as Mr. Teale suggests, to carry the knife a little backwards up to the prostate in such cases. At least if this be not done, it will be found very difficult to insinuate the finger along the staff or probe, whether along its upper aspect or its side, so as to enter the bladder; for the end of even a small finger is too large and blunt an instrument to easily find the orifice of so small a canal as this in young children. In these subjects also the ligaments and fasciæ holding the neck of the bladder in place are so slender as to yield and tear much more readily than in the youth or adult. Hence the idea of the dilator now described.



FIGS. 1 and 2.—Two views of the Dilator with united blades.
FIG. 3.—Form of a Dilator on larger scale with moveable blades.
aaa.—Handles to be held by the right hand, while the left forefinger moves up between the blades.
(The figures are on a reduced scale.)

There are two forms of it for trial. In the first, the blades are joined, and after the dilatation is complete, the finger would have to be withdrawn in order to disengage the instrument, though the bladder might probably be first explored by it, to ascertain the position and size of the stone. In the second form, one blade is received into the other near the probed extremity, and the blades thus admit of being withdrawn in succession by the right hand, while the left fore-finger is still in the bladder.

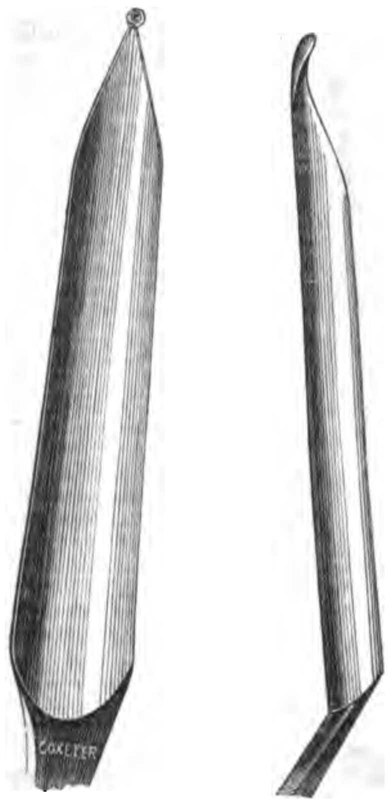
The whole length of the dilator for the youngest children is three and-a-half inches; a larger scale might be adopted for young subjects of somewhat greater age.

With regard to dilators for the median operation in adults, I have proposed to Mr. Weiss to make one on the same principle as the foregoing, but of such a size as to be opened, not by the finger, but by an ovoid plug, to be run in between the blades, of such a size as might be required in various cases according to the size of the stone. Without at all intending to prejudge the question, I think it worth trying whether an instrument on this principle would not offer some advantages over the three-bladed dilator as modified by Mr. Teale.—*Med. Times and Gazette, Dec.* 17, 1859, *p.* 614.

80.—ON MEDIAN LITHOTOMY.—NEW DIRECTOR.

By Henry Thompson, Esq., Surgeon to University College Hospital.

[The following case occurred to the writer at University College Hospital. He describes a new instrument used by him for the purpose of facilitating the proceeding.]



The patient was only three and a-half years old; and anticipating, therefore, that without some special means of carrying the finger along the staff from the membraneous portion of the urethra into the bladder, considerable difficulty might be encountered, I had a director made for the purpose. The idea was suggested by the director which I have employed in circumstances somewhat similar—viz., for the purpose of facilitating the passage of a large catheter into the bladder after Syme's operation for stricture of the urethra, and figured in my work on 'Stricture,' second edition, p. 295. In the present case I added a probe point to run along the staff, and made the whole sufficiently wide to meet the necessity of the case—viz., about half-an-inch at the extreme breadth, but gradually tapering to the probe point aforesaid. The concavity is also considerable, to receive and direct the forefinger. The adjacent figure represents the instrument of the natural size. It now more resembles a gorget, but is taper throughout and thicker, so as to prevent the possibility of its edges cutting the tissues by which it passes.

Having made the incisions strictly in the manner recommended by Mr. Allarton, I introduced the director with the concavity upwards into the groove of the staff, and pushed it gently on into the bladder. I had now no difficulty in sliding the left forefinger along into the cavity and feeling the stone which was then extracted with very little difficulty. It was perfectly spherical and about the size of a marble. The child is doing admirably well. I should say that Mr. Bowman kindly lent me the instrument described in the preceding article, and that I should have used it had not the other appeared to accomplish fully the desired object.

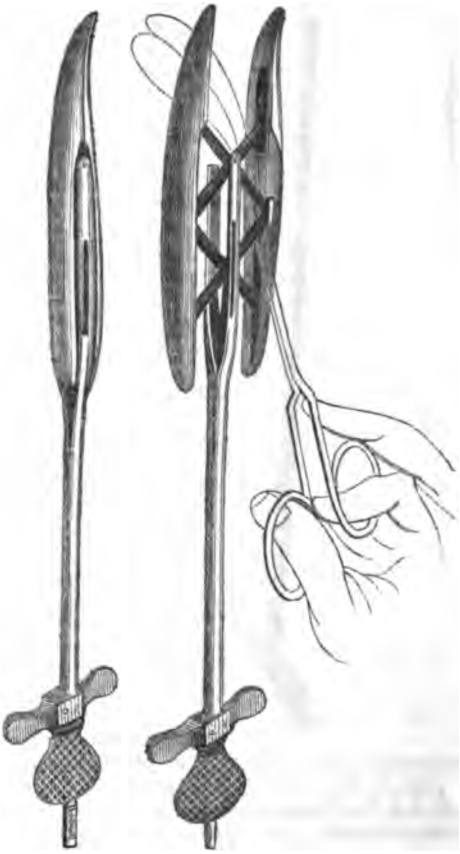
Perhaps I may be permitted to add, that when reviewing this subject at length in the 'Medical and Chirurgical Review' four years ago, in connexion with the works of Mr. Allarton and others on lithotomy and lithotrity, I expressed an opinion in favour of the median operation, and a belief that it was well adapted to many cases in which the lateral operation, and to some in which lithotrity were then applied. All I have seen since that time tends to confirm that opinion, and the experience which Mr. Teale has so admirably expounded in his recent paper, must be considered as most important evidence in behalf of median lithotomy, and to warrant the increasing favour with which it appears to be regarded by many members of the profession.—*Med. Times and Gazette, Dec.* 24, 1859, *p.* 638.

81.—ON MEDIAN LITHOTOMY: A NEW PROSTATIC DILATOR.

By Armstrong Todd, Esq., Surgeon to University College Hospital.

[The instrument recommended by Mr. Todd is on precisely the same principle as that used by him in strictures of the rectum.

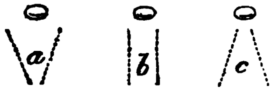
The blades, of course, are small and turned up at the extremity in the form



of a beak so as to run in the groove of the staff. Mr. Ferguson, of Giltspur-street, has made them of various sizes so as to suit the child or adult. For the mechanical description the reader is referred to the last volume of this 'Retrospect,' p. 144; the only difference which exists between the two instruments is the shape and size of the blades,—in fact its adaptation to the urethra, instead of the rectum, being made suitable for introduction through an artificial opening of small dimensions guided by a grooved staff. The principal benefit which I anticipate will be found from the use of this instrument is that the neck of the bladder and prostatic portion of the urethra are permanently kept dilated during the operation, permitting the introduction of the forceps and their free movement in the bladder, enabling the surgeon to search for and grasp the stone. The parallel dilatation, also afforded by this instrument, I think, will be considered of great advantage.

In cases where the stone is so soft as to crumble under the forceps, it becomes a difficult matter in median lithotomy to wash out the bladder, owing to the power of recontraction possessed by the neck of the bladder and prostatic urethra. It is, therefore, here very necessary to have an instrument, which while it keeps the parts dilated, permits of the introduction of the syringe, and allows of free exit to the washed-out particles of stone. A case of this kind occurred to my friend Mr. Lloyd, not long ago, at St. Bartholomew's Hospital; and he found it was impossible to wash out the bladder, owing to the cause above stated. The stone was so soft that but small portions could be removed at a time, appearing like mortar upon the blades of the forceps.

Mr. Bowman's instrument can hardly be called a dilator; it is evidently but a guide for the finger, which is in reality the dilator. Its great use, and perhaps its only use, is to obviate the danger of displacing the parts, and to prevent the production of intussusception of the neck of the bladder, which might occur by using the naked finger. This form of instrument appears to me to have one great disadvantage—which is, that it dilates the superficialparts more than the neck of the bladder; in fact, the dilatation is of a conical form,—the base externally, and the apex internally, at the neck of the bladder, which obviously is the most dilatable portion of the canal through which we have to pass. Now it is evident that it is more difficult to draw a body such as a stone through a flexible conical canal, from apex towards the base, than from base towards the apex, as may be seen by the adjoining woodcut. Nay, more, the traction from apex to base in this case is likely to produce displacement and intussusception, quite as dangerous as, although in opposite direction to, that feared from the introduction of the finger without a guide. With the dilator which I propose, the movement is parallel, as I said before, but it could easily be made to dilate the neck of the bladder more than the superficial parts, so as to make the base of the cone internal, and the apex external, if such be deemed necessary.



The mode of using this instrument appears to me to be simple:—When the urethra is opened upon the staff, the dilator is passed along the groove, and the staff being withdrawn, it is properly adjusted, and the blades made to separate by means of the thumbscrew. When sufficient dilatation is obtained, the forceps are passed along between the blades, and dipped down in front of the crossbars into the bladder, moved about until the stone is felt and firmly grasped; the thumbscrew of the dilator should be then turned, so as to approximate the blades a little, and the instrument withdrawn, leaving the forceps and stone to follow in its track.—*Medical Times and Gazette, Dec.* 24, 1859, *p*, 640.

82.—CASE OF MEDIAN LITHOTOMY; REMOVAL OF THREE STONES, TWO OF THEM BEING ENCYSTED.

By R. STEEL, Esq., M.R.C.S., Surgeon to the Blaenavon Ironworks.

[The case was in a little boy, aged nearly five years. From the symptoms the stone (which was readily detected) must have existed nearly two years.]

The patient being placed in the usual position for lithotomy, a staff grooved on its convexity was passed, and a straight bistoury was introduced, with its edge upwards, into the median line, about half an inch above the anus, so as to strike the groove in the staff; the knife was then carried upwards along the groove, dividing the membraneous portion of the urethra, and withdrawn, cutting upwards through the integuments. The bottom of the wound was about three-quarters of an inch, and at the surface about an inch and a quarter, in length. A few fibres in front of the posterior layer of the deep perineal fascia were scratched through; a long ball-pointed wire was passed into the bladder, the ball being kept well pressed into the groove of the staff; the latter was then withdrawn, and the forefinger of the left hand, well greased, and guided by the wire, passed into the bladder by a gentle rotatory motion, without the least difficulty. On withdrawing the finger, a stone presented at the opening into the bladder, and was at once, and without difficulty, extracted by the forceps. It was of an ovoid shape, and measured one inch by three-quarters in diameter. I then explored the bladder with my finger, and on the anterior wall, high up in the pubic region, it came in contact with another stone; the only portion to be felt was a small glenoid cavity, about a quarter of an inch in diameter; the rest was firmly imbedded in a cyst. After two or three minutes' cautious manipulation with the forefinger (fortunately a long and flexible one), the stone was turned out of its bed, and fell to the fundus, and was at once removed; it measured three-quarters by half an inch in diameter. On again exploring with the forefinger, I found, to my intense annoyance, that its tip could just reach a third stone, in numerous foliated fragments, attached apparently by false membrane to the anterior wall. and well above the os pubis. The forefinger was not long enough to explore the stone, but with the middle finger I found that it was larger than the first one, that it was deeply imbedded in a kind of pouch, could not safely be got at by any instrument, and that it must be detached piecemeal by the finger. By dint of patience and great caution, this was effected. Seven or eight large fragments, about the same number of smaller pieces, a quantity of detritus and shreds of a kind of network of false membrane, which had held the mass together, were, after dropping into the fundus, removed either by the forceps, the finger, or the injection of a large quantity of tepid water, patiently persevered with till I was satisfied not a fragment remained. This friable stone was found to consist of phosphate of lime.

I had never before performed Allarton's operation, and I felt misgivings that this complicated case might end fatally, and unjustly throw discredit on what I believe to be one of the most valuable improvements in modern surgery; I am, however, happy to say that the progress of the case has been most satisfactory, and may be summed up in a few words. About two hours after the operation the child seemed a little excited under the after-effects of the chloroform, but said he felt pretty well. That evening he said he was "quite mended;" but, although free from pain, he had a somewhat restless night; since which time he has gone on without any bad symptom, or any symptom whatever requiring treatment, except a little cough, which he had been troubled with some time previous to the operation. He never complained of pain but once, and that was during the passage of some urine. By the seventeenth day after the operation the wound had nearly healed; he had been passing urine freely through the urethra, for some days, but had done so a little from the first. He was very emaciated before the operation, but has been gaining flesh since; he eats heartily, sleeps well, and is delighted at being relieved from his terrible sufferings.

From this time the case ceased to require any attention; but the weather being very wet and boisterous, I would not allow him to travel home (a few miles off, and in an open conveyance) until the fifth week after the operation; at that time he had become fat, rosy, and full of spirits. I heard of him a day or two ago, and find that he continues perfectly well, and runs about as though he had never had anything the matter with him.

Remarks.—I feel I cannot speak too highly of Allarton's operation: the ease and certainty with which it can be perfomed must recommend it to every surgeon who has had opportunities of trying both it and the lateral; while the fewer tissues divided, and the insignificant loss of blood, must render it the safer operation. That it is suited to complicated cases the above must prove. The only part of the operation in which I expected difficulty was the dilating the opening in the bladder without dividing the prostate or even the posterior layer of the deep perineal fascia; here, however, I was wrong; the finger dilated the wound and passed into the bladder with the greatest ease, nor was there the least difficulty in grasping and withdrawing the first two stones when lying free in the bladder.

[A writer in the 'Lancet' signing himself "CHIRURGUS," observes, that we do not want a substitute for the knife in the cases of children, for hardly any operation can be more safe than the usual mode of lithotomy in these. What we want is a substitute for lateral lithotomy in the adult, or for lithotrity when peculiarities forbid the latter operation. He says :]

No amount of success in operating by the revived median operation on children will throw any light on the question whether it be suitable for adults where the stones *may* be large. There is nearly as much difference between the prostate of a child and that of an adult in respect to dilatability as between plaster-of-Paris when it is yet in a soft plastic state, and after it has set. Mr. Allarton, and the Italian surgeons mentioned in his book, confide in the finger as the dilator; and certainly, as respects simplicity, there is an advantage in this over the resuscitation by Mr. Teale and others, of the blunt gorget; but still the question recurs, Will the finger be sufficient to dilate, and not lacerate, the adult's prostate? I believe, with Mr. Erichsen, that statistics have not proved the superiority, in late times, of the median over the lateral operation in adults; and until a better dilator than the finger or Weiss' instrument can be found, I should avoid this plan in all adult cases where the size of the stone is doubtful; justifying my avoidance by the fact that it has proved fatal in several recently reported cases of adults, and by the remembrance of the terrible fatality from the Marian operation recorded by John Bell and others.—*Lancet, Dec.* 24, 1859, *and Jan.* 14, 1860, *pp.* 641, 48.

83.—OBSERVATIONS ON LITHOTRITY.

By WM. COULSON, Esq., Surgeon to St. Mary's Hospital.

[After premising that lithotrity should *always* be preferred to lithotomy wherever the crushing operation is applicable, and that it is applicable in all cases in which the stone is small and the genito-urinary organs sound, the author proceeds to give the following general observations upon the operation of lithotrity.]

Like all other operations, lithotrity requires certain conditions which are essential to its successful performance. To understand what these conditions are, it is only necessary to recall to mind in what the operation of lithotrity consists. First, certain instruments are introduced into the bladder, which has previously received some fluid to protect it. Secondly, the calculus is seized and broken into small fragments. Thirdly, these fragments are passed from the bladder naturally, or are removed by the aid of injections and instruments. Lastly, it is an essential condition that these several steps of the operation should be completed without producing any injury to the urinary organs, or seriously disturbing the general health of the patient. To perform lithotrity in a scientific and successful manner, these different conditions must be strictly fulfilled; and in any case where it is probable that these conditions cannot be fulfilled, the operation of lithotrity is inapplicable.

It is evident, therefore, that in forming a judgment on the nature of any case previous to operating, two important points are to be regarded: First, the size of the stone to be removed; and next, the condition of the urinary organs. I do not dwell on the general health, because this usually depends on the state of the urinary organs, and is regulated by their condition. In every case, accurate information on these two points should be obtained before lithotrity is attempted, for neglect in this matter may be attended with serious consequences. This information can only be acquired by the most careful examination with those instruments and means which modern science has furnished.

Lithotrity is a peculiar operation. In nearly all other surgical procedures we see what we are about, or are assisted by the sensations of our fingers; whereas, lithotrity is carried on from the beginning to the end in the dark, the sensations which guide us being conveyed through the medium of instruments, and not directly by the touch. Hence it is of the utmost importance that as much information as possible should be obtained before the operation is commenced.

I have already shown that the principal difficulties of the operation are connected with the physical condition of the stone and the state of the urinary organs. Now, it is impossible to obtain precise information on these points without previous examination. In fact, this examination serves a double purpose: it enables us to determine whether lithotrity is suited to the case, and, next, it clears up many points on which the facility and ultimate success of the operation may depend.

The mode of conducting the first examination is as follows:—The patient being placed in the recumbent position, with the pelvis raised and the body depressed, four to six ounces of tepid water should be injected into the bladder. He should then be examined, either with a sound having the curved part short, or with a lithotrite, the curved portion of which is likewise short, while the blades are wide and flat. In ordinary cases the short-curved sound will be quite sufficient, but whenever there is difficulty in detecting the stone, or the co-existence of organic mischief is suspected, the lithotrite should be employed. The advantages of the lithotrite over the sound as an exploratory instrument have been clearly established. M. Civiale, in his fourth lecture on Lithotrity, mentions some cases illustrative of this point; and most surgeons of experience in calculous cases have succeeded in detecting with the lithotrite a small stone which had eluded their search with the sound. In some cases, however, even with the aid of the lithotrite, it is difficult to detect the calculus. The causes of the difficulty are now, for the most part, well understood. The stone may be small and concealed behind an enlarged prostate, or the sensibility of the bladder may be so great as to render it extremely difficult to make the necessary examination. In other cases, the great capacity and feeble contractility of the bladder are obstacles to the discovery of a small stone. In ordinary instances, the calculus, when concealed behind an enlarged prostate may be detected with the short-curved sound or the lithotrite, provided this instrument be first reversed, its handle then depressed and afterwards raised, so that the point of the instrument shall reach the bladder immediately behind the prostate. In these cases the instrument employed for the examination should be longer than those in ordinary use.

In some remarks on lithotritic instruments, in cases of enlarged prostate which I contributed to the 'Lancet' of Jan. 3th, 1858, I alluded to the changes in the genito-urinary organs produced by enlargement of the prostate organs, and requiring the use of peculiar lithotritic instruments. One effect of prostatic enlargement is elongation of the urethra, this elongation being mainly confined to the prostatic portion of the urethra. In these cases the orifice of the bladder is thrown backward in proportion to the development of the enlarged gland. Hence the surgeon is very likely to find the point of the instrument which he uses catch against the superior wall of the canal before it enters the bladder. The shorter the instrument he employs, the more likely it is that he will meet with this obstacle.

Another effect of prostatic enlargement, especially when confined to the middle lobe of the prostate, is to produce changes in the floor of the bladder. The extension of the middle lobe of the prostate backward forms a reservoir or sac behind the enlarged gland. In this sacular depression of the floor the urine dwells, and a calculus may be lodged, the detection of which is often very difficult.

In all these cases a longer lithrotrite than that usually employed is indispensably necessary. The reasons may easily be made clear. The length of the canal is increased by at least an inch. The enlarged prostate encroaches another inch or more on the front part of the floor of the bladder. The calculus lies in a sort of pouch, before which the enlarged lobe of the gland rises like a bar. Hence the necessity for an instrument longer by two or three inches than that in ordinary use. I mentioned in the paper referred to, the case of a patient sent to me by Mr. James, surgeon, of Uxbridge, in whom I could not well reach the stone with ordinary instruments; and since that time, on a patient of Mr. Tatham, of Brighton, I was obliged to employ a yet longer instrument—one measuring fourteen inches. With this I could well reach the stone. With the long lithotrite it is not necessary to push the instrument to the shoulder; but a short and sharply curved beak must be used, and the pelvis must be raised. If the pelvis be not raised, the calculus may be concealed behind the prostate. It is not sufficient merely to raise the lower extremities. But if the pelvis be raised, the calculus will be displaced towards the posterior wall of the bladder. In this way a cautious and practised hand will often fall upon the stone with remarkable ease.

If the stone cannot be felt by this method, we may succeed by examining the bladder in different states of its capacity. The best instrument for making this examination is the hollow lithoclaste of M. Civiale, from the use of which I have derived great assistance. This instrument is a small lithotrite, short in the blades, and having along the centre of the male branch a groove, through which any fluid in the bladder slowly escapes. The mode of using it is simple. Three or more ounces of tepid water are thrown into the bladder, and the lithoclaste is introduced before any of the fluid is allowed to escape. The instrument is now slightly opened, and while the fluid escapes through the central groove, the extremity of the opened instrument is slowly brought into contact with the different parts of the bladder. This method is likewise applicable to the final examination of the bladder after lithotrity, for the purpose of deciding whether any fragment of the stone remains behind.

Having recognised the presence of a stone in the bladder, and formed some opinion of its size, we also by this examination learn the degree of sensibility and the physical state of the urethra; we ascertain whether any considerable enlargement of the prostate exists, and discover the condition of the bladder; all which various conditions, it is unnecessary to observe, are those which influence the surgeon in the selection of lithotrity as the operation best suited to the case. When the urethra is in a healthy state, the prostate not much enlarged, and the bladder capable of retaining four or more ounces of fluid, the operation of lithotomy is easy of performance, and generally free from pain and danger.—*Lancet, Jan.* 14, 1860, *p.* 20.

84.—A NEW URETHROTOME FOR THE TREATMENT OF OBSTINATE STRICTURES OF THE URETHRA.

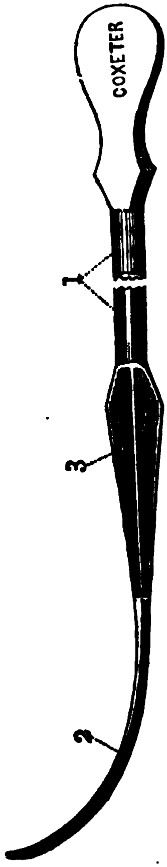
By John Marshall, Esq., F.R.S., Surgeon to University College Hospital.

[Mr. Marshall believes that the instrument described below, "involves a novel principle of action." Its construction is simple.]

It consists, as the subjoined wood-cut will show, of a solid steel bougie of the size of No. 10 in the stem, (marked 1,) of the size No. 2 for three inches at its curved end (2), and having an intermediate cutting portion (3) about one inch and a half in length.

In the construction of this part of the instrument its peculiarity consists. It is here flattened on its upper and under surfaces, so as to form a thin wedge, with two lateral standing edges, and having a circumference gradually increasing from No. 2 to rather more than No. 10 size. Its upper and under surfaces are neither plain nor convex, but present a central rib, with two lateral hollows, so as to allow of but little hold upon these surfaces when the instrument is in use, and yet maintain its stiffness and its general wedge-like form. At the same time, the grooves also serve to contain some unctuous substance by which to lubricate it.

The lateral borders of this part of the instrument are ground to a smooth, stiff, blunt edge,—something like that of a sword—not sharp enough to cut the finger or the lip, or even the tongue, when pretty firmly drawn over those parts, but readily severing any yielding though tough inorganic structure, such as leather, cat-gut, or even string, drawn over it with a sufficiently firm, sliding movement.



In using this instrument, its smaller end is passed through the stricture to be divided; and if this be carefully done, provided the orifice of the urethra has been previously dilated, if unusually narrow, no sort of cutting or even scratching of the urethra in front of the stricture takes place. The point of the instrument having been ascertained to be in the bladder by the freedom with which it can be moved about, the operator, then standing on the right side of the patient, (who is supposed to be recumbent,) and holding the handle of the instrument in his right hand, and the penis well drawn forward with his left hand, is enabled, as I have found in practice, to pass in the instrument through the stricture, in a few seconds, with a surprisingly small amount of force. The instrument acts smoothly, without catch or jerk, and with a sensation to the operator as if it were penetrating something as soft as cheese.

The operation can, of course, be performed with or without chloroform, but to a patient who had no chloroform the slight pain caused by cutting was less inconvenient than that produced by previous attempts at dilatation. In the case alluded to, the man—a retired Indian soldier, fifty-four years of age, and a patient in University College Hospital—had suffered many years from a narrow organic stricture, accompanied with albuminuria. With some patience, after several weeks, the stricture was dilated so as just to admit No. 3. In this condition, on the 24th of February, it was divided by the operation above described, when only a few drops of blood escaped from the urethra. A No. 9 elastic catheter was then passed with the greatest ease. Six hours later, the catheter was removed. From that time the patient has passed his urine in a full stream. On the day after the operation, a No. 10, being tried, did not pass, owing, probably, to swelling of the sides of the canal; but on the seventh day Nos. 8, 9, and 10, on the eleventh day, Nos. 9, 10, 11, and on the fifteenth day, Nos. 10 and 11, slipped into the bladder at once, and were withdrawn without any grasping by the walls of the urethra. I may add that neither shivering or any other constitutional disturbance followed the operation, and that the health of the patient, as might be anticipated, has improved.

The instrument now devised is evidently not a mere wedge-shaped dilator, and, I may remark, is wholly inefficient if its edges are left rounded, or non-incisive. Neither does it act in the same manner as the fine-bladed sharp-edged urethrotomes now employed, whether these cut from behind forwards or from before backwards; for whilst beginning from the internal surface of the strictured part, it divides all the hard, resisting structure, it will merely stretch any tissue beyond, which is soft and elastic enough to yield. At all events, it would seem far less likely, when deliberately used, to cut through the fibrous tissue of the corpus spongiosum urethræ than the thin sharp blades of existing urethrotomes. The very small amount of hemorrhage following its employment favours such an opinion; and if the fact really turn out to be so, there will necessarily be less risk of urinary infiltration and abscess.

Whilst, however, the nature and extent of the incisions made by the instrument, and permanency of relief afforded by it, must be the subjects of future enquiry and experience, I may be permitted to direct attention in the present communication to its simplicity of construction, the facility and safety with which it may be used, and the difference between it and the ordinary urethrotomes.

This new urethrotome may, of course, be made of different sizes; but a smaller size at the point than No. 2 might be less safe for ordinary purposes, and a smaller stem than No. 10 is not desirable, unless for a traumatic stricture in a boy's urethra, or in a case in which that canal is unusually narrow. I may further state that I have had a *straight* one made for the division of strictures in the fore part of the urethra, and also a curved *hollow* one, which acts as a catheter, and thus will determine in cases of peculiar difficulty the fact of the end of the instrument being really in the bladder, and will enable that organ to be injected with water, so as to remove its anterior wall away from the point of the instrument, and subsequently furnish evidence of the elastic catheter having reached its destination. I have also had the instrument made with the cutting edges both above and below, instead of at the sides; but for reasons which I need not now state, I have adopted the latter construction.

Finally, it is obvious that a like principle may be applied to instruments for dividing indurated strictures in other canals. It is also evident that it may be adapted to blades capable of being concealed or projected, or to a blade sliding over a smaller guide which has been previously passed.—*Lancet, April* 14, 1860, *p.* 370.

85.—CASES OF STRICTURE OF THE URETHRA TREATED BY "SLITTING UP."

(Under the care of W. A. HILLMAN, Esq., Surgeon to the Westminster Hospital.)

[The plan pursued by Mr. Hilman in these cases was first introduced into the Westminster Hospital by Mr. Holt.]

The method consists in dilating the stricture, if necessary, until it is sufficiently relaxed to allow of the passage of an instrument about the size of a No. 3 catheter. The patient is then placed under the influence of chloroform, and the "stricture-dilator" is introduced. This instrument resembles somewhat a catheter, only slightly curved at the end, and split down the middle, so as to consist of two longitudinal half-cylinders, which admit of being separated to some extent, and are fixed on a central stem or axis. When the instrument is fairly in the bladder, the spring or collar which connects the blades are relaxed, and a large perforated rod, about the size of Nos. 10-12 catheter, bevelled off at the end, is fixed on the central axis, and forcibly driven down the urethra. The effect of this is to suddenly dilate, and in most cases to rupture, the tissues of which the stricture is composed; as is evidenced by the escape of a few drops of blood. The dilator is now withdrawn, and a No. 10 or 12 catheter (according to the size of the urethra) is introduced, and the water drawn off. The catheter is taken out, and the patient sent to bed, and generally a stimulant given. He is left quiet during the following day; and, on the second day from the operation, the catheter is reintroduced, and generally passes readily. Sometimes the stricture, or the urethra, may have contracted again to the size of No. 8. This plan is extensively followed by Mr. Holt, Mr. Hillman, and Mr. Power; and a very great number of cases altogether have now been treated in this manner, the greater number having been under Mr. Holt's care. No bad symptoms have followed in any case; and we are informed that the stricture shows no greater tendency to recontract than is noticed after any other plan, and that several persons have been seen in whom the cure had already lasted three years.

We need hardly dwell on the advantages of a plan which, if successful, saves so much time and suffering as is involved in the treatment of a severe stricture by the ordinary method of dilatation; and if the forcible rupture of a stricture should prove as safe a proceeding in the hands of others as it has done in those of the surgeons of this hospital, we do not doubt that it will soon come into more general use.

Case 1.—T. W. has suffered from urethral stricture, for about fourteen years, the symptoms having gradually become more and more severe. He sometimes passed water in a very small stream, but more frequently only in drops; in his own words, he "was always making water." There was considerable admixture of mucus and pus with the urine, and he suffered from considerable prolapse of the rectum.

On his admission, a No. 1 catheter could not be passed into the bladder; but after a short stay in the hospital, a No. 1, and subsequently a No. 3, silver catheter was passed with some difficulty. After a short residence in the hospital, whilst he was under the influence of chloroform, Mr. Holt's "stricture dilator" was introduced, and the stricture split up. The dilator having been withdrawn, a No. 10 silver catheter was passed into the bladder, the bladder emptied, and the catheter removed, a few drops only of blood being lost. He had some smarting pain, as the urine passed along the urethra, for two or three days, but no unfavourable symptom of any kind followed the operation.

He was shortly discharged from the hospital passing his urine in a good stream, without pain, and was not obliged to empty the bladder at night, as he could retain more than a pint of urine without inconvenience, the mucus and pus having disappeared, and the prolapse of the rectum nearly subsided.

Case 2.—P. F., aged 56, was admitted under Mr. Hillman's care, on September 13th. He had suffered from stricture of the urethra for at least sixteen or eighteen years. The urine passed in an exceedingly narrow stream, almost indeed drop by drop; and the bladder was so contracted and irritable that it would retain scarcely any water. The patient was in the hospital for four weeks before any instrument could be introduced into the bladder; after five attempts, however, a silver catheter, smaller than No. 1, was passed, and subsequently a No. 3.

October 18th. The "stricture-dilator" having been introduced, and the patient put under the influence of chloroform, the stricture was split up, a No. 10 silver catheter was introduced; the bladder was emptied, and the catheter immediately withdrawn. A few drops only of blood escaped from the urethra. He was made warm in bed; had some hot gin and water administered. No bad symptom followed.

He left the hospital much improved in general health, passing his urine in a good stream; the bladder having so far recovered its healthy condition, that there was no necessity for him to pass urine during the night.—*British Med. Journal, Nov.* 19, 1859, *p.* 933.

86.—THE IRON-WIRE SETON IN HYDROCELE.—CASES.

By THOMAS DAVIDSON, Esq., L.R.C.P., L.R.C.S.E., and T. SYMPSON, Esq., M.R.C.S.E., Surgeon to the Lincoln County Hospital.

[Mr. Davidson's case was in a man aged 67, the subject of hydrocele of the right testis for ten years. He had been repeatedly tapped; the largest amount reported is thirty-three ounces.]

In August last, the hydrocele being of such a magnitude that it prevented the man from following his usual employment, that of a carpenter, I resolved to give him the chance of a permanent cure by employing the wire seton. On the 22d of August I introduced six threads of the proper wire from below upwards, with a curved needle suitable for the purpose. The fluid, which would measure at the least forty ounces, speedily escaped. I allowed the seton to remain thirty-six hours, when I removed it gently. At this time, everything seemed favourable, there being no pain or uneasiness; but in the course of twenty-four hours intense inflammatory action was lighted up in the tunica vaginalis and testis, and distending by effusion the scrotum to an immense extent. It was only by the most assiduous attention, and after using every available means for reducing the inflammation, that its course was ultimately checked. On perceiving fluctuation, I made a free incision into the centre of the tumour, and gave exit to a considerable quantity of fetid pus. In about ten days all discharge ceased and the wound healed; suppuration, however, repeatedly recurred, obliging me from time to time to open the abscesses. The patient was now in the most critical situation, being reduced to a state of great debility, and but for a liberal supply of port wine, beef-tea, &c., the man must have sunk.

The discharge still continuing fetid, I injected into the cavity a solution of chloride of lime morning and evening, keeping the wounds open for many weeks together, and by this means I fortunately succeeded at last in effecting a cure.

By the 10th of November, the wounds were healed, all inflammation had ceased, and the testicle was reduced to its natural size, so that the hydrocele might be pronounced perfectly cured.

The man has not up to the present date regained his strength sufficiently to enable him to resume work, although I expect that in a week or two he will be able to do so. From the extent of the suppuration, and constitutional suffering which occurred in this case, I am apprehensive that without some modification of the operation, the employment of this method of cure will be rather unsafe as a general practice, as we see from the case published in the 'Medical Times and Gazette', for Dec. 10, by Mr. J. B. Thompson, and from another reported by Dr. Gillespie some months ago, that the same constitutional disturbance and excessive suppuration takes place in the young and robust subject, as well as in those more advanced in life.

[In a case of hydrocele similarly treated by Mr. Sympson, the patient was a child of four years old, and the hydrocele the size of a hen's egg. It had been twice tapped with a fine trocar. He was admitted into the Lincoln County Hospital on March 14th, 1859.]

On the 15th, by means of a hemorrhoidal needle, a single silver wire was passed through the sac, and secured by twisting together the two cut extremities. This was allowed to remain in twenty-four hours, by which time the scrotum had become greatly tumefied, and very tender, red, and painful. The constitutional symptoms ran high, and on the 18th, it being plain that suppuration had occurred, an incision was made, which gave exit to about an ounce of pus. After this another abscess formed in the lower part of the scrotum, which required opening on the 25th. Pressure, by means of strapping, was applied on April 10, and on May 16 he was discharged with a scarred and roughened scrotum, but without trace of the hydrocele.

Remarks.—At first I was inclined to ascribe the ill effects produced by the wire in this case to its having been left in too long; but I now think them more likely to have arisen from some peculiarity of constitution: such results occasionally occurring after other methods of treatment. To all appearance the boy was in excellent health when he entered the hospital, and there seemed no reason why so simple a procedure as the passing of a silver wire through the tunica vaginalis should occasion so large an amount of irritation; but, probably, had any other operation been substituted, it might have been attended with a similar result.

As the treatment of hydrocele by metallic seton is still *sub judice*, I have thought it right to place this case on record.—*Medical Times and Gazette, Dec.* 31, 1859, *pp.* 652, 653.

87.—FRACTURE OF THE PELVIS AND RUPTURE OF THE URETHRA.

(Case under the care of JAMES PAGET, Esq., F.R.S., St. Bartholomew's Hospital.)

The following is an example of the most difficult class of cases of urethral stricture with which the surgeon can ever be called to deal. When a stricture is simply impassable, there is hope that by patience it may be dilated; but when, in addition to that lesion, there exists a false passage leading past the stricture, there is little prospect that the instrument can ever be made to find its way into the proper channel. If the false passage be large, and if it lead merely out of one portion of the urethra into another, or into the bladder, it is quite possible that the patient may not suffer greatly, and that no surgical interference may be called for. But if, on the contrary, a false chamber has been formed, admitting of an accumulation of urine, and subjecting the patient to the risk of urinary abscesses, infiltration, &c., the case becomes at once one of extreme difficulty and danger. In the case before us a severe injury to the urethra and pelvic contents had been received, and, as a consequence of extravasation, a large subsidiary cyst had been formed between the pubes and the true bladder. Into this false bladder the urine constantly escaped, and although catheters were easily introduced to their full length, yet they passed into it, and not into the proper receptacle for that fluid.

It is in cases such as this that the operation of perineal section is at once peculiarly valuable and peculiarly difficult. In commencing its performance the surgeon can never feel sure that he will be able to find the proximal or vesical tract of the urethra; and if he should fail in this, the end of the procedure is not attained. The rules for success are to cut freely, and to keep most carefully in the median line, and to obtain by examination per rectum a clear idea as to the position of the prostate.

[The patient was a miner who, two years ago, had received a severe blow on the pelvis, by the falling of a piece of rock. This was followed by paralysis of both legs, and retention of urine. A catheter was passed, and a large quantity of pure blood drawn off. This was repeated for three days with the same result. On the fourth day some urine flowed. The perineum became hard and tender, and was incised on the fifth day, and a fistulous opening remained. He continued twelve weeks in the Chester Infirmary, the catheter still requiring to be passed; the urine flowing still through the fistulous opening left, at the spot of incision. He was under surgical treatment the whole of the two years before admission under Mr. Paget, when he had entirely recovered from his paralytic symptoms, but still continued unable to relieve his bladder.]

On admission Mr. Paget was able to pass a catheter (No. 5 or 6). He remarked that the instrument went in "with all the facility of error," and stated that he felt sure that it did not enter the true bladder. Resistance was encountered at a certain point. Urine was drawn off, but its flow was preceded by a discharge of pus. Having consulted with his colleagues, on December 5 Mr. Paget determined to perform perineal section. The patient was placed under the influence of chloroform; a grooved staff was introduced, and an incision next made in the middle line. A hard, cartilaginous mass was encountered, which Mr. Paget freely divided. An attempt was now made to discover the vesical tract of the urethra by passing a large probe from the perineal incision. This was successful, and a catheter having replaced the probe, the urine flowed freely, thus proving that the cavity of the true bladder had been reached, since the fluid contained in the false sac had been drawn off in the beginning of the operation. Mr. Paget next passed a No. 8 catheter through the penis, and having brought it out at the wound, next guided it on into the bladder, by the side of that previously introduced. This was left in, no hemorrhage, shivering or any other unfavourable symptoms occurred. The bowels were left undisturbed several days, and during a whole week the catheter was left in, causing no uneasiness. He continued to improve in every respect until November 23rd, when he had an attack of severe pain in the testis and loin. This was relieved by warm baths and a narcotic, and did not return.

Dec. 3. He was now able to pass the catheter himself. It was left in an hour daily. A little urine passed through the wound.

20th. It is noted,—"He is going on very satisfactorily. He passes the catheter every day, and leaves it in an hour. The urine now comes the right way. Mr. Paget desires him to pass the instrument for a month longer."—*Med. Times and Gazette, Feb.* 11, 1860, *p.* 142.

88.—*Varicocele Ligature and Subcutaneous Section.* (Case under the care of Wm. Bowman, Esq., F.R.S.)—[This case has occurred in a young man, the varicocele having existed six months, and prevented his following any employment, from the pain in his testes, loins, &c.]

Mr. Bowman stated that the operation was based on exactly the same principle as that of subcutaneous ligature of veins for varicose ulcers. He passed two needles, an inch apart, under the veins, carefully keeping the vas deferens with the artery away, and then applied the ligature over them, just tight enough to occlude the cavity of the veins, but not to cut them. The veins were then divided between the needles by a tenotomy knife, the wound made being very small. A pad was next placed over the incision. Mr. Bowman stated that he had treated varicocele by this method on several occasions with very satisfactory results. There was no hemorrhage, nor did suppuration follow, and thus there was no chance of pus, and also none of decomposed blood being absorbed into the system. He should remove the lower needle probably on Monday, and the upper one on Wednesday. The neuralgia would probably remain for a little time after the operation. He stated that atrophy of the testis, so frequent in varicocele is due to impaired nutrition from obstructive circulation, the same condition precisely existing in varicose ulcer, and both also remediable by the same means, viz., obliteration of the varicose veins.

[November 30th. To-day Mr. Bowman removed the second needle, the lower one having been removed on Monday. The man is doing well. There is no suppuration.]—*Med. Times and Gazette, Dec.* 3, 1859, *p.* 552.

DISEASES OF THE SKIN.

89.—ON THE TREATMENT OF FAVUS.

[Despite the much-varied forms of treatment the disease often remains very obstinate. Porrigo is very often mistaken for favus, and consequently many cases supposed to be "cured" are not cases of favus at all. The following plans of treatment are those of different modern dermatologists, well acquainted with the diagnosis and true pathology of the disease.]

Mr. Startin's plan of treatment, as adopted at the Hospital for Diseases of the Skin, is to clean away the scabs, either by poultices or careful washing, and then apply freely an ointment containing sulphur and the ammonia-chloride of mercury. The latter should be in proportion of about a scruple to the ounce of lard. By this plan we have often seen scalps which have been covered with favus got perfectly clean, and apparently cured in the course of a few weeks.

Dr. Jenner's treatment is, like Mr. Startin's, based on the principle of employing parasiticidal remedies, and consists in the free use of lotions or baths containing sulphurous acid.

Dr. Hughes Bennet endeavours to prevent vegetable growth by the exclusion of atmospheric air, applying with that object cod-liver oil, and covering the scalp with oil-silk.

Dr. Neligan employed an ointment of the iodide of lead, after having cleaned the scalp by poultices and alkaline washes.

We cannot think that it is a matter of any very great consequence in what manner the cryptogam-destroyer is used, or whether the drug chosen be creosote, mercury, or sulphur. Whatever plan is adopted, the secret lies in following it out perseveringly, and without suspending it because the disease may appear for a time to be well, The removal of the hairs from the affected part by means of tweezers, is a measure which, if the patient will allow it, and the surgeon has zeal enough to adopt it, is one likely to materially increase the chance of genuine cures being obtained.—*Med. Times and Gazette, Dec.* 31, 1859, *p.* 655.

90.—*Eczema of the Hands.*—M. N. GUILLOT, of the Necker, employs the following ointment with success in the treatment of eczema of the hands:—Lard 30 parts, subcarbonate of soda, oil of cade, tar, of each from 2 to 4 parts.—*Med. Times and Gazette, March* 24, 1860, *p.* 299.

91.—*On the Treatment of Squamous Disease.* By WILLIAM MOORE, M.B., M.R.I.A., Dublin.—[After giving in detail numerous interesting cases of squamous disease of the skin, illustrating the different forms or varieties of the class, Dr. Moore makes the following remarks on the treatment:]

As a rule this genus is met with in a sub-acute form, and frequently accompanied with hepatic congestion; consequently, in the first instance, it is advisable to exhibit a mercurial or other chologogue till the functions of the liver are restored. Then the proper line of treatment is tonic and alterative, with local appliances which may tend to induce increased dermoid action, and for this purpose baths will be found invaluable.

Amongst the mineral tonics and alteratives I may mention the biniodide of mercury. It is almost the only mercurial I habitually, so to speak, exhibit in the squamous class; but, as has been justly observed by M. Ricord, the hydrargyrum is so merged in the iodide that it scarce deserves the former appellation. The different preparations of arsenic must be resorted to in many instances. Biett has strongly advocated the use of the "arseniate of ammonia," others the arsenite of potash, liquor arsenicalus, &c. For my own part I think the "iodide of arsenic," in doses of one-sixteenth to one-tenth of a grain in pill, a peculiarly nice form for its administration—in short, I look upon the iodides to be the basis of treatment in this particular class of skin diseases. The preparations of larch are decidedly tonic and alterative, and are peculiarly suited for internal exhibition in chronic cases occurring in elderly or atonic constitutions; they may be applied locally in psoriasis either alone or combined with the dilute citrine or other special stimulant ointment; I think they deserve a very high place. Hygienic requirements are indispensable in the treatment of these diseases, not only as regards the omission of non-stimulating diet, but also as to perfect cleanliness and promotion of the functions of the skin. Hence, as I have already mentioned, the value of baths is self-evident, more particularly in pityriasis and in the syphilitic sub-species, where sea-bathing or tepid sea bath will frequently be attended with the best effects.—*Dublin Hospital Gazette, Jan.* 16, 1860, *p.* 24.

92.—ON THE PREVENTION OF PITTING IN CONFLUENT SMALL-POX.

By WILLIAM STOKES, M.D., Regius Professor of Physic in the University of Dublin.

[The various modes previously employed, with a view of preventing pitting in cases of small-pox, may be thus enumerated :]

1. The puncture of the pustules when matured.
2. The application of nitrate of silver.
3. The application of oil, or of the *linimentum calcis*.
4. The covering the face with a solution of gutta percha, with collodion, or with glycerine.

[The first of these modes is best adapted to a benign form of the disease, in which the pustules, though numerous, remain discrete for a longer period than in the severer cases. As to the second mode, Dr. Stokes has had no personal experience; it will be most suitable, like the last, in milder forms of the disease. The third form Dr. S. has tried, but does not consider either application of much value, though the linimentum calcis is preferable.]

During the past five years I have used gutta percha and collodion in a considerable number of cases. These, however. were not by any means examples of the worst form of the disease. In most of them the crust came off in large flakes or patches, composed obviously of the dried exudations and the covering material, and leaving the skin uninjured. To render this treatment effective, at least so far as the exclusion of the air is concerned, it is necessary to renew the application at intervals of from twelve to twenty-four hours; for the covering seems to be repeatedly broken up by the advance of the eruption and the swelling of the parts. Some patients are greatly distressed by the feeling of constriction caused by the coating of gutta percha or collodion, and in general the treatment in question appears unadvisable where there is much vascularity, heat or swelling.

Looking at the frequency of pitting on the face, as compared with that of other parts of the surface, it is not easy to account for it unless by referring to the fact that, while the rest of the surface is kept covered, and so not only comparatively excluded from the action of the air, but in a state of humidity, the integuments of the face remain in a dry and heated state—first, from the action of the external air, and next, from the increased vascular action. Hard and hot scabs are formed, and the ulcerative process makes its way downwards to a greater or less degree. Some have held that the liability of the face to markings was to be explained by anatomical considerations. However this may be, it will be found that in cases in which from an early period certain portions of the face have been kept protected from the action of the air, and in a permanently moist state, pitting does not occur. This may be seen in cases of sthenic confluent small-pox, where, with the view of preventing the adhesion of the eyelids, poultices have been used over the eyes. In such cases it will be often found that every part of the face is marked, except those over which the little poultices had extended.

The application of poultices over the face appears to me to be the surest mode of preventing the consequent disfigurement. We should commence their use at the earliest period, and continue it to an advanced stage of the affection. In most cases they may be applied even over the nose, so as to cover the nostrils, for these passages are generally so obstructed as to be for the time useless to the patient. If the nostrils can be kept pervious by injections, the poultices need not be applied over their orifices.

If the chances of marking are in proportion to the activity of the cutaneous irritation, we may hold that this method should fulfil three important indications of treatment—

1. The exclusion of air.
2. The moderation of the local irritation; and
3. The keeping of the parts in a permanently moist state, so as to prevent the drying and hardening of the scabs.

The value of this treatment, however, will, I feel convinced, be best seen in the inflammatory or sthenic form of the disease. The best poultice will be that formed of linseed meal, which should be spread on a soft material, such as French wadding, and covered with the gutta-percha paper or oiled silk. I have never had occasion to repent the adoption of this practice.

[Dr. Stokes sums up his paper with the following conclusions:]

1. That the chances of marking are much greater in the sthenic or inflammatory, than in the asthenic or typhoid confluent small-pox.

2. That considering the change in the character of disease, both essential and local, observed during late years, we may explain the greater frequency of marking in former times.

3. That in the typhoid forms of the disease the treatment of the surface by an artificial covering, such as gutta percha, or by glycerine, will often prove satisfactory.

4. That in the more active or non-typhoid forms, the use of constant poulticing, and of every other method that will lessen local inflammation, seems to be the best method of preventing the disfigurement of the face. *Dub. Quarterly Journal, Feb.* 1860, *p.* 111.

93.—LUPUS OF SEVEN YEARS' STANDING EFFECTUALLY CURED.

(Case under the care of WEEDON COOKE, Esq., Surgeon to the Royal Free Hospital.)

[Applications of arsenic and chloride of zinc no doubt stay the diseased action in this intractable disease for a time; and cod-liver oil, as well as arsenic, administered internally, have some beneficial influence; but the disease must depend upon some constitutional cause, without a knowledge of which our treatment can be only in a great measure empirical. The present case occurred in a lad aged seventeen, an oyster-dredger, at Maldon, in Essex. He has suffered for seven years from lupoid ulcers of the face, nose, and lips.]

At the time of his admission, the whole face was covered with either the cicatrices of old ulcers, or ulcers encrusted and dipping into the muscles beneath the skin. The columna nasi was destroyed, and the disease was encroaching upon the alæ. The upper lip was thickened and ulcerated. The scaly ulceration extended upwards to the inner canthus of both eyes. Added to these miseries, he was extremely deaf, and altogether presented a most pitiable and unsightly appearance. Mr. Cooke ordered an ounce and a half of lemon juice to be taken three times a day, meat and porter, with green vegetables; and the following lotion and ointment; bichloride of mercury, eight grains; hydrochloric acid, sixteen minims; water, eight ounces: to be applied three times a day as a wash, afterwards covering the parts with zinc ointment.

He remained in the hospital under this treatment, gradually improving, until August 17th, when he was discharged, with all the ulcers entirely healed, the deafness diminished, and his general health re-established.

In May, 1857, he again came to town, and was readmitted, the disease having returned on the cheek and upper lip. The same treatment was adopted, and this time he was well again in a month.

In July, 1859, he again appeared amongst Mr. Cooke's out patients, the upper lip and one cheek only being affected with the scaly ulcers. He was enabled to stay with a sister in town, and was, therefore, not admitted, but placed under the same treatment—namely, lemon-juice and fresh green vegetables, bichloride of mercury lotion, and zinc ointment. The cure was very rapid, the ulcers having well cicatrized in less than three weeks. His hearing also at this time was very greatly improved, and his general appearance so altered that he was scarcely recognised as the same unhappy youth who first came under notice in 1856.

Mr. Cooke states that there was no sponginess of the gums, but that the strumous aspect of the lad. and his occupation as an oyster-dredger at the sea, led him to prescribe lemon-juice as an antiscorbutic remedy, and, as it turned out, with the happiest results. Still, the adjuvant effect of the local applications must be by no means slighted or overlooked in the treatment of similar cases. Perhaps, however, the leaning generally is to depend too much upon local, and especially caustic, applications.—*Lancet, Dec.* 31, 1859, *p.* 662.

94.—ON THE TREATMENT OF CORNS ON THE SOLE OF THE FOOT.

By Holmes Coote, Esq., F.R.C.S., Assistant-Surgeon to St. Bartholomew's, and to the Royal Orthopædic Hospitals.

I have lately had under my care some cases illustrating the nature of this painful affection and its treatment. It may be necessary to remark that corns are technically termed "clavi," from a fancied resemblance to the head of a nail; but most persons know that they consist of thickened epithelium or cuticle, and that a small bursa is sometimes found between them and the subjacent parts. They proceed entirely from undue pressure; hence they appear on whatever parts of the feet a pair of badly-fitting boots or shoes press unequally and unpleasantly. The remedy is simple, and consists in the person so affected wearing boots with a sole as wide as the sole of the foot, of ample length, square at the toes, and with the upper leather soft and moderately loose. But this advice is rarely followed, fashion exerting a more powerful influence than good sense.

There is, however, a form of corn which is found on the sole of the foot, the pain attending which is so great that patients are at times unable either to walk or stand. Mr. Erichsen notices it in his work on surgery. "It is usually of small size and round in shape, the neighboring cuticle being always thickened and hardened. It is extremely sensitive to the touch, the patient shrinking when it is pressed upon, as if an exposed nerve had been injured. On slicing it down with a scalpel, it will be found to be composed of soft, tough, and white epidermis, arranged in tufts or small columns, in the centre of each of which a minute black dot is perceptible. Each tuft appears to be an elongated and thickened papilla, and the black speck is a small point of coagulated blood which has been effused into it. Around the depressions in which each of these corns is seated, the hardened cuticle forms a kind of wall."—p. 439.

I have known ulceration to occur in this morbid structure, when a deep and foul sore, excessively sensitive, is formed. It may be healed by rest, but it recurs when the patient resumes the usual habits.

Now the cause of these corns will generally be found to proceed from a tense condition of some of the important tendons, that most frequently affected being the tendo-Achillis. When it is so contracted, the foot cannot be raised beyond a right angle; and it follows that the weight of the body is unduly thrown on the fore part of the sole of the foot, where the corn speedily forms. The contraction of the tendon may be so slight as to need careful examination for its detection; but so long as it exists the cause of the corn remains, and it will be found that any other measure less than the division of the tendon will be only palliative. The subcutaneous division of the tendon, its elongation, and the restoration of the foot to its normal bearings must be conducted on the usual principles of orthopædic surgery. The practice has been adopted many years at the Orthopædic Hospital; but it is not so generally known as, in my opinion, is desirable.—*Lancet, Dec.* 10, 1859, *p.* 584.

95.—ON ARNICA AS AN APPLICATION TO BRUISES.

By Mitchell Henry, Esq., F.R.C.S., Surgeon to the Middlesex Hospital.

It is gratifying to know that in the common arnica plant, and in the tincture prepared from it, we have a remedy of very singular efficacy, applicable in cases, from the most trifling to the most severe. I have employed it for several years past in a vast variety of cases, and can speak of it from experience as unapproached in power by any other remedy or combination of remedies whatever. Many persons suppose that we are indebted to the homœpaths for the introduction of arnica; but this is quite a mistake, although it has been more prominently brought into notice by their commendations. Arnica has long been in use as a popular remedy in Germany, as well as in this country. In Ireland, a poultice of the arnica leaves (the leopard's bane) is a favourite application to the "broken knees" of a horse, and is a far more rational one than the stimulating "oils" so commonly used in England. To bruises, to allay the smarting of wounds after operation, to fractures, dislocations, and all similar injuries, it may be applied with implicit confidence in its power, to the exclusion of fomentations, cooling lotions, and leeches. The only objection that can be made to it is that it sometimes irritates the skin; but this proceeds from its being employed in too great quantity and strength: or it is possible there may be a peculiar idiosyncrasy which predisposes the skin of some persons to become irritated by it. I can only say, however, that such cases must be rare indeed, for I have never met with one. I never employ it weaker than two drachms of the tincture to the pint of water, or stronger than two drachms to the ounce of water, and this only in rare cases, and for a short time, until pain has abated. The most useful strength is four or six drachms to the pint, and this may be kept as an hospital preparation, and used indiscriminately. In cases of very severe shock, as in heavy falls, a few doses of the tincture in quantities of five minims, administered internally at intervals of four or six hours, and carefully watched, for it is a depressant, will give great relief.

A few examples will serve to illustrate the foregoing remarks. And, first, one from my own personal experience. Three years ago the top of my finger was caught and violently jammed in shutting a carriage door. In the course of a few minutes the nail got black, and blood oozed from beneath its sides, whilst the pulp of the finger swelled up into a distinct tumour from the effusion of blood, and I need hardly say the pain was excessive. I went to the nearest chemist's and got a lotion composed of two drachms of the tincture of arnica in one ounce of rose-water, and applied this assiduously as fast as the lint wrapped round the finger became dry, at the same time taking care to keep the hand well elevated. In the course of an hour the pain had very much abated, and I added some water to my lotion. Before going to bed at night all inconvenience had ceased, except when the part was touched; the blackness rapidly disappeared, and in a few days my finger was perfectly well, without the loss of the nail, and in striking contrast to what usually happens in such cases.

Many of you will remember a man, lately discharged from Clayton ward, who had fallen from a scaffolding twenty feet high, and had alighted upon the broad of his back. His sufferings were extreme; he passed blood in his urine, and subsequently for two days had retention. Nevertheless, the arnica lotion applied to his back, and a few doses of the tincture taken internally, relieved him in a marvellously short space of time, although every surgeon knows that there is not a more troublesome or lingering affection than this general shaking or bruising from a heavy fall.

Another patient, not yet discharged from the same ward, had an obscure injury to the shoulder-joint, simulating dislocation, for which attempts at reduction under chloroform had been made before I saw him. The injury was probably fracture of the glenoid cavity of the scapula, and was followed by ecchymosis extending half way down the back, and by very acute pain. Here the arnica was no less useful than in the former case, and the man has now almost recovered the use of his limb. To all our fractures you know it is an established remedy, and you saw me lately employ it as a lotion to relieve the pain which followed an operation for the extraction of cataract. Some of you will recollect that very formidable case of fistula in ano which lay in Bird ward, and which required so many operations for its cure. Nothing alleviated the severe pain attendant on it so much as arnica. I am informed by our house-surgeon, Mr. Cresswell, that he paints over any black eye that may be seen in the surgery with the pure tincture, which has the effect in the early stage, of preventing the ecchymosis almost entirely, and, in a later one, of rapidly getting rid of it. But I might multiply these cases indefinitely. I am bound to say, however, that I have not as yet found it to be so efficacious in relieving the pain of cancer as we might have hoped it would be, and this probably arises from the fact that its action seems to be confined principally to the cutaneous and immediately subjacent textures. And this leads me to speak of its *modus operandi*. We know very little indeed of the ultimate action of any medicines or external application, but the probability is that arnica produces anæsthesia of the cutaneous nerves, and exerts some influence on the ganglionic nerves, which surround the blood-vessels, and regulate their action; for it certainly arrests the formation of thrombus and ecchymosis. However, whether this is theoretically correct, or not, the practical effect of arnica is incontestable.—*Lancet, Dec.* 10, 1859, *p.* 579.

96.—ON THE TREATMENT OF WOUNDS.

By Mitchell Henry, Esq., F.R.C.S.. Surgeon to the Middlesex Hospital.

Strapping and adhesive materials, except sometimes as tending to support and consolidate the sound textures, should be avoided:—metallic sutures, either of well annealed iron wire or of silver wire, being substituted for them. Metallic sutures excite no irritation, and may be fearlessly employed in every situation and to all textures. Unlike sutures made of vegetable materials, the holes through which they pass undergo no suppuration or ulceration, and they may be left *in situ* for an indefinite period—for three weeks if it be desired; indeed, it is not an uncommon event for us to lose a wire suture altogether, the patient leaving the hospital with it still in his body. The arnica lotion should be applied over the wounded part, which, when practicable, should also be kept elevated high above the patient's body; and the relief that these two measures afford will surprise persons who are not familiar with them. The only exception to the propriety of elevating the limb is in cases in which there is much loss of temperature from defective circulation, or a fear of sloughing from the severity of the injury. To limbs in this condition, thick layers of cotton wool form the most advisable application; but, in my opinion, no worse practice can be adopted than that which is too common—namely, the placing of the injured part in a hot poultice that it may "recover itself." The results of careful adjustment, and of attempts to save textures and parts, almost against hope, will amply repay the surgeon, who, at the same time, places his reliance in opium and wine as constitutional treatment. I may here mention that when reaction and inflammatory fever set in, the tincture of aconite, when carefully watched, will exhibit the surprising power which it exerts over all forms of vascular excitement.

Suppurating Sores and Wounds.—Suppuration is a great evil, and may become a great peril to the patient. All applications that promote it are, therefore, to be avoided—such as hot fomentations of all kinds, poultices and ointments. Torn and disorganized parts will suppurate in spite of all that we can do. Our efforts must, then, be directed to rendering the process as little inconvenient as possible. The most scrupulous attention must be paid to supporting the suppurating textures by means of splints, strapping the sound parts of the limb, and applying graduated pressure, so as to limit the area of suppuration, and prevent the matter from burrowing. Next, the pus itself must be rendered as innocuous as possible by attention to cleanliness, and by disinfection when, as is most commonly the case, this also is required. Long ago impressed with the fact that erysipelas, diarrhœa, phlebitis, and low fever could often be traced to the foul condition of suppurating sores on the patient's own body, I have tried almost every disinfectant that has come before me, including of late the mixture of plaster-of-Paris and coal-tar, now so much eulogized in France. One objection to it, and to several others,—as, for instance, charcoal, one of the best of all,—is that they are unseemly, or smell disagreeably. No such objection, however, applies to a solution of Condy's fluid (the permanganate of potash), which, for two years past, I have largely and most beneficially employed in the case of all varieties of suppurating sores. A solution of from half a drachm to two or four drachms of the purple fluid, to the pint of water, will keep even the most offensive sore sweet and clean, and, as a general rule, produce no pain whatever. If it does, the solution must be made weaker, until the required strength is attained, for there is a great difference in respect to its tolerance amongst patients. Its benefits are, perhaps, more strikingly displayed in the case of burns than in any other form of suppurating sore. From the day that suppuration commences in the case of a burn or scald, under my care, it is dressed with lint saturated with a weak solution of this fluid, commencing with half a drachm to the pint of pater, over which a layer of cotton wool is applied. I have no hesitation in saying that burns thus treated will get well better than under any other system that I am acquainted with. The smell, always one of the most distressing features in these cases, is very greatly lessened, if not altogether destroyed; and the patient, no longer tormented with a horrible odour constantly under his nose, regains his appetite for food, whilst he is spared the chances of constitutional affections from absorption of putrid matters.

Amidst the variety of cases thus treated which you have all seen, I need but single out, as an example, the case of Bridget C——, who for eight months lay in Bird ward. Hers was one of the worst cases of burn that I ever saw recover, extending, as it did, over nearly all the left side of the trunk and abdomen, by far the most perilous situations, as well as over the left upper and lower extremities. Those of you also who have watched, as I requested you would watch that old woman from whom I recently removed a large polypus which had grown into the frontal sinus, and displaced the eye, will have observed how the serious typhoid condition into which she was thrown by the putrid discharge flowing back into the pharynx, was chiefly believed by the assiduity with which the discharge was disinfected by injecting the two-drachm solution of Condy's fluid into the nares every four hours. So also you have often seen it employed as a wash for the mouth, as an application to destroy the fœtor of cancerous sores, and as an injection in uterine carcinoma. Chemical science teaches us that the permanganates are true disinfectants because they oxygenate, and do not act merely as deodorizers.—*Lancet, Dec.* 10, 1859, *p.* 580.

SYPHILITIC DISEASES.

97.—ON THE TREATMENT OF GONORRHŒA WITHOUT SPECIFICS.

By J. L. Milton, Esq., St. Martin's-le-Grand.

To bring into as narrow a focus as possible the arguments for and against all the systems of treatment as yet discussed, I shall take the liberty of reducing them to a few aphorisms; the pith of all wisdom so vaunted by Bacon. Dr. Johnson long ago told men that there was little original in the best book ever written, and I am sadly afraid mine is not one of the best. However, the collections of my experience are,

1. That the remedies yet enumerated, recommended, and dismissed to ob-

livion, often by their unnatural parents, though adequate to cure by far the greater number of cases, still leave many unrelieved.

2. That most of them are disagreeable, some dangerous and some futile.

3. That there are no rules to guide us in distinguishing at the outset those cases which are, from those which are not, amenable to these remedies, the divisions given by Eisenmann Swedia, &c., having no bearing on this point.

4. That where so large a list of remedies is given, some attempt ought to be made to decide with accuracy in what cases each remedy should be tried, which as yet has not been done, so that every cure obtained is only an additional source of confusion.

5. That the reputation of injections has been injured by the want of any certain rules as to the relative value of the different substances employed, and the strength requisite in different cases, thus leading to the indiscriminate application of different substances of the same strength on the one hand, and on the other, to the equally indiscriminate application of injections of the same strength to cases not equally fitted to bear them.

6. That treatment has been made secondary to disputes about the nature, sources, and history of this disease, and to speculations, for they deserve no better title, of the most transcendental kind, about the action of medicines; whereas the cure of disease ought to precede all other considerations; for however great may be the value of science, the welfare of man is a still greater object to every well-regulated mind.

I have spoken plainly on this topic. The trite generalities, the incessant repetitions of many writers are more likely to raise our admiration of their diplomacy, than to give us a clear view of their internal convictions, if they had any! They are worse than the vaculliations of a Greek chorus, where the temporary hero is in one scene a monster of stupid, ferocious cruelty, and in another a high-minded monarch of first-rate conservative principles, justly defending his throne and aristocracy; or where a slave is one moment a miserable martyr or a selfish oligarchy, and subsequently transmuted into a faithful servant, to be duly rewarded in good time with a wreath and a pension from the state.

Perhaps I am suffering from the malady which Livy tells us is chronic among authors, but such are really the only inferences I can draw.

Proposed Plan of Treatment.—After this rather uncomplimentary piece of preliminary discussion, the reader will naturally inquire whether I have anything better to offer in its stead. I reply that I must leave that point to his decision, and beg to submit for examination, first of all a plan of abortive treatment, and to demonstrate the results it seems to offer. To do this properly I must first take the liberty of dividing all cases of gonorrhœa into two classes, viz., those which admit, and those which do not admit, of such a plan.

Those, then, which seem most adapted for it are—

1. Cases where the patients present themselves before great pain and running have set in.

2. Patients who have had gonorrhœa previously, and in whom the present attack does not appear to be very severe.

3. Those cases where the patient is desirous of an immediate cure at any price, and who would rather go through anything for a day or two than have a long illness.

And before going to the details I must digress for a few minutes to combat an opinion which seems very prevalent, and which is that M. Ricord is constantly in the habit of using in abortive treatment of a similar kind, or, in other words, of precluding all measures with a strong injection of nitrate of silver. This may be an incorrect assumption, as I have no written authority for it, but I know I have repeatedly heard it stated, both in private and public, without contradiction. Now nothing could be wider of the mark. M. Ricord's abortive treatment, as laid down in his 'Traité Pratique,' consists of rest, low diet, thirty or forty leeches to the perineum, followed by copaiba, and *mild* injections of nitrate of silver, and he expressly confines his recommendations of a strong solution of this salt to those cases which begin " without pain, without any sign of inflammation."

Abortive Treatment of Gonorrhœa.—Before taking a single step, it is indispensably necessary to ascertain whether the patient can rest for the entire day after, and if not whether he is disposed to suffer considerable inconvenience. If he is unable or unwilling to do either, it is best at once to lay aside all thoughts of an abortive cure and refer the case to the second class.

But if this co-operation on his part can be obtained, the abortive treatment may at once be commenced. The patient should make water, and the surgeon then inject him with a solution of nitrate of silver containing five grains to an ounce of distilled water. The tube of the syringe ought to be at least an inch and a half long, so that the whole of the specific seat of the gonorrhœa is acted upon, and should consist of platinum or silver electro-gilded, as otherwise the salt soon corrodes the soldering and even the silver.

By limiting the strength of the solution to five grains, we avoid the severe pain which is caused by the strong solutions of this salt, and by retaining the injection in the urethra for several minutes, we can in almost every case attain equally as powerful an effect as by using a more concentrated solution for a shorter space of time. Should it fail to occasion either burning or smarting it may be withdrawn, and a solution of ten grains to an ounce employed instead.

The deep burning pain which now ensues is widely different from that produced by the salts of zinc, and is often accompanied by flushes of heat which fairly thrill through the frame. It is, however, generally soon relieved by bathing the penis with hot water, and a hot bath will for the most part effectually remove what the warm water has left undone.

The next step is to prescribe a dose of calomel, seidlitz powders, or citrate of magnesia, at least four grains, followed by purgative draughts of salts and senna every two hours until several loose stools are procured. The bowels should be completely scoured out, and no food allowed except a little warm tea or gruel, to assist the action of the medicines.

After every stool the patient should inject with a solution of sulphate of zinc from three to five grains in the ounce, The injection is to be kept in contact with the mucous membrane, till a slight sense of burning is induced, when it may at once be withdrawn. The penis should be bathed each time with water as hot as it can be borne, and the greater the heat the more complete the relief, not only to the pain produced by injecting, but also to the scalding, weight, &c.

The next day the discharge is generally thin and very small in quantity the symptoms of inflammation have disappeared and the cure is mostly completed in a day or two by the use of the same means, the patient using the injection every time he makes water, and gradually raising the strength of it till it reaches ten grains to an ounce. Mild aperients and low diet may also be continued. When this plan fails, the case may be referred to the second class, for I believe, that abortive treatment to succeed at all, must succeed at once.

Ordinary Treatment.—Every other case of gonorrhœa, every case in which the abortive treatment has failed, or in which it cannot be applied, and every case accompanied by excessive pain and irritability of the urethra, or of long standing, and attended by fixed pain on the under surface of this canal, might be placed in the second class. It is to these that I wish to apply a new treatment, substituting for all internal treatment certain salts of potass, with rhubarb, and injections so combined, graduated, and applied as to act permanently but without much pain over the whole diseased surface.

Having convinced myself by experience of the inutility of the other preparations, the acetate was tried in the hope that the alkali in a diuretic form would act as well, without the disagreeable effects resulting from the administration of the liquor potassæ. I was for some time disappointed, but by combining it with spirits of nitre and rhubarb all the attendant disadvantages were overcome. The best proportions seem to be five drachms of acetate of potass with three drachms of spirit of nitre in a six ounce mixture; an ounce of which may be taken two or three times daily.

In more severe cases the chlorate of potass may be added, and as many disappointments and failures resulted at the outset, an available form of prescription may be of service. The best I have seen is formed by pouring five ounces of boiling distilled water upon two drachms of powdered chlorate of potass, and then adding three drachms of liquor potassæ, and three of the acetate.

When this solution is taken in doses of an ounce two or three times a day, an alteration in the discharge is soon noticed, indeed within forty-eight hours it is often materially diminished, becoming at the same time thinner, less coloured, and more mucous. This effect seems to be produced with equal rapidity, in cases of long standing and recent ones, in women and in men, so that one might be tempted to look upon it as one of the true antiphlogistics in inflammations of this kind.—*Med. Circular, Oct.* 5, 1859, *p.* 165.

98.—*On the Treatment of Chordee.* By J. L. Milton, Esq., M.R.C.S.—After having tried almost every antispasmodic, including ether, chloroform, and sumbal, I can safely say that I have found nothing equal to camphor in the fluid form. In the solid state it does not act so rapidly; and, in fact, a remedy in a liquid form, as it must from its extremely fine state of division act more rapidly, is more suited for spasm. The spirit of camphor offers all the advantages sought for, and, given in drachm doses, is equally energetic and rapid in its action. I have also tried the essence of camphor, a patent medicine; it is a little more agreeable, and a great deal more expensive.

As in many other cases the chain of morbid action should be broken or checked at once; and this is much more effectually done by giving two or three full doses, at short intervals, without the least remission, than by small quantities, however long continued and regularly taken. The surgeon may, therefore, safely adopt the following plan:—

A teaspoonful is to be taken at night in water before going to bed; and every time the patient wakes with the chordee, let him at once rise and repeat the dose. In mild cases, one dose for a night or two is generally enough; and even in more severe cases the spasm is usually very much alleviated by the third or fourth night. So long as the gonorrhœa remains very bad, which ought never to be more than five or six nights, the patient may take a dose before going to bed. This remedy also answers extremely well in the bearing-down pains to which women are sometimes subject in gonorrhœa; but as these pains are generally worst in the daytime, the medicine may be given then; and here it is really a matter of convenience to use the essence of camphor, as it mixes well with any medicine they may happen to be taking.

In both cases, however, it must be given in full doses. A smaller quantity than a teaspoonful of either essence or spirit is of little service; and as this quantity is perfectly safe, it is best to insure success at once. In one or two instances it has produced some sickness, probably from something having been previously taken which had disordered the stomach. This, however, has not occurred more than twice or thrice, and the sickness was of very little moment. So that I only allude to the fact, lest any one might be discouraged by the appearance of this symptom from giving so valuable a remedy as the camphor really is.

The patient should be directed to keep the camphor in a tightly-corked bottle and in a cool place, and to have it at night by his bedside ready to take. It is best taken in plenty of cold water.—*Med. Circular, Feb.* 29. 1860, *p.* 132.

99.—ON SORES OF A NON-SPECIFIC CHARACTER.

By John Harrison, Esq., F.R.C.S., Albany Court Yard.

Of the sores which present themselves on the genital organs after promiscuous sexual intercourse, there are two principal kinds: the one distinguished as the true syphilitic or *indurated* chancre; the other, as the false, simple, or *soft* chancre. Both are contagious, but it is from the former only that secondary or constitutional symptoms arise, though it is the latter which is most liable to be attended by suppurating buboes.

True chancre is usually solitary, or, if there be more than one, they all appear at the same time. On the contrary, there are usually several simple chancres, but instead of occurring all at once, they may appear in succession. Simple chancres are much more frequently met with than true chancres—in the proportion, it has been reckoned, of about two to one. True chancre is not readily inoculable on the body of the patient himself, nor on the body of another person already affected with syphilis; simple chancre, on the other hand, is inoculable, not only on the body of the patient himself, but also on the body of any other individual.

Besides the sores just described, it is important to remember that ulcerations of the genitals are occasionally met with originating in simple causes, such as herpetic eruption, or even a slight injury, abrasion, or excoriation. Such ulcerations, in consequence of a disordered state of the constitution, neglect of cleanliness, &c., may assume an unhealthy and obstinate character, and are liable to be mistaken by those not conversant with the subject for venereal sores.

An old patient called on me a few days ago to tell me that his wife was sinking from carcinoma uteri. This gentleman has experienced for years, at times, excoriations of the glans and irritable discharges from the urethra. He has, however, felt satisfied of the cause. On one occasion, an herpetic sore, from the irritation just mentioned, was a long time before it healed, and might have been looked upon with great suspicion. For the last two years, intercourse with his wife having been prohibited, he has been free from excoriations and urethral discharge.

The matter given out from simple ulcerations of the genitals, though in itself comparatively innocuous, may acquire an acrid and irritating quality, and thus become capable of giving rise to similar ulcerations in another person with whom the affected individual may have sexual intercourse. Nay, symptoms simulating the constitutional symptoms of syphilis, such as eruptions, sore-throat, &c., may, if I mistake not, supervene on such ulcers of the genitals. Whether these constitutional symptoms arise from the absorption of the matter generated by the sores themselves is another question, just as the mode of origin of gonorrhœal rheumatism is a question.

The following is a case in which suspicious appearances presented themselves, though the cause was in reality of a very simple nature:—

A young lady having received an injury on the right labium, swelling and inflammation took place rapidly, and when I first saw her—some few days after the accident—a slough of considerable size was being thrown off from the inner surface of the labium. On separation, it left an ulcer with a sharp, well-defined edge, which proved troublesome, and took a long time to heal. At one period there was considerable thickening about it, so that I was almost inclined to look upon it with suspicion, and was nearly thrown off my guard by its appearance. The hymen was perfect, and there was that maidenly delicacy about the patient which was unmistakable. There was, in fact, nothing venereal about the sore. In such cases we must take every circumstance into account. I have frequently seen syphilitic sores in females of exactly similar appearance, and if I ever saw a chancre—so far as external or visible characters are concerned, it was this. What a grievous mistake might have been made!

Abscess in the loose cellular tissue of the labium, from slight mechanical injury, now and then takes place, which bursts on the inner surface, leaving a sinus that is occasionally a long time in filling up. In a case of this kind, where there is leucorrhœa, a mistake is liable to be made. I have seen condylomatous soft sores between the nates in females, in whom, from their rank in life, neglect or want of cleanliness was scarcely to be expected as a cause. In one instance, the condylomata were connected with an eruption which somewhat excited my suspicions. It is certainly open to doubt whether or not many such cases are really always so simple in their nature, and so free from syphilitic taint, as we might be led to suppose.

Sores occurring on the genital organs there is, then, reason to conclude are not always specific in character, though but too frequently considered as such. A distinction ought, therefore, to be drawn between simple sores and those which are truly syphilitic and liable to be followed by a train of after-symptoms, in consequence of the absorption from them of a specific poison into the system. It is most important, at the commencement of the treatment of sores on the genital organs, to determine, as far as possible, their character—whether venereal or non-venereal; and if venereal, whether syphilitic or non-syphilitic; or, in other words, whether the sore before us be one which requires for its cure merely simple, mild, unirritating local applications, with attention to the general state of the health, or whether it requires a specific plan of treatment. Those who have had much practical experience in this department of our profession must admit that this is a question which in many instances it is almost impossible to decide off-hand. From my own experience, I do not believe that we can, in all cases, discriminate at once and with entire certainty (whatever may have been written or said on the subject) those sores which will be followed by secondary symptoms, and those which will not, at whatever stage we may see them. Thus, it is frequently found that, in some states of the constitution, a trivial excoriation, having no venereal origin whatever—by this I mean, not derived from impure sexual intercourse,—if mismanaged, or irritated by the application of over-stimulating or unctuous substances, is liable to be altered in character by the unhealthy inflammation thereby excited, and made to assume a suspicious appearance; whereas a really syphilitic sore, scarcely attracting notice, being left to itself, often heals spontaneously, though it may be followed by secondary symptoms. It is therefore wrong to be too hasty in pronouncing sores syphilitic, and forthwith to administer mercury. In the worse alternative, nothing is lost by a little delay; whilst, if the originally simple non-venereal or non-syphilitic sores are treated as specific, by the uncalled-for administration of mercury, &c., much mischief may be done.

How frequently do we see a common graze or cut finger put on a bad aspect, with inflammatory induration surrounding it, and show little disposition to heal; the same thing often takes place with a simple abrasion or excoriation of the delicate investing membrance of the glans or inside of the prepuce. I am inclined to think that in many cases sufficient attention (particularly with reference to ulcers of the genital organs) is not given to the known indisposition, in certain states of the constitution, which a sore or ulcer shows in putting on the healing or reparative process, in whatever part of the body it may be situate. We ought not, therefore, to expect some ulcerations of the organs in question to differ from ulcerations elsewhere.

It is a fault too frequently committed to consider the generality of ulcerations of the genital organs we meet with in practice as venereal. We do not, I fear, take sufficiently into consideration the liability of these organs to the same mechanical and chemical injury as other parts. I have, over and over again, seen sores, which had been pronounced syphilitic, heal under the simplest possible treatment, no specific plan having been adopted; and I have had opportunities of watching the result,—no secondary symptom have shown themselves in these cases. Had mercury been administered and irritating applications been had recourse to, the result might probably have been different. The balance of the system would, at least, have been unnecessarily disturbed, and an undue susceptibility to morbific causes created. The simplest non-venereal sore may be surrounded by induration;

were we, therefore, to give mercury for every sore with surrounding hardness we encounter, we should, indeed, frequently be prescribing the medicine unnecessarily. In strumous subjects, there is often induration around a sore. Such cases are but too commonly made worse by the injudicious administration of mercury.—*Lancet, Feb.* 4, 1860, *p.* 113.

100.—SUCCESSFUL TREATMENT OF GONORRHŒA AND GLEET WITHOUT COPAIBA.

By Weeden Cooke, Esq. Being an abstract of a paper read before the Harveian Society.

The author commences by stating it as the result of a very large experience, that copaiba in the treatment of gonorrhœa is not only unnecessary, but even, in a great many instances, injurious, and in all offensive to the last degree. As introductory to the subject of treatment, Mr. Cooke discussed the subject of the cause of chordee, the reason for the scalding of the urine, the distinction between true and spurious gonorrhœa, and the time when infection may or may not be apprehended. The conclusions he arrived at were—

1st. The chordee, in ninety-nine cases out of a hundred, was due to spasm, and not to effusion of lymph; and that cubebs, acting as an antispasmodic, formed the most efficacious remedy for this symptom.

2ndly. That the scalding was the result of the acid urine passing over the highly inflamed mucous surface of the urethra; and that this was to be remedied by the administration of the alkaline carbonates, for the purpose of neutralizing the acidity of the urine, thus removing the principal cause of the continuance of the inflammation.

3rdly. That in all disputed cases, the true gonorrhœa may he known from the spurious by the presence of redness, heat, pain and swelling, together with a purulent discharge, more or less green or offensive; whilst a discharge produced by connexion with a person who has leucorrhœa, or has recently been confined, will not be accompanied with these inflammatory signs and the discharge will be milky in consistency and colour, differing much from the thick purulent discharge of the veritable gonorrhœa.

4thly. The time when infection may or may not be apprehended was discussed at its incubative stage, and at its close. A case was given, showing that the disease may be caught from one person, and not communicated to another two nights after, because the purulent discharge had not commenced at the time of the second intercourse. Respecting infection at the close of the disease, the author had been enabled, from experience, to establish as a law for his own guidance, that gleet—i. e., a mucous discharge from the urethra, consequent upon gonorrhœa—does not set up gonorrhœa in another person; but that, whilst any pus is to be found in the discharge, there is probability of infection.

Passing on to the subject of treatment, Mr. Cooke said that, upon inquiry at the London Custom House, he found that 118,396 pounds of copaiba were admitted into the Port of London only, during the first ten months of the year 1859. If this be administered at the rate of half a drachm three times a-day, and supposing each person takes it for three weeks, we have here copaiba enough to treat 473,584, or close upon half a million persons, and that in ten months only of the year. Considering how often it fails to cure the disease; how frequently it is rejected by, or at any rate, disorders the stomach; how tell-tale and disagreeable is its odour from the mouth and skin; how occasionally it produces a papular eruption all over the body; and that, in many instances, swelled testicle and stricture may be traced to its irritating influence, while gonorrhœa, rheumatism, and ophthalmia have been attributed to its administration; "considering all these objections," the author remarked, "is it not extraordinary that this disgusting medicine continues so long to hold its ground?" The abortive treatment by strong injections of nitrate of silver had proved a failure because in some instances inflammation of the bladder had resulted. The treatment by diluents was slow in its action, and not readily employed by persons engaged in active business. That by diuretics was scarcely more successful; whilst the administration of saline aperients was generally attended with an aggravation of the ardor urinæ as well as chordee. The treatment which had been most successful in the author's experience was the chemical treatment by the alkaline carbonates, given with a view of neutralizing the acid in the urine. Thus one great source of irritation was removed from the inflamed urethra, and the subsidence of the inflammation, which nature would effect, was allowed to take place. As auxiliaries, especially when there is œdema of the prepuce, lead lotions, and elevation of the penis against the abdomen, were commended. The inflammation having subsided and a muco-purulent discharge being left, the author had found, after giving trial to all the injections which have been at any time in vogue, that the chloride of zinc, introduced into this branch of practice by Mr. Lloyd, of St. Bartholomew's, was the most efficacious of any in curing the disease, and that with less discomfort, and in a much shorter time than by any other means. Since employing this treatment, he had had little, if any, orchitis amongst his patients. The strength of the injection he most commonly employed was two grains to the ounce, but in some instances one grain to the ounce is sufficient. Whilst advocating this treatment in persons of healthy constitution, it was necessary to completely change it in others. In the strumous, in the dyspeptic, in those of dissipated habits, and where the diseased person is an old offender, the alkaline carbonates are not called for, because either the urine is not acid or the inflammation does not run high. In such cases the tincture of iron, or sulphuric acid and bark, or gentian, or calumba, may be advantageously employed from the commencement; and the chloride-of-zinc injection in these cases is also of the utmost value in rapidly overcoming the disease.

Respecting diet, the author considered that after the subsidence of the inflammatory symptoms scarcely any restriction need be enforced, and that beer or wine in moderate quantities may be advantageously used by those who are accustomed to these beverages. He had found long-established cases of gleet yield readily to the chloride-of-zinc injection, accompanied with tonic treatment and generous living.—*Lancet, Jan.* 28, 1860, *p.* 92.

DISEASES OF THE EYE AND EAR.

101.—ON DIVISION OF THE CILIARY MUSCLE IN GLAUCOMA, &c.

By Henry Hancock, Esq., F.R.C.S., Senior Surgeon to the Royal Westminster Ophthalmic and Charing-Cross Hospitals, &c.

[Mr. Hancock regards glaucoma as depending upon an arthritic condition of the blood, and that the bloodvessels, sooner or later, become diseased and changed in structure, in the same way as the vessels and valves of the heart are affected in arthritic disease.]

According to Bowman, these vessels, in recent cases, excepting small aneurismal swellings of the retinal capillaries, seem to have a healthy appearance, and are free from atheromatous and fatty degeneration, but in the more advanced cases these changes are met with. It is not unreasonable, therefore, to infer that, though in the earlier stages there may not be any appreciable change of structure in the minute vessels of the retina and choroid, still they are in an unhealthy condition, predisposed to regeneration, and less able to resist pressure or to overcome any interruption to the circulation of their contents. Accordingly they form little capillary pouches in the retina, which frequently give way, the blood either spreading amongst the elementary structures of the retina, or, bursting through the hyaloid membrane, forms clots in the vitreous humour, whilst the larger vessels of the choroid, becoming varicose, by their pressure outwards cause absorption of the sclerotica, and produce stapylomata of that coat of the eye. Consequent upon these changes in the bloodvessels, the humours of the eye are affected; the vitreous humour, containing blood-discs and fibrous sheds, assumes a yellowish tinge, whilst the lens has the same or a greenish tint. As the disease progresses, effusion takes place within the eyeball, rendering it exceedingly tense and hard by the resulting intra-ocular pressure, which, acting upon the ciliary nerves and retina, causes intense pain, and ultimately total blindness.

A difference of opinion appears to obtain as to the exact seat of this effusion. Mackenzie, for instance, (p. 896, 4th edit.) says: "The dissolved state of the vitreous humour, which my dissections of glaucomatous eyes lead me to consider as forming part of this disease, is always attended, at least in the middle stages of glaucoma, by an abnormal firmness of the eye to the touch, evidently arising from over-distension of the tunics." Baron Von Gräfe, according to Wolfe, ('The Lancet,' Dec. 10, 1859,) considers acute glaucoma to be a choroiditis, or an irido-choroiditis, with an infusion into the aqueous and vitreous humours, the augmentation of which causes intra-ocular pressure, thus producing excavation of the optic nerve and irido-plagia; whilst we read in Hulke's description of Bowman's dissections that, so far from the vitreous humour being dissolved or loaded with fluid, it is of remarkable consistence, and does not flow readily. When viewed by the ophthalmoscope, a glaucomatous eye usually presents a dilated state of the retinal veins, which are often tortuous and turgid with blood. Small ecchymoses are seen scattered over the surface of the retina, and occasionally small blood-clots in the vitreous humour, with pulsation of the arteria centralis retinæ, and excavation of the optic papilla.

In June, 1856, Baron Von Gräfe performed his celebrated operation of irridectomy for the first time. "The mode in which he performs it is by passing a Beer's cataract knife into the sclerotica, about a line's distance from the margin of the cornea on the outer side, and, having directed it forwards into the anterior chamber, cutting upwards so as to make an incision nearly half an inch in length. Through this the iris readily prolapses, or, if it do not, is readily drawn by forceps, and a portion of it, varying from a fifth to a third of its whole, is cut away. The remains of the prolapsed iris is left in the wound."

How the removal of one-fourth or one-fifth of the iris will cure a disease the pathological appearances of which I have just given you, has never been satisfactorily explained. It is said to be very successful, although its warmest advocates are by no means agreed as to the *modus operandi* of the operation. According to Hulke, "Von Gräfe conjectures that the excision of the portion of the iris removes pressure or tension by the diminution of the secretory surface of the iris by the relaxation of the tensor muscle of the choroid (ciliary muscle), and the influence exerted over the circulation of the choroid." According to Jordan, he maintains, "that the relation between the two chambers is altered, and that a removal of a portion of the iris tends to restore the communication desired; whilst, according to Hildige, he asserts, "that by excising a portion of the iris, the secreting surface is diminished, and its secretion thereby reduced to a minimum."

Mr. Bowman supposes that the primary relief of the internal tension results from the escape of the aqueous humour at the time of operation, and its continuous trickling; that "as the wound becomes united, the gap in the iris allows the aqueous and vitreous humours to come together, of course with the intervention of the delicate hyaloid and suspensory ligament; whereas the iris was before an effectual barrier between them, as shown by the researches of Cramer, Donder, and others." He conceives the result of excision of the iris probably is, "the redundant fluid effused behind and mingled with the vitreous humour, causing it to compress the retina, is permitted to transude into the aqueous humour, and then to escape through the cornea (a road not previously open to it); or by being absorbed by the vessels distributed on the anterior surface of the iris. As this removal of effused fluid gradually takes place, opportunity is given for gradual restoration of the aqueous humour in its natural quantity, and for the return of the lens and iris to their proper positions, whilst the globe acquires more firmness without becoming tense."

Mr. Critchett, on the other hand, considers that "time is allowed for the adjustment of the normal tension, and a sort of safety-valve is left for a time, to prevent such equilibrium from being again disturbed."

You will observe from the preceding quotations that although Von Gräfe conjectures that the excision of a portion of the iris, besides other results, removes pressure or tension by the relaxation of the ciliary muscle and the influence exerted over the circulation of the choroid, the prevailing opinion is, that blindness in acute glaucoma mainly depends upon intra-ocular pressure from effusion, and that the operation of iridectomy, by evacuating the fluid, diminishing the secreting surface of the iris, and allowing the contact of the aqueous humour with the hyaloid membrane, effects a cure. If this opinion be correct—if the great object for attainment be simply the removal of intra-ocular pressure, it is extremely difficult to discover the superiority of iridectomy over "paracantesis oculi," as practised by Reverius in the year 1679, or in our own time by Middlemore and Desmarres; neither is it clear how, by the excision of one-fourth or one-fifth of the iris, the effusion can be reduced to a minimum, since three-fourths of the iris, with the whole of the choroid remain. And the same doubt attends the third hypothesis, that relief is obtained by allowing the aqueous and vitreous humours to come together (the delicate hyaloid intervening); for if the assertion of Cramer, Donder, and others, that in the normal condition of parts the iris is an effectual barrier between these two humours, is correct, we can scarcely understand how the mutilation of so important a constituent of the eyeball as the iris, and the substitution of an unnatural for a natural relation of parts, can lead to the restoration and subsequent preservation of a normal and healthy performance of function. The following question may also fairly be asked: If the relief, as conjectured by Gräfe, results from the relaxation of the ciliary muscle, and the influence thereby exerted over the circulation of the choroid, why should we seek for this relief in a roundabout manner, by removing a large portion of the iris, and producing permanent disfigurement and the objectionable results attending this procedure? Why should we not at once attack the ciliary muscle itself?

I differ from those who regard acute glaucoma merely as a choroiditis, or an irido-choroiditis, with infusion into the vitreous and aqueous humours, as they seem to me to regard results as causes. I believe that glaucoma, whether acute or chronic, is essentially a disease of the blood and blood-vessels, and that the effusion or infusion, as may be described, is the result of this condition, which if not arrested sooner or later, destroys sight. I do not, therefore, believe that any operation will of itself cure glaucoma, but that, by removing the impediment to the circulation through the blood-vessels of the choroid and retina, the disease may be arrested until, if not too far advanced, it may frequently be cured by the aid of constitutional remedies.

I have observed, what I have not found noticed by any previous writer, that in acute glaucoma the eyeball is constricted and marked by a circular depression at the point corresponding to the ciliary muscle, whilst the vessels around this part are gorged to a great degree. The eyeball is elongated in its antero-posterior diameter, and the cornea lessened in all its diameters, and rendered more conical than natural; whilst, when the patient turns his eyeball sideways, irregular bulging of the sclerotica (staphyloma) is exposed to view. In one or two cases, also, in which I performed iridectomy, the pupil was dilated to excess, and the iris so tense and rigid that it resembled a piece of cat-gut, and could with difficulty be drawn through the wound. It is not at all clear how regular equal pressure from fluid within the eyeball can, *per se*, produce cupping of the optic papilla, pulsation of the retinal artery, aneurismal swellings of the retinal veins, a varicose condition of the choroid veins, &c.

Equable pressure from confined and compressed fluid exerted in all directions from within outwards, would, if uninfluenced by extraneous circumstances, tend rather to stretch the retina and choroid, and thus prevent the cupping of the optic papilla and bulging of the choroid. We can, however, readily understand that, the lateral expansion of the eyeball being, in a great degree, prevented by the constriction of the ciliary muscle, the force of the compressed fluid acts more powerfully in the antero-posterior direction; hence the puckering and cupping of the retina, the irregular bulging of the choroid, the alteration in the shape of the cornea, and the elongation of the eyeball.

Having carefully marked these several changes, and studied them in conjunction with the appearances observed by the ophthalmoscope in the interior of the eyeball, and with those seen on dissection, I compared them with the normal anatomy of the eye. I directed my attention to the connexion between the inner elastic layer of the cornea with the ciliary muscle, and considered how the vessels from the choroid pass through this muscle to reach the iris, the peculiar arrangement of the vessels of that latter organ, and of the choroid (especially of the choroidal veins) with regard to the ciliary muscle. I regarded also the relation between that muscle and the ora serrata of the retina, as well as the distribution of the retinal vessels close to their junction.

All these considerations led me to suspect that the ophthalmoscopic and pathological appearances of the blood-vessels were greatly enhanced by, if not, in some instances, entirely due to, the obstruction of the circulation caused by the undue and excessive constriction exerted upon them by the spasmodic or extreme contraction of the ciliary muscle, analogous to the spasm so often observed in the muscular fibres of the urethra, as well as in the sphincter ani muscle in certain affections of those parts.

This supposition was strengthened by the character of pain so often described to me by patients as ushering in the attack of acute glaucoma; for instance, a lady, to whom I was called by Mr. Jackson, informed me that having been exposed to a very strong light at a party given by one of the foreign ambassadors, she felt, upon her return home, as though she had received a violent blow upon her eye, followed by excruciating spasmodic pain, which lasted for several hours.

From these facts, I was led to hope that by cutting the muscle across, as we divide the sphincter ani under analogous circumstances, I should not only get rid of the effused fluid and relieve the constriction of the different parts connected with the ciliary muscle, but at the same time, by removing the impediment to the circulation of the blood, favour the return of the vessels to their normal condition, and so prevent a recurrence of the effusion into the eye; and I was the more inclined to make the trial inasmuch as, whilst failing, after the most careful study of what had been written of Gräfe's operation, for and against, to discover the principles which regulate its performance, I found that even where most successful, it causes certain results which it is most desirable should be obviated. For instance—

1. The disfigurement resulting from the removal of a portion of the iris, and the formation of a coloboma iridis.

2. The removal of one-fourth or one-fifth of the iris.

Whatever difference of opinion obtains with regard to other points connected with iridectomy, there does not appear to be any on this. All agree that the smaller the quantity of iris removed, the better. "By the excision of a portion of the iris, the edge of the lens, with its suspensory ligament passing in front of the vitreous humour to the ciliary process, is exposed to view; therefore, to remedy this inconvenience, Mr. Bowman makes an incision above, because he believes that the cover thus given by the upper lid to the margin of the lens, which has been exposed by the removal of the iris, contributes to the perfection of vision."

3. The loss of the power of adapting the eye to near objects, which it in some degree retains in chronic glaucoma.

"The exercise of this power depending upon the increased curvature of the lens in the pupillary area from the pressure of the iris on its margin,—in action which becomes almost, if not quite, impossible when part of the iris is excised."

By the operation which I am about to propose to you, these inconveniences are avoided. It is very simple, and may be performed easily and quickly.

I introduce a Beer's cataract knife at the outer and lower margin of the cornea where it joins the sclerotica. The point of the knife is pushed obliquely backwards and downwards until the fibres of the sclerotica are divided obliquely for rather more than one-eighth of an inch; by this incision the ciliary muscle is divided, whilst the accumulated fluid flows by the side of the knife. This procedure is rarely followed by bad symptoms. In one case there was inflammation, but it was reduced without difficulty. The operation appears to me to present the following advantages:—

1. It obviates the objections to iridectomy.
2. It relieves pain by the removal of the constriction of the eyeball, and the consequent pressure upon the nerves from the undue contraction of the ciliary muscle.
3. By it, the accumulated fluid is evacuated, and, the impediment to the circulation through the blood-vessels being got rid of, they are placed in a favourable condition to recover their normal state; and the probability of a recurrence of the effusion is greatly diminished.
4. By the situation and oblique direction of the incision, a free drainage of the fluid is provided for,
5. The iris is but slightly wounded, and the pupil is preserved of its original size and shape, and in its normal situation.
6. The danger of wounding the lens is avoided.—*Lancet, Feb.* 11, 1860, *p.* 133.

103.—*Laceration of the Upper Lid; Great Difficulty in procuring Union.*—Every now and then, after operations for hair-lip and other plastic procedures, on removal of the sutures, the parts again separate, to the great disappointment of both patient and surgeon. The following case may serve as a good model of the persevering treatment which ought to be pursued under such circumstances:—

A healthy lad, aged 15, had the upper eyelid of his left eye torn by catching it on a hook. The laceration involved the cartilage, and extended from the free border of the lid, in a direction upwards for at least half an inch, causing a gaping wound. He was taken to St. Thomas's Hospital shortly after the injury, and the torn parts were united by sutures. The accident occurred on Friday, December 23. The sutures tore out, and on the Monday following he was brought to the Ophthalmic Hospital with a large and most unsightly wound. The house-surgeon to the hospital lightly refreshed the edges and put in wire sutures. These again tore out, and the wound gaped as before. Hare-lip pins were now used, but for a third time the hopes of union were disappointed. On January 9, more than two weeks after the injury, Mr. Dixon had the boy put under chloroform and pared the edges freely, and having done so passed a hare-lip pin a little above the free edge of the lid and at a considerable distance from the margin of the wound, so as to give it good hold. The upper part of the wound was brought together by two points of interrupted suture. From this last (the fourth) operation most perfect results were obtained. The parts united firmly, and a fortnight later, when the boy came under our observation, only a linear cicatrix was visible, and there was no irregularity in the contour of the lid—*Med. Times and Gazette, Feb.* 18, 1860, *p.* 165.

103.—ON LACHRYMAL OBSTRUCTIONS TREATED ON MR. BOWMAN'S PLAN.

By T. Pridgin Teale, Jun., Esq., M.A. Oxon., F.R.C.S.

[Mr. Pridgin Teale has now for more than two years frequently practised the mode of treatment recommended by Mr. Bowman for the relief of lachrymal obstructions. He first describes the mode in which he has usually performed the operation.]

Mode of Operating.—Critchett's grooved probe is introduced vertically into the lower punctum, and then passed in a horizontal direction along the canaliculus, and, if possible, into the lachrymal sac, so that its point may rest firmly against the nasal wall of the sac. Should the punctum be too small to admit the probe, it may be enlarged with the point of a cataract-knife, or an attempt may be made to pass the probe through the upper punctum and canal. The entrance of the probe into a distended sac may, in most cases, be easily known; since pressure of the probe outside the sac renders it more tense, whereas little or no increase of tension takes place on pressing the probe if it has fairly entered the sac. In cases in which the sac is not distended, the arrest of the probe in the canaliculus external to the sac may be inferred, if pressure with the probe causes a dimpling of the skin over the tendo occuli. For slitting up the canal a common Beer's knife answers well; the point of which may be carried along the groove in the probe until it is arrested by the termination of the groove; at this stage, in order to free the knife from the groove, the probe may be rotated on its axis, without changing the direction of the knife, which may be then thrust onwards until it is arrested by the nasal wall of the sac. A sufficient opening may thus be secured into the sac.

This mode of dividing the canaliculus differs somewhat from that of Mr. Bowman, in which the canal is not slit up beyond the caruncle. He is able thereby to introduce into the sac his largest probe (No. 6) through the unwounded part of the canal. Mr. Bowman's mode, which I have occasionally adopted, has the advantage over the other of inflicting a smaller surgical wound. My reasons for generally adopting the complete division of the canaliculus are,

1st. That it is more easy to introduce the probe into the sac, both at the time of the operation, and, subsequently, should the probing be performed at long intervals.

2nd. That the patient suffers less from the introduction of the larger probes into the sac.

3rd. That a probe two or three sizes larger than Mr. Bowman's No. 6 can be used without difficulty from the commencement.

4th. That I have not hitherto observed any evil resulting from it.

After slitting up the canaliculus I have seldom found any difficulty in keeping it open, as in most instances the lachrymal passages were in a state of chronic inflammation consequent upon obstruction of the nasal duct, a condition unfavourable to immediate union. In the only cases in which I met with much difficulty the punctum was strictured, but there was no attendant irritation of the mucous lining of the canal. In such a case it might be well to excite a temporary irritation of the newly-cut edges by the application of sulphate of copper.

I have seen Mr. Bowman use a convenient substitute for the grooved probe and cataract knife, namely, a narrow-bladed knife with a blunt point, which he introduces at the punctum and carries along the lachrymal canal to the extent required.

Entrance into the sac having been secured, one of the larger probes is passed down to the strictured part. If the obstruction yields easily, the probe is pressed on till it reaches the floor of the nostril. If, however, it does not yield to moderate force, it is better to delay further efforts for a few days. This allows time for the parts to become less swollen and irritable from the relief afforded by the free division of the canaliculas; and, at a second attempt the probe may be pressed more boldly against the strictured part.

The probes which I have used are of two kinds: first, the series 1 to 6 described by Mr. Bowman, which are uniform in calibre and are curved in two planes to correspond with the nearly spiral shape of the nasal duct; secondly, bulbed probes, the size of the bulb in two of them being equivalent to Bowman's No. 3 and 6 respectively; and a third probe, having a bulb two sizes larger than No. 6, which I have called No. 8. The bulb of No. 8 is of the size of the ordinary wire gauge No. 15.

These bulbed probes are curved in one plane only, the narrow part above the bulb allowing them to lie in, and in some degree to adapt themselves to, the irregular course of the duct. The bulbed probes I find, perhaps from habit, more easy of introduction than Bowman's, and I think they give less pain to the patient. I prefer Bowman's large probe when any considerable pressure is required to overcome an obstruction, as the direction of the pressure can be made thereby more vertically, and more in the axis of the nasal duct, than by the bulbed probes which are curved only in one plane. After the probe has been once passed through the nasal duct, the treatment is usually very simple. The probe may be passed a second time, if necessary, at the end of a week, and may be repeated weekly, or at progressively increasing intervals, as long as the epiphora continues to relapse. On first adopting this treatment I used the probe every second or third day; but, finding the frequent repetition unnecessary, and perhaps injurious, I have been gradually led to allow longer intervals to elapse before each operation.

In my earlier cases I used the styles with the ends tapered, bent, and at a right angle, formerly suggested, but generally laid aside, by Mr. Bowman. These were introduced through the divided canaliculus into the sac and nasal duct, and allowed to remain for periods of four, eight, or twelve hours. They appeared, however, to retard progress by the irritation which they caused. Since the publication of Mr. Bowman's paper in November, 1857, in which he advised the "intermittent use of probes," I have never had recourse to those bent ones, though exceptional cases may occur in which they may prove of service.

When a patient has been free from epiphora for a month, he may be released from further treatment, unless there should be any relapse.—*Med. Times and Gazette, Jan. 7, 1860, p. 9.*

104.—ACUTE SCLEROTITIS AND IRITIS,

ILLUSTRATING THE ANTIPHLOGISTIC POWERS OF MORPHIA.

By J. ZACHARIAH LAWRENCE, Esq., F.R.C.S., M.B., Surgeon to the South London Ophthalmic Hospital.

[Perhaps the most practically important distinction in the ophthalmiæ is, whether the conjunctiva or the sclerotic is the part principally affected. In the latter, the pain is deeper seated, more paroxysmal, more agonizing. The treatment usually adopted is bleeding, blistering, and mercurialization. In the first of the following cases, the author at first administered the morphia, with the view of relieving the excessive pain, no further result being looked for. But he found that morphia really seemed to have powerful antiphlogistic powers, the decline of the disease soon following its free administration.]

Case 1.—*Acute Sclerotitis—Morphia Treatment—Decline of the Disease in about Four-and-twenty Hours.*—S. S., a middle-aged woman was admitted to the South London Ophthalmic Hospital on Nov. 3, 1858. The sclerotic was intensely injected, the conjunctiva slightly; the "sclerotic zone" well marked. She suffered such severe shooting pain in the eye-ball, eye-brow, and infra-orbital region, as to render her quite sleepless.

Nov. 3. ℞. Morph. hydrochlor. gr. ¼ every third hour. Warm water fomentations to the eye.

6th. Took the morphia regularly up to four p. m. yesterday, when she took the last powder. Towards the evening of the 4th the pain in the eye began to abate; now she feels but a slight aching in the eye, on exposure to light. The sclerotic vascularity has considerably diminished. She now recovered rapidly under the treatment of a slight conjunctivitis.

Case 2.—Acute Sclerotitis—Morphia Treatment—Decline of the Disease in less than twelve Hours.—H. B., an elderly, but strong man, admitted to the St. Marylebone Dispensary, July 27, 1859. Sclerotitis of a week's duration characterized by intense vascularity of the sclerotic, and a "sharp, burning" pain in the eye-ball and forehead, with nocturnal exacerbation, rendering the patient sleepless. Suffering simultaneously from gout in the great toe. Has done nothing but foment the eye.

℞. Morph. hydrochlor. gr. ¼, 3tia. quaque horâ. Warm water fomentations to the eye.

July 29th. Has taken the medicine as prescribed. Slept well, but not heavily, on the night of the 27th after midnight, when the hitherto severe pain in the eye left him. To-day, the vascularity of the tunics greatly diminished; the pain in the eye-ball, brow, and forehead gone, leaving but a trifling pain at the side of the nose. Bowels have not acted since the 27th. To leave off the morphia and take an ounce of castor-oil.

Aug. 1. Perfectly recovered.

Case 3.—Double Acute Iritis—Failure of Leeching and Mercurialization—Morphia Treatment—Decline of Disease within Four-and-twenty Hours.—E. P. was admitted to the South London Ophthalmic Hospital on Aug. 27, 1859, during my absence from town, and up to Sept. 10, when I first saw her, had been treated by leeching, blistering, murcurialization, and belladonna lotion for the previous three weeks.

Sept. 10. Iris discoloured; sclerotic deeply injected; pupils dilated (from the belladonna lotion); humours muddy; complaints of pain in the eye-balls and eye-brows; "like a rheumatic pain, of an overwhelming weight, of the light causing her great agony;" eye-sight very dim.

℞ Morph. hydrochlor. gr. ½, 4ta. quaque horâ. Warm water fomentations to the eyes.

14th. Took the first dose of medicine on the night of the 10th. The pain abated, and she expressly stated "very suddenly." She slept that night. On the following morning she could face the light much better. The medicine has made her feel very sick and drowsy. To-day, she complains only of a little "pricking and shooting pain." Her eyes are still dim and weak, but the sclerotic injection is nearly gone.

28th. Since the last report has been taking the morphia in diminished doses, and subsequently a grain of quinine three times a-day. Her eyes are to all appearance perfectly sound; nothing remains of her disease but a slight haziness of vision.

Case 4.—Acute Sclerotitis—Morphia Treatment—Decline of the Disease in about Seven Hours.—B. L., aged 40, a working engineer, was admitted to the South London Ophthalmic Hospital on Sept. 24, 1859. Five or six years ago he was struck on the now inflamed eye by something from a forge-fire. He recovered from the accident in about a month. The eye has been inflamed, as it is now, for the last three weeks. It presents all the usual signs of acute sclerotitis; great sclerotic vascularity (the "sclerotic zone" well marked), excessive lacrymation, great pain (especially at night, rendering him sleepless) referred to the eye-ball, eye-brow, and temple, and compared by the patient to the sensation of "a weight hanging from his forehead, and pulling him down;" eye-sight "foggy;" over inner part of the cornea, a rose-coloured opaque speck, with a minute depression in its centre, evidenced the accident of five years back, but the most careful examination failed to detect any foreign body in the anterior chamber.

Sept. 24. ℞. Morph. hydrochlor., gr. ½, every third hour, watching its effects; warm water fomentations to the eye. Took the first dose about four p.m., felt sleepy about six p.m.; second dose about seven p.m.; the pain began then gradually to "die away;" the third dose about eleven p.m. —slept for three or four hours. The following day (Sunday) at noon but trifling pain was felt, and he slept soundly that night.

28th. The case was reduced to one of slight conjunctivitis; all pain has left him; found his bowels confined from the medicine. To leave off the morphia, and take a purgative dose of calomel and colocynth, which completed the cure.

Case 5.—Traumatic Acute Sclerotitis—Morphia Treatment—Decline of the Disease in less than Four-and-twenty Hours.—C. H., aged 46, was on a Thursday evening engaged in Messrs. M.'s factory, pouring some molten iron into a sand mould, when a quantity of hot sand flew into his eye. He came to the South London Ophthalmic Hospital on Saturday, Oct. 1, 1859. With the exception of two minute particles of sand, which I removed with a spill of blotting paper, all the sand had been removed by one of his fellow-workmen. I found him suffering from intense sclerotitis, marked by universal and high vascularity of the sclerotic and conjunctiva, great lacrymation and excessive pain in the eye-ball, compared by the patient to the "prodding of a knife," and rendering him quite sleepless.

Oct. 1. ℞. Morph. hydrochlor., gr. ½, every third hour. Warm water fomentations.

Took the first dose of morphia about three p.m., and then regularly every three hours. It made him feel very drowsy, and that (Saturday) night he slept soundly. The violent pain was entirely gone on the following morning.

3rd. The case reduced to one of a simple conjunctivitis and treated by a purgative dose of calomel, which completed the cure.

Case 6.—Acute Sclerotitis—Failure of the Morphia Treatment—Recovery under Depletion and Mercurialization.—E. S., aged 48, applied at the South London Ophthalmic Hospital on January 12, 1859. About twelve months before she lost the sight of the now-inflamed eye by a cork from a soda-water bottle. The consequent inflammation of the eye lasted for only a few days; but three or four months afterwards her eyesight began gradually to fade, and she can now only distinguish the outlines (but not the colours) of objects with the injured eye. About three weeks before applying to the Hospital she caught cold in the eye, which now presents the following signs:—Intense sclerotic and conjunctival vascularity ("sclerotic zone" well marked); pupil central of medium size, angular, destitute of contractility. Pain intense, referred to the right eye-ball and right side of the head, proceeding from the vertex downwards to the level of the ala nasi.

Jan. 12. ℞. Morph. hydrochl., gr. ⅛, every fourth hour. Warm water fomentations to the eye.

15th. Pain and other symptoms unabated. She recovered slowly under leeching, blistering, and mercurialization.

Whether in this case the deeply diseased state of the eye, or the (too) small doses of the morphia, influenced the failure of the drug, must remain a matter of conjecture.

These cases I consider to establish an important practical fact, viz., that morphia is *per se* a powerful antiphlogistic, capable of curing these acute inflammations of the eye, in which up to the present time blood-letting, blistering, and mercurialization have been considered necessary. As regards loss of blood, all will be agreed on the propriety of dispensing with it, where it can be done so with safety. Again how constant an occurrence is it to see paroxysms of acute inflammations for a time apparently relieved by blood-letting, till the subsequent vascular reaction sets in, but to recur again and again, and require as many repetitions of this same objectionable remedy. I would further ask surgeons and physicians, What evidence have they that in the combination of mercury and opium* given with a view of "putting the patient under the influence of mercury," as it is termed, is it not really the *opium* which does the good, and that the mercury and its action on the mouth may not be, to say the least, useless? And I would finally ask the physicians of this country to test the powers of morphia in the treatment of the acute inflammations of the internal organs of the body.

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* Again, mercury is presumed to have an "absorbing power" over plastic effusions, such as occur in acute iritis; here too it is a fair question whether the absorption of the inflammatory exudations is not rather a natural process, supervening on the cessation of the inflammation (such as we daily see in the absorption of divided cataracts after the operation by solution, as soon as the inflamma-

If we seek for an explanation of the above very remarkable action of morphia in reducing abnormal fulness of the vessels of the sclerotic, we may find it in the relations of pain to vascular congestion. Pain has generally been regarded rather as the effect, than as the cause of the repletion of blood-vessels; but it is quite an open question, whether or not in certain classes of cases the order of things may not be inverted? Such may be the case in the inflammation of the sclerotic we have just been discussing. That on the other hand, vascular congestion may react as a cause of pain, is not improbable. The theory I would submit is that the action of morphia in these cases depends on its known power of reducing nervous irritability, which may be viewed as the primary cause of the inflammation. In these deep-seated inflammations of the eye this view is very much borne out by the seat of the pain; this will be found to follow strictly the branches of the fifth nerve; indeed the precision with which the patients themselves localise the pain is very remarkable, whilst we have further evidence of the nervous nature of these cases in the intense watering of the eye (dependent on irritation of the lachrymal branch of the fifth nerve). In this way I conceive the irritation is propagated to the vessels through the intervention of the connexions existing between the fifth and sympathetic nerves.—*Med. Times and Gazette, Dec*, 31, 1859, *p*. 651.

105.—*Polypus of the Ear.—Treatment by Torsion.*—The patient was a woman, aged 30. She had had the disease six years. It had on one occasion been partly removed, but was now of large size and extended from the meatus. It was covered with cuticle and was quite firm. Mr. Bowman had punctured it, and evacuated some serum, and thus reduced its size; but it was soon again as large as ever. Mr. Bowman said that it was impossible to state with certainty the point from which it grew, but it was probably the membrana tympani. Various methods of treating polypus of the ear were in use. Soft ones were removed in the same way as polypi of the nose, or their neck was ligatured by an ingenious instrument contrived for that purpose. He remarked that considering that the canal was narrow and curved, the latter methods were of doubtful value. It was necessary to use great caution in extraction; for if, as often happens, the polypus grows from the membrana tympani, this structure would be destroyed, the tympanic chain of bones might also be destroyed, and even death ensue by disease extending from the petrous portion of the temporal bone to the brain.

On Wednesday Mr. Bowman commenced the treatment of the case by slow torsion. He transfixed the extruded part by a needle, and then gave it a twist. He gave it another twist to-day. One effect on the tumour of this procedure was to render it livid and almost black, showing the immediate effect on the circulation of the mass. Mr. Bowman hoped that by this means he should cause ulceration of the neck of the tumour, and thus ensure its safe and complete removal.—*Med. Times and Gazette, Dec.* 3, 1859, *p*. 553.

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tory consequences of the operation have passed off), than any, if I may be allowed the expression, "mercurial" process?

MIDWIFERY;

AND THE DISEASES OF WOMEN, ETC.

106.—THREE CASES OF INDUCTION OF PREMATURE LABOUR PERFORMED AFTER COHEN'S METHOD.

By E. Noeggerath, M.D., New York.

[The first patient was a German lady, of short stature, and with rhachitic lower extremities. She had been delivered in her first confinement by craniotomy.]

The pelvis was a model of rhachitic deformity. The promontory of the sacrum protruding forward and towards the left side of the pelvic cavity, diminished the antero-posterior diameter to 2½—2¾ inches, while the lateral diameter remained unchanged in extent; the outlet of the small pelvis was rather enlarged in consequence of the widely open pubic arch and the flattening of the sacral curvature. The whole basin presented but a very small degree of inclination. The general state of health of the patient was satisfactory. On Monday, 2d June, about 11 o'clock in the morning, Dr. G. C. E. Weber and myself proceeded to perform the operation of inducing labour after the method of Schweighäuser, Cohen. The woman was placed upon her back with the nates projecting somewhat over the edge of the bed, and the feet supported by two chairs; an elastic catheter, of the ordinary size, was introduced into the mouth of the uterus, and pushed upwards, with the intention of bringing the instrument between the anterior wall of the uterus, and the fœtal membranes;—the point of it entered the womb to the extent of about four inches—then, with a syringe adjusted to it, we injected about seven ounces of water, heated to 90° or 100° Farenheit. As soon as the fluid touched the internal surface of the uterus, the woman complained of uneasy feeling in the abdomen, and we distinctly felt the uterus in a state of rigidity, which lasted for several minutes. After a time, the finger was removed from the external opening of the catheter, when a portion of the water was rejected through the instrument with considerable force. The withdrawal of the tube was followed by another escape of some water. During the following thirty minutes the uterus was in an almost continual state of contraction with but very few and short intermissions of flaccidity. Besides a slight degree of excitement and little headache, the woman's state of health, as well as her pulse, proved to be unchanged. Towards noon the pains grew stronger, but less in frequency, with longer intervals.

At about seven o'clock in the night, the pain lessened in a degree that we thought it proper to make another injection. This was applied in the same way, with the exception that we did not change the ordinary position of the patient in her bed, because the lips of the os uteri were already so much retracted by the previous pains, that the introduction of the catheter would meet with no difficulty at all. Whether the water was injected with a somewhat greater force than at the first time we cannot decide, but it all remained in the uterus, and the operation was followed by a sudden enlargement of the womb. Mrs. M. experienced a very distressing pain in her abdomen; much more so than she did at the former injections. It made such an impression upon her system that she fell into an almost unconscious state; the pulse sunk suddenly, so as to be scarcely perceptible; her face instantly became purple, and her breathing very much embarrassed. Half an hour later, when she recovered from these symptoms, she was seized with a violent chill, which lasted for nearly two hours. This was followed by a feverish condition, general heat, and a pulse of 130 in a minute. This alarming state gradually subsided, and a renewed succession of strong uterine contractions commenced. At seven o'clock a. m. of the following day, we were told that she endured almost incessant and very severe labour pains during the last night. At this time we found that the vaginal cervix had disappeared completely, the os uteri was opened to the size of a silver dollar, the well-filled bag protruded into the vagina with every recurring pain. Now we could ascertain, beyond question, a vertex presentation. At nine o'clock a.m. the os uteri dilated to its full extent, and the membraneous cyst broke while it was protruded almost to the external orifice. At that time, the vertex was just engaged in the entrance of the pelvis. Passing over the very interesting peculiarities of this cranial parturition, it will be sufficient to say, that it required a full hour of time to bring the head down through the brim of the small pelvis, notwithstanding those tremendous pains, which are only witnessed with rhachitic females. But when the greatest circumference of the cranium had passed the upper part of the pelvis, then one of these violent pains was sufficient to drive the head through the whole cavity, and at once out of the labia externa up to the shoulders. The entire parturition, from the time of the first injection, was achieved in less than twenty-four hours.

The child, though born in a weak condition, was soon brought to the most satisfactory state of breathing and crying. After the placenta was removed by the ordinary manipulations, the uterus proved to be well contracted. The mother's condition was satisfactory, and has continued favourable.

[In the second case the pelvis was markedly rhachitic, and the antero-posterior diameter of the brim rather more than three inches.]

She was placed across the bed, the feet being supported by two chairs. By gently pushing the fundus uteri backwards, the os tincæ was brought more fully in a direction corresponding with the axis of the pelvis, and an elastic English catheter with a metallic mandrin was introduced into the cavity of the womb, between its anterior wall and the membranes, as far as one and one-half inches, as it was impossible to push it any further without using considerable force. Through it about three ounces of warm water were injected and instantly expelled beneath the instrument. This was repeated with the same result. We now withdrew the catheter, and introduced it again in a somewhat different direction. By this manœuvre the catheter could be introduced considerably further upwards, and the full amount of the injected water was retained. Immediately after this, the uterus became hard and rigid, and the patient had to press downwards as if in labour pains, which lasted for about half a minute. The patient was now ordered to rise and walk about the room. No water was discharged. From half-past nine a.m., June 8th, when the first injection was made until half-past ten, a.m., she experienced four well-marked though feeble pains. From this time up to half-past nine, p.m., regular labour pains, increasing in strength and rapidity of succession were observed, and with almost every one of them a small quantity of water was discharged. Still their influence upon the os uteri was as yet very insignificant, being dilated to about the size of a two shilling piece. Towards midnight the pains grew very strong, and at about three, a.m., a large quantity of water was discharged with one forcing pain. Dr. Krackowizer saw the patient at four, a.m., and found the right scapula presenting (in front), head towards the left side; fœtal pulsations easily perceptible on the left side of the abdomen below the umbilicus, The patient was placed under chloroform, when the doctor turned the child by one foot and extracted it, except the head. The operation of turning was attended with some difficulties, and could not be performed as quickly as was desirable. The head itself proved too large to be extracted in the usual way, and consequently the forceps was applied, and thus the child was delivered. It proved to be still-born, and, although every effort was made to revive it life could not be restored. The mother did perfectly well and was up after the ninth day.

[We omit the third case—and one previously published—as these suffice to explain the nature of the operative procedure.]

This limited number of observations is, of course, insufficient for a final discussion with regard to the value of the method, but added to the statistics already known, they will throw additional light upon the operation, and tend to determine its true position. The first man who conceived the idea of inducing premature labour by injection of water into the uterus, was Dr. Tac. Fried. Schweighäuser, of Strassburg. In his excellent work, 'Das Gebären nach der beobachteten Natur,' &c., Strassburg and Leipzig, 1825, he recommends to throw a quantity of warm water into the womb for that purpose. But, as he never seems to have practised it, we must attribute the whole merit to Dr. H. M. Cohen, of Hamburg, who first introduced this proceeding into practice. He called the attention of the profession to this method in a thesis written in the year 1846. Since that time we have through the different medical journals, accounts of upwards of sixty cases in which Dr, Cohen's directions were imitated, all of which were very favourable to the operation.

Let us now endeavour to compare these results with those of other methods. It would be a waste of time, to discuss anew the value of puncturing the membranes. What accoucheur would not prefer a method by which the membranes remain intact, thus avoiding all the trouble, and all the danger, connected with a dry labour? And as to ergot? I think no unprejudiced accoucheur will now resort to this remedy, with a view of inducing premature confinement, partly on account of the uncertainty of its operation (one failure in every fifth case. Krause), and partly on account of its generally admitted poisonous influence upon the fœtus. This remedy has had its day, and it ought not to be mentioned only from a historical point of view. The dilatation of the os uteri, by compressed sponge (Kluge's method) has met with invincible obstacles in many cases. In some instances, a sufficient dilatation of the os uteri was effected, but no pains followed; cases of this kind have been reported by Hobeau, Jæsche, Jacoby, and Barnes, and altogether about eighteen cases are reported where other means had to be employed, as the action of the sponge proved to be insufficient. Moreover, the application of compressed sponge is tedious both to the patient and the accoucheur. The same may be said of Busch's instrumental dilatation, and the method of Hamilton and of Riecke. The plugging of the vagina with scraped linen (Schöller), or with an animal bladder (Hüter), or with the colpeurynter (Braun) are, doubtless, more safe than the methods just mentioned, but altogether not free from inconveniences. The best of these contrivances is Dr. Braun's caoutchouc bladder-plug. Still, some cases are reported were it was unable to produce pains. Its chief drawback is the irritation of the vagina, and lower sections of the uterus, in consequence of its application. Thus Professor Breit of Tübingen, has published a case of a woman who died from inflammation of the internal genital organs effected by the colpeurynter. But for controlling hemorrhage, and promoting labour in cases of placenta prævia, the bladder-plug will always remain the remedy par excellence. Scanzoni's methods of inducing premature confinement by irritation of the nipples, or by irritation of the vagina and uterus, with carbonic acid, have met already with a number of failures counterbalancing entirely the amount of success obtained by them. The methods of Drs. Simpson, Merrem, Lehmann, Krause, which are intended to effect labour, by the introduction of a sound or a catheter with immediate removal, or with a view of leaving the instrument in the uterus, seem to be simple and effectual remedies for this purpose. But very few cases are reported in which they failed. Dr. Baun's latest proposition seems to be invented for the sake only of making a new invention. He proposes to introduce a gut-string between the membranes and the inner surface of the uterus, the effect of which proceeding is certainly none other than that obtained by Krause's method. The use of galvanism (Radford, Simpson, Mikschik) is often very painful, not certain in its results, and tedious for the accoucheur, even should he happen to be in possession of an electro-galvanic apparatus.

Before entering upon a discussion of the position which ought to be assigned to the douche, we will briefly mention a case in which this remedy was used without making the least impression upon the pregnant uterus. The woman to whom we refer was received into the lying-in-hospital of Bonn, *enciente* with her first child, and at about seven months. She was of small rhachitic stature, and, although her spinal column was pretty straight, she measured not more than about four feet ten inches. Her pelvis was, therefore, not spacious and a pretty fair specimen of pelvis justo-minor, with an antero-posterior diameter of three and three-quarter inches. Under these circumstances, it was thought advisable not to let her go the full term, and the douche was selected for exciting labour pains in the thirty-sixth or thirty-seventh week of pregnancy. We had a large douche ascendante, which threw a powerful stream of water from a height of twelve feet. The basin on the top of it was filled with hot water (100° F.), and I directed the nozzle of the tube as near as possible towards the os tincæ. In this way the water was allowed to play against the lower segment of the womb twice a-day for fifteen minutes, and this application continued for a full month. This douche was applied at least fifty-six times, but in vain; not the slightest impression could be made upon the uterus, and not the least indication of uterine contraction could be obtained from beginning to end. She, therefore, was left alone till her full time, and as she had an exceedingly small child, and very strong pains, she was delivered even without the aid of the forceps.

But this is not the only instance of this kind. Dr. Krause, in his elaborate treatise on induction of premature labour, notices thirteen cases in which the douche was insufficient to effect labour (Scanzoni, three; Kowalsky, two; Michaelis, Grenser, Ziehl, Kilian, Ritgen, Goudoever, Dubois, Levy, each one), and not a few cases are mentioned where thirty to seventy applications were required to induce efficient pains (Diesterweg, Germann, Grenser, Arneth). We are in possession of accounts of about ninety-four cases in which the douche was used. Of this number, fourteen mothers sickened during the application of the douche—*i. e.*, one was taken with nausea, two with vomiting, three with hemorrhage, one with violent diarrhœa, two with vaginitis, two with metritis, three with fever; out of these ninety-three women, in whom the douche was applied, twelve died in childbed. This is a number unparalleled in the history of induction of premature labour. From eighty cases in which ergot was used, only three women died; from one hundred and thirty-five cases of induction of labour by tapping, eleven died; from ninety-six cases of intra-uterine injection, three died—viz., two from eclampsia, one from puerperal fever. Out of these eleven cases of death after application of the douche, six were owing to metritis. We are inclined to believe that some, if not all, of these metritides were caused by the douche itself. It is right to suppose that the congestion produced and constantly repeated by the act of throwing a full stream of warm water, as often as thirty or seventy times, in an interval of a few weeks, against the uterus, will at last become stationary, and pass through the different stages of an inflammatory process. It further appears from a perusal of the facts] stated above, that the douche cannot be relied upon when applied for the purpose of inducing premature labour; it proved insufficient in about every sixth case, and had to be exchanged for another method. The uncertainty of action combined with the unfavorable results to the life of the mother are objections which cannot be denied. With regard to the fate of the children, it must be remarked that a considerable number of them were born in a cross presentation, altogether a larger percentage than with the other methods, a fact easily explained by the influence of a concussion (with an upward tendency) of the lower uterine segment and its contents. But as it is well known that almost all children who are artificially delivered before the end of the seventh month are still-born, it will be readily understood, that everything that has a tendendy to produce malpositions does actually increase the per-centage of still-births. These considerations are modified when we examine the history of those cases, in which the stream of water, thrown from the douche, was directed so as to enter the os uteri. The manœuvre just mentioned has been recommended or executed by Kiwisch himself, by Arneth of Vienna, by Simon Thomas, by Trogher, by G. T. Elliot, A. K. Gardner, and many others. A glance at the results of the operation performed in this manner reveals a remarkable difference in the effects of the douche when applied in the usual manner. In most instances the result was striking, pains arising soon after the first application, while a few cases are recorded where death followed upon its administration. One instance of this kind is reported by Chiari, in which during the application of the douche, the patient was taken with convulsions, cyanosis dyspnœa, and died soon afterwards. A similar case is reported by Dr. Germann (see 'Monatscrift für Geburtsk.' xii., p. 193), who, after thirty-seven unsuccessful applications of the douche in the usual manner, introduced the mouthpiece of the chlysopompe into the os uteri one inch, and threw about one or one and a half ounces of cold water into the cavity of the womb. The patient perceived immediately afterwards a kind of tension and expansion of her abdomen. A few hours later, she had a most violen chill, and at once a series of the most forcible labour pains, of such a character that Dr. G. became alarmed for the patient's safety. The child was born, and twenty-four hours later the woman was a corpse.

It appears that the application of the douche, with the nozzle inserted into the mouth of the uterus, is a proceeding in many respects similar to that above described as Cohen's method, only less certain in its results, and, as it seems, more dangerous. We think that every reader of this article will conclude with us that the douche, used in the way first recommended by Kiwisch —*i. e.*, without introduction of the mouthpiece into the cavity of the neck, is a procedure, in the generality of cases, too slow, too uncertain, and, as it seems, too dangerous for both mother and child, to be relied upon, and it is now quite common to direct the stream of the douche into the uterus. But if labour is promptly induced in this way, this is mainly due to the water which entered by chance into the uterine cavity. It is really only by chance that the water passes between the uterine walls and the membranes, unless driven in by strong force; a proceeding too dangerous, as we have shown above, to be recommended. But if it is oar intention to throw a certain quantity of water into the uterus, why not choose a method which is better adapted to the purpose, far more prompt in its effect, more safe to mother and child, easier for the operator, and less troublesome to the patient? We, therefore, propose to abandon the douche entirely, except in certain cases hereafter to be named, and substitute for it the injection of warm water into the cavity of the womb, by means of a catheter and a common syringe. I am sure that every one who has once tested Dr. Cohen's method, will be struck with the gentleness and promptness of its action, and the simplicity of its execution. In most instances, only one or two injections were required, and the average duration of labour from the time of the first injection was two days; not one instance is known of its failure, while the prompt recovery of the mothers in childbed, with the exception of those few cases where death resulted from eclampsia, gives us the best guarantee of the harmlessness of this procedure. Moreover, the apparatus required consists of such simple means, that every country-practitioner, residing in the smallest village, is in possession of them; they consist of an elastic catheter, a common enema-syringe, and a few ounces of warm water. The performance of this simple operation requires only a sufficient knowledge of the female sexual organs in the state of gestation, its execution is fully detailed in the history of the cases at the head of this article, and the only precaution to be taken is, to inject the water not with violence and force, but gently and slowly. But we meet, from time to time, with such a disposition of the internal sexual organs, that the introduction of a catheter is absolutely impossible, whether from a firm closure of the os, or from a location of the vaginal portion, so that it is out of our reach, in an upward or backward direction. Under such circumstances, we have to resort to a preparatory treatment in order to change the condition of the lower uterine segment, a treatment which in many cases may prove sufficient to induce efficient labour pains.

Of all means which may be chosen for this purpose, the douche is no doubt best adapted to our purpose. In acting principally upon the lower circumference of the womb, it is apt to soften the parts, to open somewhat the os, and to bring the vaginal portion more in the direction of the pelvic axis. We will further remark that Cohen's method ought not to be resorted to when induction of labour is required in case of uterine hemorrhage, from whatever cause it may arise. In such cases, nothing can surpass the caoutchouc bladder-plug (Braun's colpeurynter), which, introduced empty and filled with ice water, at once controls the bleeding by the double action of cold and pressure, and is almost sure to induce efficient labour-pains by its mere presence in the vagina.—*Contributions to Midwifery, p.* 9.

107.—TURNING IN CASES OF CONTRACTED PELVIC BRIM.

By Robert Barnes, M.D., F.R.C.S., Physician to the Royal Maternity Charity, &c.

[Dr. Barnes does not venture so far as Dr. Tyler Smith, (who even now maintains the possibility of abolishing craniotomy in the case of living children,) though he believes there are resources in the obstetric art, safe to both mother and child, the relative merits of which, as compared with craniotomy, are as yet by no means sufficiently appreciated. He says :]

The question I propose to examine is this :

Ought we to acquiesce in the dogma, that where the forceps fails, craniotomy is our only and last resource?

The affirmative answer involves two objections of serious moment. First, it condemns a multitude of children yet unborn to certain destruction. Secondly, it arrests, in *limine*, all improvement, all progress in the obstetric art, as bearing upon labour obstructed through contraction of the pelvis.

Humanity and science, then, conspire in urging us to hesitate long, and to examine patiently, before yielding our assent to a decision so revolting.

The case against turning and in favour of craniotomy in contracted pelvis may be stated briefly in this way. It is estimated that rather more than one-third of the children are lost under turning where the pelvis is of the ordinary size. It is urged that a living child may pass through a pelvis, the conjugate diameter of which is 3·25″, with or without the forceps. Now, it must be conceded that the main object of delivering by turning is to save the child's life. But in the case of a pelvis having a conjugate diameter of 3·25″, the operation is unnecessary. Its application must therefore be limited to cases in which the pelvis measures less than 3·25″. And, it is asked, what is the chance of the birth of a living child when the violence of compression is added to the ordinary risk of turning? Are we justified, for the sake of the very slender hope of saving the child under such circumstances, in subjecting the mother to the dangers of contusion, of laceration, of inflammation, which it is but natural to anticipate will follow upon dragging the child by sheer force through a narrow pelvis.

Such is the argument, endorsed by great names, that has to be encountered. It required no ordinary sagacity, and no common courage, to detect the fallacy of the argument, and to rebel against the authority that supported it. It required moral courage to dispute what seemed a well-settled rule of practice ; it called for the courage of clinical enterprise to carry out a plan of treatment, the success of which was doubtful, the result of which might be disastrous. Under all this discouragement Professor Simpson revived the operation. If it be destined to resume a place in the obstetric art, this result will be mainly due to his sagacity and enterprise. But in according due praise to Dr. Simpson, the merit of the practitioners of former times must not be forgotten. The operation is not new, but revived. Dr. Simpson, presenting his papers on this subject in 1850 in a collected form, prefaced them with the following remark :—"Four additional sections or chapters were intended to be added—namely, one on the mechanism of the proposed mode of delivery as influencing the steps of the practice itself; a second, on the cases of obstructed labour chiefly adapted for delivery by turning; a third, on the best method of conducting the operation in these special cases; and a fourth, or last, upon the history of the practice in ancient and in modern times."

I am not aware that Dr. Simpson has yet realized his intention of writing the last or historical section. But the deficiency has been ably supplied by Dr. Charles West and Dr. Ramsbotham. Since obstetric doctrines, like other things, sometimes revolve in cycles, it is interesting to examine the circumstances under which the operation we are now considering arose, and the phases it has undergone. For some time after the value of turning in order to deliver in cases of transverse presentation had been demonstrated by Ambroise Paré, the operation grew into favour, and was resorted to under a great variety of circumstances. It would appear to have been the general resource where artificial delivery was indicated on account of obstruction or urgent complications. The reason was simply that, the forceps being unknown, and the other instruments in use being so rude and inefficient, the hand was, and perhaps still is, really the obstetrician's best instrument. To it he therefore trusted. Many acquired great skill in turning. The operation was resorted to in cases of obstructed labour. It was only after the introduction and vulgarization of the forceps that turning fell into disuse. And when the craniotomy instruments were perfected, for the purposes of lessening the head and extracting it by crochet or forceps, craniotomy presented a solution of the difficulty so tempting by its facility that turning came to be nearly, if not altogether discarded, The practice, however, was partially maintained in this country by Dr. Denman; in France by Baudelocque and Lachapelle; in Germany by Stein and Osiander the elder. But although it was still distinctly advocated in 1833 by Osiander the younger, it may be said that the application of turning as a method of delivery in contracted pelvis is either emphatically condemned in the schools, or holds at best a very doubtful place. Now we are "trying back." And it is time. I cannot help declaring that craniotomy is carried to a frightful excess. As holding out a hope, at least, of lessening that excess, it has become a matter of urgent moment to direct our most anxious and candid attention to the applications of turning.—*Lancet, March* 24, 1860, *p.* 291.

108.—CASE ILLUSTRATING THE VALUE OF TURNING IN LABOUR OBSTRUCTED THROUGH COARCTATION OF THE PELVIC BRIM.

By Robert Barnes, M.D., F.R.C.P., Physician to the Royal Maternity Charity.

On the 20th of July, 1857, at eleven a. m., my assistance was requested by a surgeon in a case of convulsions. The patient was nineteen years of age, and in labour with her first child. She had been twenty-four hours in labour. Convulsions set in very strongly about two hours before my arrival. She had been bled to eight ounces. Her breathing was semi-stertorous; she was semi-comatose; the pupils contracted; face, body, and limbs enormously distended with fluid. She had passed about six ounces of urine, which "set" almost in a jelly on boiling, it was so loaded with albumen. The os uteri was of the size of a half-crown; head presenting; contraction fair. When I saw her again, at three p.m., there was more dilatation of the os; she had had one fit since. The os was rigid. Another fit now occurred. I ascertained that the antero-posterior diameter was considerably contracted; that the head was large, and firmly ossified; and that the long forceps could not be applied. In addition to the bony contraction there was the further diminution of capacity from infiltration of the cellular tissue with serum. It was urgent to deliver. I did not think the case favourable for turning, and therefore lessened the head. Meconium had passed; and, although the condition of the patient did not admit of uterine auscultation, I thought it probable that the child was dead from the effects of blood-poisoning. The extraction of head by crotchet was tedious; the bones had to be much broken up. I am now disposed to think that the delivery might have been accelerated by turning after lessening the head. There was a fit almost immediately after delivery. The uterus contracted well; there was very little hemorrhage. I learned that the patient recovered well.

Second labor.—I had urged that, in the event of a second pregnancy, labour should be induced at seven or eight months. I heard, however, nothing more of the patient until the 31st of July in the following year. when she was again in labour at full time. I was then sent for on account of protracted labour. The head, it was represented, had been partially in the brim for seven hours without progress. The forehead was turned to pubes. I perforated at once, and delivered by crotchet. The woman recovered well.

Third labour.—Early last April, my renewed recommendations to have labour brought on prematurely having made an impression, Mrs. H—— came to me when pregnant. On the 1st of June, when it was calculated that she was quite seven months gone, I began the use of means for inducing labour. The os being closed, I inserted a sponge plug into the cervix, and ordered the uterine douche three times a day. On the sixth I removed the sponge, which had slipped out of the cervix. The os was now dilated to the size of a shilling, admitting of the head being felt. I now gave several doses of ergot at intervals until the 8th. The patient declared that the waters escaped freely towards evening; but at eleven p.m. I ruptured the membranes. The enlargement of the uterus was excessive, from the quantity of liquor amnii. This impeded free contraction. I applied a tight binder to support the uterus and promote the discharge of liquor amnii, which still impeded contraction. The head lay transversely, occiput to right ilium. Labour lingered until four a.m. of the 9th, when the os being sufficiently dilated to permit trial of forceps, I made the attempt, hoping that the smallness of the head might leave room for a pair with narrow blades; but the protrusion of promontory frustrated this attempt. I then gave chloroform, but not to a degree of surgical anæsthesia, and introduced my left hand to turn. I felt the cord pulsating. The right knee was seized, and turning effected by help of external pressure. The half-breech and shoulders passed with some difficulty; but to bring the head through the brim I had to use considerable force, so much that I hardly expected the child would be born alive. The head slipped through the narrow brim into the cavity of the pelvis with a sudden jerk. There was no difficulty at the outlet. The child, a male, was born asphyxiated; the heart was beating feebly; it made one or two inspiratory efforts. After five minutes' resort to the rotation-method, breathing was gradually established. Cold aspersions on the distribution of the respiratory nerve, and momentary dipping in warm water, completed recovery. The child did well. The placenta was extracted with some difficulty, as is not seldom the case in contracted pelvis. The mother recovered without a bad symptom. At the date of publication the child is thriving.

Commentary.—The preceding history needs but few remarks. The urgency of the mother's state in the two labours at term left no choice but to deliver by lessening the head. The narrowing of the pelvic brim precluded the use of the forceps. In both these labours the children were large, and the heads firmly ossified. An attempt to deliver by turning, instead of perforation, would, in my opinion, not have succeeded. The disproportion between head and pelvis was too great to hold out a reasonable hope of bringing into the world living children, and the state of the mother imperatively indicated resort to the method that was quickest and least severe. The question whether some little time might not have been saved by turning after lessening the head, may fairly be entertained. The result of the third labour is eminently satisfactory. But for the application of turning in conjunction with the induction of premature labour, this patient was doomed to conceive only to see her fruit perish in the act of birth. To have anticipated the period selected for inducing labour, so far as to secure a fœtus capable of passing through the narrow pelvis by the natural efforts, would have been to ascend beyond the probable limits of viability.

Here, then, is a case lying so clearly between the boundaries of the necessity for perforation, or of the applicability of the forceps, on the one hand, and the induction of labour at a stage when the birth of a viable child is improbable, on the other hand, as to place in a striking light the value of the operation of turning. Turning accomplished what no other proceeding could accomplish. It saved the life of the child, whilst it added nothing to the risk of the mother. The case is a complete illustration of Conservative Midwifery.—*Lancet, April* 7, 1860, *p.* 341.

109.—*Turning as a Substitute for the Forceps in cases of Narrow Pelvis.* By Robert Jones, Esq., Strefford.—In a series of papers published some years ago, Dr. Simpson advocated the operation of turning in labours connected with narrow pelves, in preference to the use of long forceps or craniotomy, provided it be had recourse to early, as being attended with less danger to the mother, and as affording a greater chance of saving the life of the child. He states that morbid contraction of the brim is, whatever mode of delivery be adopted, liable to cause rupture of the uterus, especially when the labour has been allowed to be protracted, and the compressed tissues of the cervix have consequently been rendered friable; and that, owing to the elasticity of the cranial bones, great force might be used in extracting the head, without endangering the child's life, &c.

The following case is related as corroborative of these views. A few years ago, early in May, Mrs. Cook requested me to attend her in her forthcoming confinement. She is a little spare Irishwoman, aged 38, the wife of a coachman. She had then nearly completed the eighth month of her eighth pregnancy. She informed me that her first four labours were tedious and painful; the children were small, but born alive, and were still living; that, in her next two confinements, she was attended by two different medical men, both living at Leintwardine: that in each the labour was painful and very protracted, and craniotomy was had recourse to in each. In her seventh confinement, which happened eighteen months ago, she was attended by a gentleman practising in Ludlow; this time the labour lasted thirty hours; it was left to nature, and the child was born dead.

I took this history as sufficient evidence of the existence of a morbidly contracted pelvis, and at once suggested the induction of premature labour. To this my patient stoutly objected. There was, therefore, nothing for it but to wait the natural time. Accordingly, early in the morning of the following 21st of June, I was sent for. I found she had been in labour all night, and for the four last hours the pains had been very powerful. The vagina was well lubricated, the sacrum very hollow, and the promontory greatly projecting forwards. The os uteri was well dilated, the membranes entire, and the child's head just entering the brim. With a trifling degree of force, I pushed back the head, carried my hand into the uterus, and brought down the feet without the least difficulty. Not so, however, with the head; for this required all or nearly all the strength I could use and keep up for some minutes. The head at length passed. The child was apparently dead; but the funis beat feebly, and at long intervals. Cold water being dashed gently on the face repeatedly, the child gasped, cried, and ultimately breathed well, and, I believe, is still living. The mother made a good recovery.—*British Med. Journal, Jan.* 14, 1860, *p.* 30.

110.—*Practical Remarks on Fœtal Auscultation.* By R. Druitt, Esq., Member of the Royal College of Physicians, London.—Of all the signs which distinguish the enlarged uterus from other tumours (though authors do not notice it), none is more valuable than the following:—The uterus, like other hollow viscera, has a regular peristaltic motion, continuous throughout pregnancy (and after delivery), and consisting in periodic contractions, which cause a moderate, but decided tension of the organ, and are followed by flaccidity and repose. No other tumour, not tympanitic, can do this; and during the fits of contraction the shape, dimensions, and outline, of the organ are unmistakeable. When about to auscultate, gently shampoo or roll the abdominal parietes over the womb, till you find it hard and resisting. That is the moment for auscultation. Be careful to put the stethoscope on the womb itself, and perpendicular to its surface thus isolated and defined; not merely in a vague way on the abdomen. Search carefully on the horizontal line on a level with the anterior superior spine of the ilium; beginning on the left side, then a little above and below; if unsuccessful go to the right side. Take care that your attitude is easy and produces no rushing noise in your ears; and if the woman be pregnant of a live child you will here within a limited space, the heart-beat as distinctly before birth, as you can after birth, and just like it.—*Med. Times and Gazette, Jan.* 21, 1860, *p.* 58.

111.—REMARKS ON THE EMPLOYMENT OF UTERINE PESSARIES, WITH THE DESCRIPTION OF A NEW INSTRUMENT.

By E. Noeggerath, M.D., New York.

In recommending the use of pessaries in the treatment of prolapsus uteri, I am far from resorting to it in every-day practice, viz., that of diagnosticating prolapsus uteri, and prescribing a pessary at once. Nay, there are cases which do not justify instrumental treatment at all, while almost every single case demands a preparatory treatment before a pessary can be applied. The necessity of a careful examination and a full consideration of the complication present cannot be urged too strongly. The neglect of this principle is the common source of failure in the treatment of prolapsus. For the same reason, no physician should prescribe a pessary on the sole assertion of the patient herself, that she suffers from falling of the womb. I have frequently met with patients, who believed themselves to be subject to this complaint, who, upon examination, were found to have metritis or malpositions and flexions of the womb. It is obvious, that a pessary in this class of cases, would be injurious instead of beneficial.

The patient must be examined as well in an erect as in a horizontal position, as it often happens that a prolapsus disappears when the patient is lying on her back. After the presence of prolapsus has been ascertained in this way, the patient must be subjected to a thorough examination, while in a horizontal position. It is best to begin with the palpitation of the abdomen, in order to get a knowledge of abnormities in the supra-pelvic and pelvic cavities. Hereafter the prolapsed portions themselves must be inspected, and the state of the anterior and posterior wall, and that of the womb itself, have to be taken into consideration.

Moreover, the colour and condition of the respective mucous membranes have to be taken into consideration, as well as the presence of ulcerations, their different character, their seat in the cervical canal, near the orifice, or on the walls of the vagina. Hereafter the prolapsed portions have to be touched all around with the fingers, in order to ascertain their condition, and the possibility of full or partial reduction. In order to get a full view of the position of the uterus, it is well to introduce one or two fingers into that portion of the vagina which is inside of the pelvis. By examining through the rectum, we may ascertain how far it is involved in the prolapsus. Hereafter the situation and size of the womb has to be ascertained with the probe, and that of the bladder with the catheter. After this the parts must be pushed upwards, in order to examine the sexual organs inside of the pelvis and the pelvis itself. In those cases, where the neck of the uterus is not in sight, it has to be explored with the speculum.

The different forms which a prolapsus may represent are as follows:

1. One of the walls of the vagina may prolapse, without participation of the womb, viz.:

(*a*) Prolapsus of the anterior wall of the vagina.

(*b*) Prolapsus of the posterior wall. These cases are generally recorded under the name of cystocele and rectocele vaginalis.

2. Prolapsus of one or both vaginal walls, with partial prolapsus of the womb.

(*a*) Prolapsus of the anter-wall of the vagina and partial prolapsus of the womb.

(*b*) Prolapsus of the posterior wall of the vagina and partial prolapsus of the womb.

(*c*) Prolapsus of both walls of the vagina and partial prolapsus of the womb.

The cases of prolapsus of the anterior wall and the uterus are very often connected with the retroversion and flexions of the womb. The body of the womb is generally turned somewhat backwards, pressing upon the os sacrum and rectum. These cases, therefore, are very often complicated with very troublesome constipations of the bowels.

3. Prolapsus of both vaginal walls and complete prolapsus of the womb. This variety is the most commonly met with, because women affected with the disease very often do not apply for medical advice until twenty or even forty years have passed since its first start.

4. Prolapsus of the uterus. This is of very rare occurrence. The inferior portion of the womb, generally hypertrophied in a great measure, protrudes between the labia majora as a thin cone, which sometimes attains the length of three or four inches. As its lower end is rounded off, and perforated by the orifice, it resembles the penis of the male.

In most cases of prolapsus the lining membrane is the seat of superficial or deeper ulceration. The ulcerations coincident with prolapsus must be divided into two different classes, viz., those which are the consequences of idiopathic uterine disease, and these which are the result of mechanical irritations. This distinction is important with regard to treatment. The ulcerations from a mechanical cause are limited by irregular, sharp, callous edges, and their base is discoloured with a brownish hue, yielding a dirty, thin, often very offensive secretion. The ulcerations from chronic metritis are of a more inflammatory character, inclined to bleeding, spreading rapidly on the slightest occasion, and very obstinate to treatment, unless the metritis has been subdued beforehand.

Other complications very often connected with prolapsus are *retroflexio*, *retroversio*, and *anteflexio*. Every complete prolapsus uteri is followed by *hypertrophy* of the organ, which attains in most cases the longitudinal axis, while at times the womb is considerably increased in thickness. In the first instances, the probe may be advanced into its cavity as far as five or seven inches. In other cases the cervical portion alone or one of the lips only are hypertrophied.

In consequence of the *displacement of the bladder*, always present in cases of prolapsus of the anterior wall of the vagina, the urethra is often covered with fungous vegetations, which at times attain the length of half an inch in diameter. *Hernea recti* and *prolapsus ani* are of comparatively rare occurrence, while *rüpture* of the *perineum* is not seldom. These and other complications have to be removed, as far as possible, before the application of a pessary can be thought of. The treatment of some is very tedious, and demands a good deal of patience from the attending physician and the woman herself.

Chronic metritis, hyperæmia and painfulness of the prolapsed parts, must be treated with leeches, scarifications, anodynes, resorbents, &c. The ulcerations have to be cured thoroughly before a permanent retention of the womb can be thought of. It is perfectly contradictory to experience, that the reposition of the parts into the vagina is sufficient for the cure of these ulcerations, an opinion cherished by some of our very first obstetric physicians. The only complication which requires no treatment before the application of a pessary is simple hypertrophy of the womb.

The most efficient remedies for treating these ulcerations are nitrate of silver, acideum pyrolignosum, scarifications, removal with the knife of the callous edges, fomentations with lead-water, slight cathartics.

The *ulcerations* of the *vaginal walls* are of a very intractable nature; they are never benefited by the application of caustics, such as nitrate of silver; scarifications repeated every third or fourth day, and the applications of acid—pyrolignosum, answer much better. They often require twelve or eighteen months' treatment before a sufficiently firm scar has been attained. The ulcerations seated in or near the cervical canal must be healed up (at least as far as they spread over the lips) before a pessary can be introduced, while the treatment of the intra-cervical ulcerations may be continued afterwards with the speculum. It must never be forgotten, that all ulcerations which are touched by the pessary will increase and make the use of an instrument impossible. Only in those exceptional cases, where the ulcerations resist the most rational and persevering treatment, they may be covered with a piece of soft and dry lint, and a pessary introduced afterwards, and treatment continued intra-vaginam. In those cases where bodily rest can be resorted to, it is of great value for the cure of ulcerations; at any rate in treating these affections, the greatest cleanliness must be observed, the parts must be thoroughly sponged after going to stool, and they must be covered always with a clean piece of dry linen.

The use of a pessary seems to be connected with the greatest difficulty in those patients where prolapsus is complicated with both hypertrophy and flexion of the womb.

In the very first days of its application violent back-ache, a sensation of bearing-down and prolapsus of one of the vaginal walls, make their appearance. When examined, the body of the retroflected uterus is found very painful, and ulcerations appear on different places.

In these cases it is a good plan to elevate the retroflected womb by the uterine sound, thus fixing it towards the promontory. Then a pessary may be introduced and absolute rest recommended for some time. If this is not sufficient, the only means left, is to introduce a soft sponge behind the cervical neck, which, in many cases, does retain the prolapsed womb in its position. The sponge has to be removed, cleansed and reintroduced daily for some weeks before another application of the pessary may be tried, which at first must be applied in connection with the sponge. By a strict and indefatigable adherence to these rules, a pessary is finally endured without any inconvenience.

After a full consideration and treatment of the different complications, it is of the greatest importance to choose the right kind of instrument.

The requisites of a good instrument are as follows: 1. It must retain the womb in or near its natural position. 2. It must neither irritate the womb nor the vagina. 3. It must not interfere with the patient's moving round, sitting, or excretion of urine and fæces. 4. It must be composed of a substance which resists the corrosive influence of the secretions from the genitals. 5. It must be constructed so as to be easily introduced, removed and cleaned by the patient herself. 6. It must be as cheap as possible.

The different pessaries may be divided into two sections, viz., those which support the womb directly, and those which support it indirectly by elevating the vagina. Until late years, only the former were exclusively applied, as this idea most naturally suggested itself at first sight. They are divided again into *stalked* and *unstalked.* Both are intended to give a direct support to the fallen uterus. Later researches seem to show that the chief and most natural support of the uterus was presented by the vagina, and in this view surgical operations as well as instruments were invented, and, as it seems, successfully applied for the cure of prolapsus.

The first man who clearly followed this indication in constructing his pessary, was Prof. Kilian, in 1846, and he called it elytromochlion—*i. e.*, vaginal supporter.

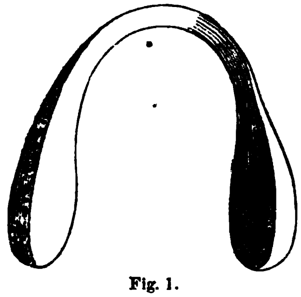


Fig. 1.

His instrument consisted of a thin, steel spring, four inches long, the points of which ended in wooden buttons, and the whole of it was covered with a thin layer of india-rubber. In introducing the instrument, the ends of it are approximated to each other as much as is required for its easy introduction into the vagina.

In applying it, it must be elevated in the direction of the lateral diameter of the vagina, while its convex portion is directed towards the anterior walls of the pelvis. The instrument thus bent is gently pushed upwards, so that its points take a position to the right and left side of the uterine neck, as high up as possible in the laquear vaginæ.

Although the instrument has been abandoned by the profession, owing to the fact that very few women can bear the pressure which it necessarily must exert, in order to sustain itself in the vagina, the elytromochlion of Kilian has been employed in some cases successfully, thus proving that the theory of its construction was based upon sound principles.

In 1853, Dr. Zwank, of Hamburg, published the description of his new *hysterophor*. It consists of two ovoid thin pieces of metal, covered with india-rubber, or of wood, connected on one end by a joint. In the neighbourhood of this joint, on the external surface of the wings, is a metallic pin, on each side two inches long, which can be screwed together at the lower end.

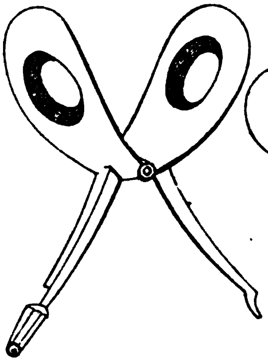


Fig. 2.

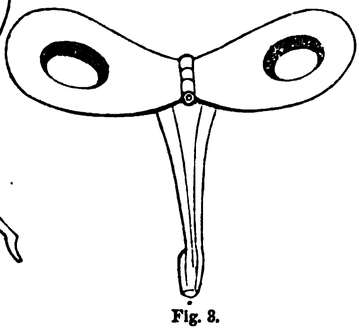


Fig. 3.

In applying the instrument, the wings are approached as much as possible (fig. 2), and introduced so that its convex portion is turned towards the os sacrum, and pushed upwards as high as possible, towards the anterior portion of the laquear vaginæ, in front of the neck of the uterus. Afterwards the lower ends of the metallic handles are compressed, and fastened by the screw (fig. 3). In this position the instrument is retained by itself.

About the same time, Dr. Schilling, of Munich, invented quite a similar instrument to that of Zwank; the only difference being, that the movement of the wings is effected, and can be regulated by the screw at its lower end. The purpose of both instruments is, to gently expand the lateral portions, and sustain the superior wall of the vagina, thus preventing its inversion, and consequently, the falling of the womb.

Dr. Zwank's instrument was received enthusiastically by the profession in Germany. Such men as C. Mayer, Bhiari, Braun, Scanzoni, Breslau, &c., thought it of sufficient importance, to publish their observations in favour of this instrument, and at the present time it has actually supplanted all of its kind.

What is the reason of this? Is it because the profession seized upon the instrument because it was a new invention? Is it because an instrument was wanted? or has it fulfilled what it claimed to do?

The question which we propose to consider is, whether this instrument has any advantage over others hitherto applied for the same purpose. It certainly has; because, 1. It is lighter. 2. It touches only a comparatively small circumference of the vagina, and scarcely any portion of the womb; thus preventing irritation and ulceration of the vagina, incarceration of the uterus, fluor albus, menorrhagia. 3. It can easily be introduced and removed, easily brought to its proper place, easily cleaned by the patient herself. This is a combination of advantages, sought for in vain among the best of previously invented pessaries. On the other hand, the hysterophors of Zwank and Schilling have some disadvantages, owing to the substance of which they are composed. The greatest number of them, as now in use, are covered with a coat of vulcanized india-rubber. The discharges of the vagina destroy it in a very short time. After this has been done, the metallic portions begin to rest and decay, thus irritating the vulva; the furrows of the screw at the lower end of the instrument begin to crust, or the screw, if turned too firmly, cannot be untwisted. Some patients have little dexterity, and do not know how to manage the screw at all. An illustration of these facts I am seeing daily, in the case of a lady belonging to the first class of society. She is the widow of a well-known physician of this city, and has suffered from prolapsus uteri ever since her first confinement, many years ago. The most thorough examination is unable to detect anything abnormal about her genital organs, except prolapsus uteri. She has been under the very best treatment of genital practitioners and uterine specialists. Everything has been resorted to, to effect a radical cure, and all kinds of pessaries used, but in vain. At length one of Zwank's pessaries was suggested. She has worn it now for a year, and is perfectly satisfied; the only drawback being the loss of the india-rubber coating, and the rusting of the metallic skeleton.

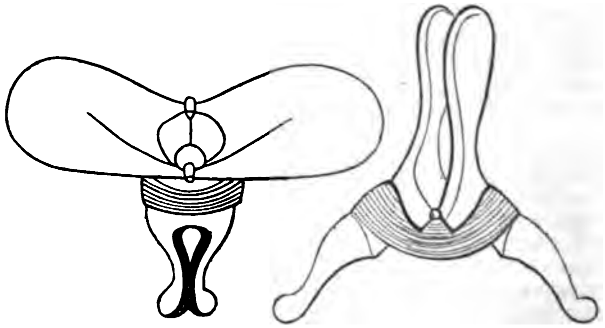


Fig. 4. Fig. 5.

In order to avoid these inconveniences, Dr. Eulenburg, of Coblenz, modified Dr. Zwank's pessary, and described his instrument in a short thesis, in 1857. It is made entirely of boxwood, and its wings are a little differently shaped, viz.: they are slightly curved downwards at both ends, so that the lower side forms a concave surface. In consequence of this shape, the lateral branches closely adapt themselves to the inner surface of the ramus descendens ossium pubis; thus presenting a kind of hook, which gives a strong hold to the instrument when in the vagina. Both wings move in the centre part by two joints, thus leaving a hole in the middle, through which the secretions of the vagina are allowed to escape. Instead of the screw, Dr. Eulenburg perfected the opening and shutting of the wings, by means of an elastic-india ring, which runs in a channel around the body of the hysterophor, immediately below the two joints.

By this contrivance, the introduction of the instrument is greatly simplified, and as it shuts on its own account, by the elasticity of the india-rubber ring, its application becomes very easy, thus requiring not the least ingenuity upon the patient's part (see figs. 4 and 5). As every particle of metal is avoided (except the small pin, running through the joint), and as the boxwood resists more than any other substance the corrosive influence of the vaginal discharges, it is lighter, will keep longer, and will cause less irritation than the other instruments.

The author found *four* different sizes, fitting to the greatest number of cases, viz.: for the measure from side to side, 2¾″, 3″, 3¼″, and 3½″, and correspondingly the largest antero-posterior diameter of every wing, 1″ 3‴, for the two largest sizes, and for the following, 1″ 4‴ and 1″ 5‴.

The first application of the instrument ought to be performed by the physician himself, who has to choose the size required for every case. His judgment will be conducted by the sensation of the patient, after walking to and fro for a while, and more so by the way in which the india-rubber ring contracts. If the extra-vaginal portion is not shut entirely, the instrument is too large, and has to be removed; if it shuts too quick, a larger one must be chosen. The following duties devolve upon the patient herself, viz., removing and cleaning it at bed-time, and re-adjusting it before getting up in the morning. This is performed by seizing the buttons at the lower end, and while separating them from each other, as much as possible, the other end of the instrument is to be gently introduced into the vagina, till it cannot go any further; and (when left alone) now it shuts on its own account. The same way is followed in its extraction. Before its introduction it ought to be well-oiled. In order to render this pessary even more harmless, it is advisable to cover its branches with a kind of glove, made of soft-deer skin, which coat may be moistened with cod-liver oil before every application.

Of great importance is the breadth and direction of the pubic arch, because this is the chief guide for the selection of a pessary. It can be ascertained by introducing the second and third finger behind the arcus and expand both fingers till each of them touches one side of the arcus. The distance of the fingers thus obtained may guide our judgment in the choice of an instrument. As a general rule it may be stated, that a comparatively small instrument ought to be tried first, because, it very often happens, that even the most extensive prolapsus is benefitted by small instruments.

After the instrument has been closed, the patient must be questioned as to what her sensations are. If the instrument was too large, a singular kind of smarting is perceived and considerable uneasiness expressed. It is a good plan to have the patient walk around, in order to ascertain if the prolapsus will be perfectly retained by the instrument.

Even in cases where the perineum has been ruptured, our instrument has been used with perfect success; the only precaution to be taken, is the choice of a broad pessary.

On the second day after the application of the instrument, the patient must be seen again by her attending physician, because at this time generally certain symptoms occur, which originate from the presence of a foreign body in the vagina, and which prove, *if they are very intense*, that the instrument is too large.

The symptoms alluded to, are a chilly sensation, heat, headache, trembling, nausea, want of appetite, obstinate constipation. The instrument must be removed, and the vagina must be examined with the speculum, to see if a portion of it is inflamed or ulcerated, a condition always met with if the instrument chosen was too large. After the third day is over, chills and heat are very trifling, and disappear entirely sometime afterwards.

If the instrument is borne after some days with no discomfort at all, the patient must be taught how to use it, and must repeat the manœuvre of adjusting and removing it several times in the presence of the physician.

At the time of the monthly courses, the patient had better have the instrument removed, provided she can keep quiet. At times it happens, that a portion of the anterior wall of the vagina falls beneath the pessary. In this case, a broader instrument must be chosen, or a small piece of plugged linen must be placed in the midst of the instrument, corresponding with the prolapsed portion, which is easily retained by this contrivance.

In recommending these instruments, and especially the latter one, to the consideration of the profession, I am sustained by the experience of our European brethren, who have used them with such general satisfaction, that scarcely any other form is now in use. Lately Dr. A. Mayer, of Berlin, has published a paper on the use of Zwank's pessary, wherein he reports to have successfully applied it in two hundred and thirty cases. For my own part, I avoid the use of pessaries as much as possible. But I have had under my care a number of cases, in which a pessary was the only means justifiable. I have tried a great variety of them, and have now come to the conclusion that Zwank's (or Eulenburg's) hysterophor answer better the requisites of a good pessary than any other.

I, therefore, ask practitioners to give it a fair trial. I do not mean to buy a hysterophor, and sell it to the next woman with prolapsus uteri, but after carefully selecting the case, in which nothing but a good pessary will give sufficient satisfaction, let the different sizes be tested, until the proper instrument is found.—*Contributions to Midwifery, p.* 38.

112.—ON VESICO-VAGINAL FISTULA—ILLUSTRATING A NEW MODE OF OPERATION.

By Isaac Baker Brown, Esq., F.R.C.S.

[At the commencement of his paper the author briefly relates several cases of this difficult lesion, six of which were successfully cured—making in all a sum of twenty-six cases—an amount of success greater than that published by any other surgeon. Dr. Brown then proceeds to offer the following practical observations and suggestions.]

I propose to speak—

I. Of the position of the patient during the operation.
II. Of the various modes of operating.
III. Of the causes of the lesion.
IV. Of the causes of the failure.
V. Of the after-treatment.

I. *Of the position of the patient during the operation.*—Three positions have been recommended: on the hands and knees, as advised by Dr. Bozeman; in the semi-prone position, as suggested by Simms; in the lithotomy position, as recommended by myself. I am still of opinion that this latter position is best, both for the patient whilst under the use of chloroform, and for the surgeon, who can sit down quietly before the patient during the operation; whereas, on the hands and knees the patient cannot take chloroform, and the surgeon must stand in a very awkward posture during a long and tedious operation, which becomes exceedingly fatiguing. Of the semi-prone position I have had no experience, nor have I yet seen the necessity for its adoption. It must still be recollected that, whilst advocating the lithotomy position, there will be found some cases where, from peculiarity of the position of the lesion or of the female organs themselves, it may be advisable not to administer chloroform, and where the opening can be better seen and reached on the hands and knees than in the lithotomy position.

II. *The modes of operating.*—Four classes or modes may be mentioned: (*a*) by simple sutures as recommended by Hayward; (*b*) by metallic sutures as practised by Gossett in 1834, and more fully placed before the profession by Simms of New York; (*c*) metallic sutures guarded by clamps, as recommended by Simms, or by button, as suggested by Bozeman; (*d*) a plan which I now desire to recommend, namely, metallic sutures with a separate bar clamp to each suture. The suture may be either of silver wire or galvanized iron wire, as prepared by Cocker Brothers, of Sheffield, under the direction and suggestion of my friend Dr. Aveling. The first three plans have been fully elaborated by the gentlemen whom I have mentioned, and do not require any further observations here; I will therefore only dwell, and that briefly on the fourth plan.

Having placed the patient in the lithotomy position, I commence paring the edges of the opening, using for this purpose the three knives represented in Fig. 1. As there has been some misapprehension about my claim as the originator of these knives, I may here state that they were designed by myself without communication with any person whatever, and pub l used and recommended long before my friend Dr. Bozeman came to this country. During one of the many discussions he and I had upon plastic surgery, I showed him these knives, as affording great facilities for denuding

Fig. 1.

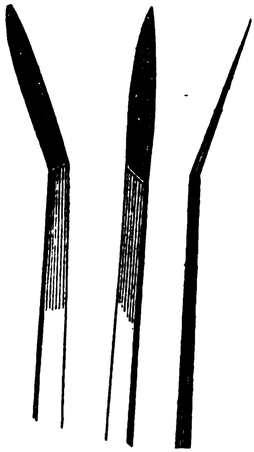


Fig. 3.

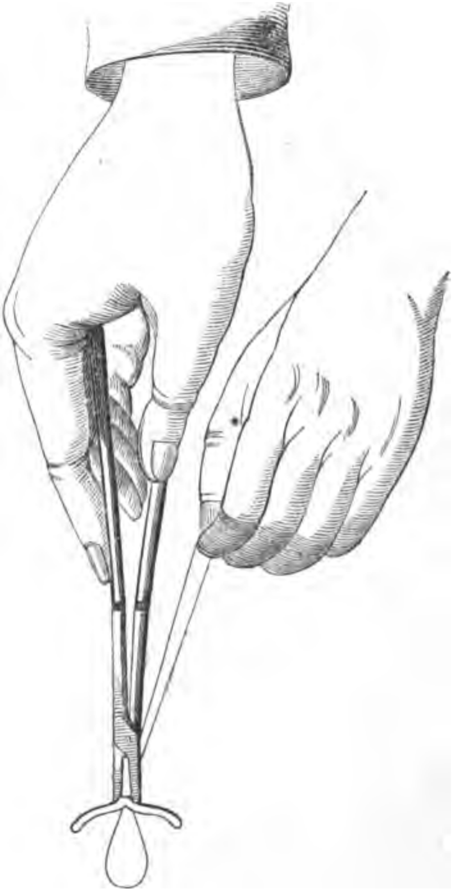
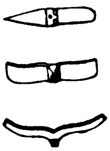


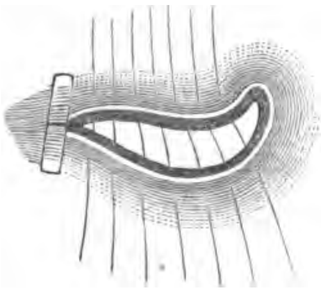
Fig. 2.



the edges of the fistula; when he, in his own quiet manner, opened his instrument-case and took out three knives precisely similar to mine, which were made in Montgomery (U. S.) from his own design, and under his own immediate eye. It will be thus seen that we both felt the necessity for, and had made, the same kind of instruments, without communication with each other.

The edges being pared, I now pass the needle, as practised by Simpson, and then through it either the silver or galvanized iron suture. Having thus passed as many sutures as may be necessary, I take the two ends of the first suture, and pass them through the eyelet-hole of the simple bar lamp (Fig. 2), which, it will be seen, is slightly curved, and having a nipple projecting from its back. I then hold the two ends of the wire with my left hand, and seizing the nipple of the clamp with a pair of long forceps, pass it back until I see the edges of the fistula are in perfect contact. I then firmly squeeze the nipple (Fig 3.) This has the effect of partially tsraightening the clamp and securing the wires (Fig. 4.) Each suture is treated in the same way until all are closed.

Fig. 4.



The advantages of this plan are manifold :—

1. Its celerity as compared with others.

2. The certainty that the edges are in perfect apposition all along the fistula.

3. That, however irregular the opening, you can follow its tortuosity without the slightest difficulty.

4. Should the opening be very large, the edges are kept perfectly together, whilst the vagina is not stretched out, as it would be under the large button. Each suture is also well guarded by the clamp, and there is no trouble in removing them. Simply the anterior side of each suture is cut, under the clamp, and then this, being seized by a pair of forceps, and slight traction made upon it, is easily withdrawn.

By this mode of operating, I have succeeded in completing the operation, in slight cases, within a quarter of an hour, and within three-quarters of an hour in the worst cases. Formerly, such cases took two or three hours; and I have heard Bozeman and Simpson say even a much longer time.

III. *The causes of the lesion.*—I here repeat that, in almost every case, this lesion has taken place from very protracted labour; and I cannot too strongly urge upon the profession to abolish the practice of waiting days and nights with the head pressing upon the bladder and soft parts generally.

IV. *Causes of failure after operation.*—It may arise from not carefully denuding the edges; from not introducing the needle deep enough through the tissues; from not being sufficiently careful in bringing together the edges of the fistula; from neglect in the position of the patient afterwards (this position should be upon the side); from neglecting to keep the bladder empty, either by the catheter being left in the bladder, or by its very frequent use. Occasionally the general health may be so much impaired as to predispose the patient to pyæmia, or to prevent healthy plastic matter being thrown out sufficiently soon to ensure union. It is necessary, therefore, to attend to the general health of the patient before operating. Another not unfrequent cause of failure arises from a certain indurated condition of the parts, which is induced by frequent operations, especially cauterization. The parts become almost cartilaginous, and are unable to throw out plastic matter. Such a case has come under my notice, where three fistulæ existed; two of these were easily cured, but the third has defied all attempts at present from this cause.

V. *The after-treatment.*—On the free use of opium and generous diet, and strict attention to the bladder, depend the success of the operation, however well performed in the first instance.—*Lancet, Dec.* 10, 1859, *p.* 581.

113.—A NEW PRINCIPLE OF TREATMENT, AND APPARATUS FOR VESICO-VAGINAL FISTULA.

By Robert Battey, M.D., Georgia, U. S.

[The principle of the treatment advocated by the writer is "direct pressure upon the approximated edges of the fistula," and the apparatus used to carry out this principle combines "both the compress and the splint;" and the author believes that by it cases of special difficulty can be treated with a certainty of success in the first operation, not hitnerto attainable.]

The combined compress and splint shown (Fig. 1), is made of sheet lead, of such thickness as to be flexible and easily adapted to the vaginal curve, and firm enough to retain the form given to it. It is prepared in strips of a convenient length, and usually five-sixteenths of an inch in width, or wider if required in any special case. At a distance, of say one-sixteenth of an inch, from either edge of the strip, faint lines are drawn and crossed at intervals of three-sixteenths of an inch by similar lines, running transversely. The bar is now perforated with holes upon the one side, and notched with a

Fig. 1.

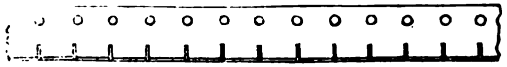


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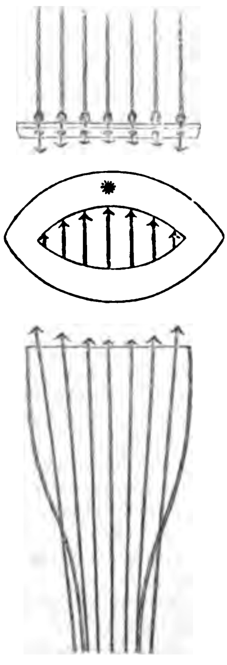
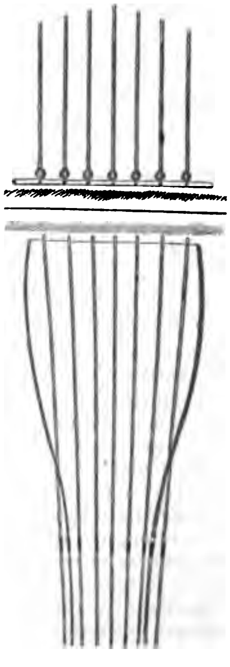
Fig. 2.



Half size. Shows the sutures in place; the splint, turned upon its edge, and the wooden spatula applied to the proximal lip of fistula.

Fig. 3.



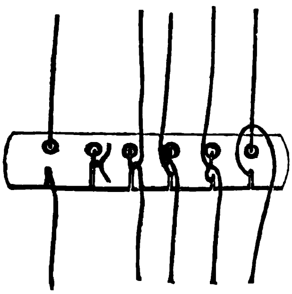
Half size. The opening has been closed by longitudinal traction, upon the wires, while counter-pressure is used by means of the wooded spatula.

very fine saw upon the other, as shown in the drawing. A piece of proper length is cut off, and smoothed with the knife, when required.

Operation.—The edges are pared, and the sutures introduced as heretofore, inserting the needle usually three-eighths of an inch from the pared surface. When the tension upon the tissues is very great, it is desirable to take deeper hold, and increase a little the width of the splint. It is always desirable to bring out the needle at the edge of the vesicle mucous membrane, including in the stitch as much as possible of connective tissue. As the wires are introduced they are passed through the holes in the splint, and secured by perforated shot, leaving an inch or more of free end, the splint being held up against the speculum, out of the way of the operator. Holding the splint in one hand, with the other tighten the wires, giving an equal tension to each, and draw down the splint upon the posterior lip of the fistula. If traction be now made upon the wires, it will be perceived that a remarkable power is had to drag down the vagina and uterus, giving easy access to the fistula; and also if a thin strip of wood, bevelled to a dull edge, be placed under the suture wires (see Fig. 2), and pressed upwards, an equally marked power for carrying up the anterior lip and closing the fistulous opening.

Fig. 3 exhibits the fistula closed and the splint turned up, giving a full view of the approximated edges, that the operator may assure himself that his work is as perfect as he desires. The splint is now turned down, and the wooden spatula confided to an assistant, who holds the parts in firm apposition while the operator secures the wires, as represented in Fig. 4. For this purpose a slender bit of wood, or the handle of an ordinary scalpel, with a small notch cut in the extremity, and a long pair of artery forceps, will be convenient and simple instruments. Placing the scalpel-handle under the wire, raise it into the notch in the splint, and push the wire on beyond the splint, as shown in suture No. 1 upon the right of the splint (Fig. 4); seize the distal end of the wire with the forceps, and bring it forwards around the loop and back again upon the other side, as shown in suture No. 2; still retaining the hold of the forceps, make traction upon both ends of the wire, and we have the two loops closely interlocked, as shown at suture No. 3; now seizing the wire at this point, clamp it firmly, make a secure twist (suture No. 4), and cutting off the free ends, turn down the twist as shown in the finished suture No. 5.

Fig. 4.



Full size.

The manipulation is more simple than the description, and is executed with ease and rapidity after a little practice upon a wire run into the tablecloth. If there be much tension upon the tissues, and difficulty be encountered in retaining the wire in the splint notch until the twist can be made, the notch should be closed upon the wire by pinching the edge of the splint with strong forceps. This trouble may usually be avoided by having the notches sawed so as to fit the wire closely, when the friction will hold it quite securely.

The advantages proposed to be gained by the new apparatus are—

1. The compress upon the approximated edges condenses the tissues, and, forming a water-tight joint, effectually prevents the leakage of urine, while no danger of strangulation is incurred; hence ensures the cure.

2. The compress directly hastens adhesive union, by bringing the surfaces with more certainty into firm and even contact.

3. The opportunity afforded of viewing the parts up to the point of final and satisfactory closure, is as complete as could be desired.

4. The power of the apparatus for bringing down the vagina, and for closing the opening, when there is great loss of substance and rigidity of tissues; and this with less danger of the wires cutting out.

5. The splint keeping the parts at rest, prevents the urine being drawn

through between the sutures by capillary force; and offers a smooth, even surface upon which the cicatrix is to be formed.

6. The adaptation of this apparatus to recto-vaginal fistula, ruptured perineum, and also to hare-lip, is too obvious to require special comment. —*Lancet*, *Dec*, 31, 1859, *p.* 665.

114.—ON THE SURGICAL TREATMENT OF OVARIAN DROPSY.

By J. Y. Simpson, M.D., F.R.S.E., Professor of Medicine and Midwifery in the University of Edinburgh.

[Respecting the position of the patient during the operation of paracentesis, Dr. Simpson commences by describing the position formerly adopted, viz., the patient sitting upright in a chair, with a sheet surrounding and supporting the abdomen, which was tightened at pleasure, by assistants standing behind the patient.]

I proposed and put in practice, several years ago, a new method of operating, in which the compress is entirely dispensed with, and the operation is rendered far more simple and easy at once for the patient, the practitioner, and the assistants. According to this plan the patient is not made to sit up at all, but is allowed to remain in bed in the horizontal posture; for she is less liable to syncope in the latter than in the former position; besides that she is by this means saved from all the fears and anxieties of what must always appear to her to be very formidable preparations. She is made simply to lie forward to the very front of the bed, and the protuberant abdomen is made to project to some extent beyond the edge of the bed. At the same time this part of the bed is, of course, to be protected by means of some folds of a sheet or towels. The trocar having been introduced into the cyst at the selected point, and with the usual precautions, the expulsion of the fluid is left to be effected at first by the elasticity of the abdominal walls, and afterwards by the force of the atmospheric pressure on the abdominal surface. As the evacuation goes on slowly and steadily, you can usually perceive the outline of the secondary cysts—if such are present—gradually becoming more and more prominent; and in most cases we can perceive an elevated line passing from the anterior superior spine of the ilium towards the linea alba a little below the umbilicus, while on either side of this ridge the depression is usually most distinct. As the discharge proceeds, the cysts and abdominal wall covering it, become steadily and uniformly compressed and flattened, far more steadily and uniformly under atmospheric pressure than under the pressure of any kind of mechanical binder. When the cyst is nearly emptied, the evacuation of the last remaining portion of the fluid may be favoured by having the patient turned somewhat more prone, and sometimes perhaps—but not often—gently pressing the sac with the hands. But let me advise you to be careful not to manipulate ovarian cysts too much during their evacuation, lest in incautiously raising the hand after exerting some degree of compression, you give an opportunity for the atmospheric air to be sucked up through the canula into the interior of the cyst; for inflammation of the lining membrane of the cyst is liable to be set up as a consequence of its entrance. After the cyst has been as entirely evacuated as possible, place your finger on the orifice of the canula and cautiously withdraw the tube; the abdominal parietes which it traverses being compressed by the fingers and thumb as the instrument is withdrawn through them. A pad of lint is adapted over the wound, and strapped down by strips of adhesive plaster. These strips should be about an inch in breadth, but two or two and a half feet long; for when you use them of this length—placing them across the abdomen of the patient—they form at the same time the simplest and most efficient belt or bandage that you can apply, in order to effect some degree of compression, and thus prevent all possibility of the occurrence of syncope. I used at one time to place a binder around the patient after completing a tapping in this way; but the application of it causes her some degree of annoyance, and I am not aware that it fulfils any indication that may not be better and more simply answered by the long pieces of strapping of which I have spoken. They are in reality a series of small binders, more easy in their adaptation, and as efficacious in their action as the larger linen or flannel bandage.

Instrument required for the Operation.—I have just a sentence or two more to add in regard to the kind of instrument with which the operation of tapping is to be performed, before I pass to the consideration of the other surgical means of treating cystic tumours of the ovary. Where the operation has already been repeatedly performed, and where you have reason to believe the surface of the cyst to have become adherent to the abdominal parietes, a perforation may be made with a lancet or bistoury at once through the abdominal wall and the wall of the cyst; and through this opening the fluid may be allowed to drain away. But generally you will make use of the ordinary trocar and canula for the evacuation of ovarian cysts, and in regard to that instrument only two remarks occur to me as worthy of your special attention :—*First.* See that the trocar be sharp; for a blunt-pointed instrument causes the patient a degree of pain which may be almost entirely avoided by using an instrument in proper order, and which should be mitigated to a certain degree even in the use of a blunt one, by your making a preliminary incision through the skin with a lancet or bistoury. *Secondly.* Take care that the silver tube, or canula, be not too sharp at the point; for when this is the case, it may happen that the lining membrane of the cyst will be injured, and even a blood-vessel wounded; and hemorrhage into the interior of a newly-evacuated ovarian sac is a most untoward complication, and destroys in no small measure the patient's chances of recovery. I told you a minute ago that the entrance of air into the interior of the sac during its evacuation, was an accident which we ought to do all in our power to avoid and avert. In tapping the cavity of the pleura in cases of empyema or pleuritic effusions of any kind, the danger of allowing air to regurgitate into the space from which the fluid is escaping, is even greater than in abdominal tappings; and a variety of contrivances have been adopted with a view to prevent it. Canulæ with valves, or with stopcocks, or with a piece of soft tubing, or a fold of softened bladder or leather, attached to the extremity, which would allow the fluid to pass outwards, but which collapsed under atmospheric pressure when the fluid ceased to flow; and variously modified instruments have also been used with the view of preventing the ingress of air into the cavity which is being emptied. Perhaps the best kind of instrument that you can use for this purpose, is one first described by Mr. Charles Thompson, in the 'Medical Times and Gazette' of March 27, 1858. (See 'Retrospect,' vol. xxxvii., p. 138). It consists of a canula, to which the trocar fits tightly, like a piston, and which has a small projection at the outer or lower part. To this projection an elastic tube can be fastened with the view of carrying the fluid into a large tub or basin placed at the bedside; and if this tube be filled with water, and the end of it be immersed in that or any other liquid, it acts as a syphon after the trocar is drawn back, and promotes the more effectual evacuation of the cyst. See

Fig. 1.

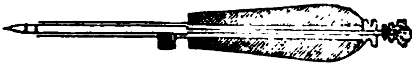
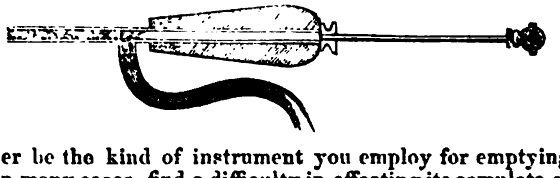


Fig. 2.



Whatever be the kind of instrument you employ for emptying the cyst, you will, in many cases, find a difficulty in effecting its complete evacuation, which you cannot avert by any modification in the form of canula that you employ. This difficulty arises from the presence of small semi-solid masses which occur in greater or less abundance in many dropsical ovarian sacs, and which are apt to get into the tube, and plug it up, after the greater portion of the fluid has escaped, so that the removal of the latter portion of the fluid becomes obstructed. These occluding masses are sometimes formed of lymphy exudations or other products of an inflammatory process. But they are occasionally found where no inflammatory attack seemed ever to have occurred, and in such cases they consist of the *debris* of the broken-down septa that once divided a single large cyst into several small ones, or of albuminous matters in a state of partial consolidation, or of blood-clots. Whenever, therefore, you go to tap an ovarian cyst, you ought to have some instrument by you with which you can free the canula of such obstructing masses, either by pushing them back into the cavity of the cyst, or, better still, pulling them outwards, so as to allow of the free escape of the remaining fluid. There is no instrument that you can use better adapted for this purpose than the ordinary uterine sound, which I should, therefore, advise you to take with you to every case of ovarian tapping. Besides making use of it for clearing the occluded canula, you can explore with it the interior of the cyst by passing it through the tube; and by this means you may gain some information as to the extent of the cavity, the number and size of the secondary cysts, and the presence of calcareous and other matters in the interior. For many years I was in the habit of using the uterine sound in this manner; and I have sometimes passed it between the surface of the tumour and the abdominal parietes, with the view of discovering the presence and extent of any existing adhesions. But I have seen dangerous symptoms result from the probing of dropsical cysts of the ovary, and I would advise you, therefore, to abstain from having recourse to it, except in cases where it is a matter of primary importance to gain certain information as to the condition of the interior; and even then, before having recourse to it, it would be well for you to pause and consider whether the information you are likely to receive by the process be of such value as to warrant you in subjecting the patient to the risks of inflammation attendant on the exploration. There is sometimes a farther source of difficulty in the complete evacuation of ovarian cysts, when a number of secondary cysts of large size are present; and in such cases the primary sac can only be emptied by making a perforation in the walls of the minor sacs, so as to allow them to collapse, and to be emptied of their contents also. This perforation you will be best able to make by means of a long sharp-pointed probe, such as the strong stylet of a large elastic catheter sharpened at the point. Passing this through the canula, and pressing the point of it steadily against the septum at its most yielding point, the instrument may easily be made to perforate it; and, if the contents of the subsidiary cyst be not too viscid, they will escape through the punctured orifice into the large cavity, whence they will escape through the canula. There is one great danger attendant on this proceeding, and that is the risk of injuring one of the blood-vessels that may be ramifying in the septum; and you must therefore avoid it as much as possible.

[With regard to the cases requiring the operation of injection of iodine, the writer observes:]

The occurrence of inflammation in the peritoneal coverings of ovarian growths, and the formation of extensive adhesions between them and the adjacent surfaces, often form insuperable barriers to the operation of ovariotomy, but present no hindrances to the injection of iodine into the cavity of the cyst. But, on the other hand, I have in most cases been particularly careful to avoid injecting iodine into an ovarian cyst when I have noticed the contents to be mixed up with inflammatory products, although I have done so in a few cases where the application of the irritant to the chronically-inflamed surface seemed likely to produce no violent effect, but only a slightly alterative action; and I think it is always a safe rule to follow, not to have recourse to injections of any kind into an acutely-inflamed ovarian cyst. For if the dangers of tapping are tenfold greater in cases where some degree of inflammation has been lighted up in the cyst previous to the operation, the risks, it appears to me, will be only aggravated by the application of any irritating substance to the already morbidly-disposed surface. Again, in cases where you have any great degree of difficulty in effecting a full and complete evacuation of a cyst from the co-mixture of a multitude of gelatinous semi-solid masses with its fluid contents, the injection of iodine is apt to be attended with this drawback—that it produces coagulation of the albuminous matters contained in the cyst, and thus forms a number of clotted masses, which cannot escape through the canula, and are not likely soon to be absorbed; but which are very likely to act the part of foreign bodies and lead to inflammation of the lining membrane. Probably some little chemical inquiry will betimes enable the obstetric practitioner to overcome this difficulty. Out of the body, the addition of a small quantity of aqua potassæ to the gelatinous fluid drawn from an ovarian sac will occasionally, if not always, make this fluid freely and perfectly mix with tincture of iodine; while before the addition of the alkali the iodine only mixed in points and patches with the ovarian fluid, and if injected by itself into a sac with these contents would have consequently been most inadequately and imperfectly applied to the lining membrane, provided, at any part. any of the secretion of the cyst remained. With the exceptions which I have stated, I believe the injections of iodine into the interior of evacuated ovarian cysts may, as a rule, be had recourse to with safety, with retardation of the disease in many cases and with permanent success and cure in some. The hope of benefit and of cure will vary with the character of the disease which is treated. In cases of multilocular cystic tumours, you can hardly hope by means of one single injection to produce a complete and lasting cure, for then you only empty and act upon the lining membrane of one out of a multitude of cysts, or at most of several cysts in communication with that one; and the utmost you can then usually expect is, that the chief cyst, or cysts, of the group will by this means become obliterated, and the progress of the disease be thus stayed for a time. In some rare cases, perhaps, the irritant injected into the cavity of one cyst may have the happy effect of inducing a certain amount of absorption in the walls of those around, so that in this way some of these other cysts may come also to be in some degree diminished. But I greatly doubt whether the iodine injected into one cyst ever has the effect of leading to the complete obliteration of the neighbouring sacs, unless it reaches their cavities; and this can only be done, in most cases, by having recourse to the separate tapping and injection of each particular cyst. Boinet places great faith in the treatment of multilocular ovarian tumours by injecting cyst after cyst, and there can be no doubt that a succession of cysts may be in this way cured by repeated injections. The cases, however, for which iodine injections seem to be best adapted are those where the tumour is unilocular, where the single cyst has been already tapped once or oftener, and still continues free from all traces of inflammatory action. In all such cases I believe iodine may be injected with the greatest possible degree of safety to the patient, and with the most hopeful prospects of a full and final cure. By means of iodine injections I have, as it appears to me, freed a number of patients from a disease which, if uninterfered with, or interfered with only in the way of a palliative paracentesis, must sooner or later have carried them off; and so far as I am able to judge, the cure has been complete and is likely to be permanent. Such seems to have been the case, for example with a young woman who called at my house, along with a relative, the other day, and who was one of the earliest patients that I injected with tincture of iodine. She was tapped for the first time eight years ago, because of an ovarian tumour which had been growing for about a year before, and had attained such great size as to interfere with her breathing, and otherwise to render her uncomfortable. The fluid drawn off was perfectly clear; and in about six months it had grown as large as before, but then seemed to remain stationary. Six years ago, exactly two years after the first tapping, she was again tapped, and this time a quantity of tincture of iodine was thrown into the cyst. She had a slight degree of pain afterwards, which was subdued by a one-grain opium pill; and remained in bed for a fortnight. From that time to this she has suffered from no untoward symptom, and at present it is altogether impossible to feel any tumour whatever in the pelvis, either through the abdominal parietes or through the vagina, although when I saw her two years ago I could still distinguish an abnormal body there; and the contracted and atrophied cyst must doubtless still remain. How, then, is this operation to be accomplished?

Mode of Operation and Apparatus Required.—When you go to inject iodine into an ovarian cyst in any case where you have determined on having recourse to this operation, you require to take with you—1st, An ordinary trocar and canula, with which to puncture and evacuate the cyst; 2nd, A long glass tube. with a nozzle, capable of being adapted to; 3rd, A gum-elastic male catheter, with the opening at the end of the tube; 4th, A uterine sound; 5th, Ten or a dozen ounces of tincture of iodine, or of the liquor iodinei compositus; 6th, a piece of lint, and some long strips of sticking-plaster.

In performing the operation the patient is placed on her side at the front of the bed, as I have already described to you at some length, and the full and complete evacuation of the cyst is to be effected in the same manner, and with precisely the same precautions, as in an ordinary simple tapping. Having emptied the cyst, you place your finger upon the orifice of the canula, to prevent the possibility of the ingress of air, while an assistant fills the injecting apparatus with the fluid you have determined upon using. Perhaps the simplest and most convenient form of injecting apparatus is that which I now show you. (See Fig. 3.) It consists of a long black tube (A), capable of holding eight or ten ounces of fluid, fitted into a pointed wooden box or nozzle (B). With this instrument you can effect your purpose perfectly well, placing the palm of the hand on the wide mouth of the tube, to prevent the action of the atmospheric pressure when you wish to restrain the flow of liquid from the lower orifice, and removing the hand when you are prepared to allow the liquid to run out into the ovarian sac. But the use of such an instrument is attended with some inconveniences, that may be obviated by introducing between the nozzle and the long tube that is passed into the interior of the cyst, an inch or two of india-rubber tubing (CC), which you can close at will, by means of a simple spring (D). So long as the spring is allowed to remain closed no fluid can escape from the tube; but the moment its hold is relaxed by the ends being compressed, the weight of the atmosphere suffices to drive out the fluid with the due degree of force; and should the flow appear to be interrupted, or should it cease altogether, from obstruction in the catheter, or some other cause, you can usually force it on easily enough by blowing into the tube with a moderate amount of force. To carry the fluid into the interior of the cyst you make use of the long elastic catheter, to which I have already referred. This is passed through the canula into the interior of the cavity; and before commencing the operation you must have made sure that the catheter and canula are of such repective dimensions that the former can be passed easily through the tube of the latter when it becomes necessary to inject. Before introducing the catheter into the cavity of the cyst, you must attach it to the point of the apparatus I have just described; and you must be careful to have it filled with the fluid by relaxing for a second or two the pressure of the spring on the intervening inch of india-rubber tubing. If you neglect this precaution, and introduce the catheter into the cyst before attaching it to the instrument and without filling it with the iodine, you can easily see that the liquid, ere it can enter the cavity of the cyst, must first drive before

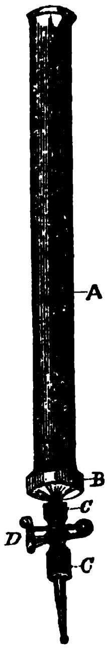


FIG. 3.—Apparatus used for injecting fluids into the interior of evacuated ovarian cysts.

it the air that is contained in the catheter, and the operation becomes unnecessarily and unfortunately complicated by the presence of the air in the newly-formed cyst. While the injection of the liquid is going on, the patient is still lying on her side; but after enough has been thrown in, the catheter is removed, the finger is again placed upon the orifice of the canula, and the patient is made to turn on her back, and perhaps in other directions also, while the tumour is manipulated and kneaded with the free hand, with the view of insuring a more effectual application of the irritant to the whole surface of the lining membrane of the sac. When this has been done, the patient is again turned on her side, and the fluid is allowed to run out. Its escape may even sometimes be hastened by pressure cautiously applied; for your object in injecting any preparation of iodine into the interior of an ovarian cyst is not to leave it there as a lasting irritant, but simply to paint all over its inner surface with the view of changing the secreting action of its lining membrane, and of setting up there the adhesive stage of inflammatory effusion. The injected fluid, then, having been all, as far as possible, again withdrawn, you proceed carefully to remove the canula, and afterwards to apply a pad of lint, and three or four straps of adhesive plaster, such as we saw to be necessary after an ordinary tapping. Such is the simplest, safest, and most satisfactory process that I know for introducing irritant injections into the interior of evacuated cysts of the ovary.

But there are still two questions in connexion with the operation that you will be ready to ask me, and which I must endeavour briefly to answer. What kind of fluid, or rather what preparation of iodine is best adapted for ovarian injections? and, What amount of fluid may or ought at one time to be used?

Quality and Quantity of Iodine Required.—In reply to the former of these questions, the quality of the iodine solution, allow me to say, that the preparation which I have most frequently made use of for the cure of ovarian dropsy, is the ordinary tincture of iodine of the Edinburgh Pharmacopœia. This, as you are aware, is a very strong preparation, containing half-a-drachm of iodine in every ounce of rectified spirit. Fearing lest the alcohol in this preparation might produce its peculiarly intoxicating effect on the system, in some cases where there was a likelihood of the fluid being to some extent retained in the cavity, I have used the liquor iodinei compositions of the Edinburgh Pharmacopœia instead. This is a much less potent preparation, containing only eight grains of iodine in every ounce of the fluid, along with half-a-drachm of iodide of potassium. It has proved effectual, however, in a number of instances; and may be safely employed where you wish particularly to avoid all chance of the narcotic effects of the spiritous preparation. I have never seen these effects produced to any very alarming extent, even in cases where a large quantity of tincture of iodine has been used. At the worst the patient is incoherent, or lies in a drunken stupor for an hour or two, but she soon awakes from this condition as the exhalation and elimination of the poison goes on simultaneously from the lungs, the kidneys, and the skin. The alcohol is speedily dispelled; but for some days afterwards the odour of the iodine can still be perceived in the patient's pulmonary and cutaneous exhalations. You can also trace it chemically in the urine. As for the quantity of iodine to be used, that will, of course, vary according to the special case. When the cyst is of large dimensions, you can hardly hope to bring the irritant in contact with the whole of its lining membrane unless a considerable quantity be introduced. From eight to twelve ounces may be required for the larger class of cysts; and I have sometimes injected as much as fourteen ounces at once. For smaller cysts, from four to eight ounces may suffice; but there are probably very few cases where you will succeed in producing any appreciable effect by injecting less than six ounces. The quantity of fluid injected can easily be regulated either by having the glass tube graduated to ounces, or by keeping it in a bottle correspondingly marked and measured off.

There is still one point connected with the iodine treatment of ovarian dropsy, in regard to which you must permit me to make one or two remarks before I have done with this subject. I mean

The Mode in which Irritant Injections effect a Cure.—This is a subject that still requires investigation; and to the question, How does the injection of iodine cure ovarian dropsy? different authors have given different replies. Some say that it produces its effect by acting on the walls of the cyst in such a way as to destroy their power of secreting the fluid they habitually poured forth. Others, again, aver that iodine only cures by exciting such a degree of inflammation as leads to the complete obliteration of the cyst and the adhesion of the collapsed and contracted walls. Whatever views have been held, seem to have been adopted merely as a matter of opinion. There is no observation on record, so far as I know, to show what actually happens in cases where iodine has produced a cure. My own impression is, that the usual effect of the injection of iodine is complete obliteration of the injected sac by adhesive inflammation. I ground this opinion, *first*, On the analogous effects of the injection of iodine for the cure of hydrocele: and *secondly*, On the direct observation I had an opportunity of making in the case of a patient who had at one time been subjected to the injection of iodine for the cure of ovarian dropsy. The patient I refer to came to me, many years ago, suffering from a multilocular ovarian tumour, for which she had been tapped several times and in rapid succession, without any better result than the most temporary relief. I injected the most prominent cyst with tincture of iodine. She recovered well, and went home much reduced in size, and showing no appearance of any new development in the tumour. Long afterwards she returned to Edinburgh, wishing to be again operated on as before, as some of the secondary cysts had, after a time, taken on a more active growth, and the size of the tumour began to inconvenience her. She was fevered, however, by the fatigue of her journey, became rapidly exhausted, and died without anything being done surgically to the local disease. On making a post-mortem examination, we found the new cyst inflamed, and covered on the inner surface with patches of lymph; and we could see besides what we supposed to be the old original cyst obliterated and filled up with plastic lymph, which was of a dark greenish colour, probably from the permanence of some of the iodine.

[On the subject of treating ovarian disease by the application of *electricity or galvanism*, Dr. Simpson observes:]

This means of treatment consists in causing a current of electricity, or galvanism, through the tumour, either by the application of the handles of the battery to the surface of the body or through the vagina; or by introducing, first of all, a number of acupuncture needles into the interior of the cyst, and causing the current to pass through them directly into the fluid. The effect, according to Jobert, would be the coagulation of the fluid, which, he avers, should take place in six or seven applications, and without determining any inflammatory action.

I have tried the effects of galvanism in many cases of ovarian dropsy, applying both through acupuncture needles, and simply to the surface; but the effect has usually been very slight, and I certainly never found any good result from it. On the contrary, in one patient in whom I made use of it, introducing some acupuncture needles into the interior of the cyst, and thus passing the galvanic current directly through the fluid, such a degree of inflammation was lighted up in the interior of the cyst, that she died in consequence of it.

[It is certainly only in rare and exceptional instances that *ovariotomy* is a justifiable operation. But the question is—In what instances?]

First, It is never to be dreamt of under any circumstances or in any case where the patient is at the same time suffering under organic disease or derangment of the kidneys, lungs, heart, or any other organ; never, in short, unless the constitution of the patient is in other respects healthy and favourable. *Secondly*, It is never to be thought of in any cases of cystic disease of the ovary, when that disease is mixed up, as it sometimes is, with carcinomatous or encephaloid deposits in the tumour. The co-existence, or the fear of the co-existence, of cancer with the cystic ovarian degeneration. is a complete barrier to the idea of ovariotomy. *Thirdly*, It is not to be had recourse to—at all events never in the first instance—in cases where the disease consists of a single ovarian cyst, or of a mass of ovarian cysts, with one cyst of such a preponderating and predominant size, as to make the diseased mass resemble a unilocular dropsy of the ovary. *Fourthly*, It is not to be had recourse to in these cases, because we have good hopes of often controlling and sometimes curing the disease by milder and safer measures; as by 1, paracentesis, and allowing the fluid, if bland, to escape into the peritoneal cavity; 2, by tapping through the roof of the vagina, and, if necessary, establishing a fistula between the cyst and the vaginal canal; or 3, by obliterating the cyst or cysts by the injection of iodine. The only cases of ovarian dropsy fit for ovariotomy, are cases of the common compound or multilocular form of the disease, in which these preceding measures have failed to restrain the onward growth and progress of the disease towards the inevitable destruction and death of the patient. But all cases of compound or multilocular ovarian disease do not belong to this category. In a few, Nature. as we have seen, herself taps and relieves the patient from time to time. With these cases you would assuredly not intermeddle. In a few, the course of the malady is very slow, and seems occasionally—but very, very rarely—to become as it were arrested in its march. With these cases you would not interfere in the way of any surgical treatment whatever. In some instances of multilocular ovarian dropsy the fistulous tapping of the largest and predominating cysts, or the injection of them with iodine, stays or retards, and occasionally so arrests the further development of the malady, that no more serious or severe surgical measures are to be considered necessary. But, unfortunately, in a large number of multilocular ovarian dropsies, all these measures prove primarily or ultimately, totally ineffectual; palliative tappings come to be required more and more frequently; and it becomes a matter of almost mathematical certainty that the poor patient will ere long sink under their repetition, or under a continuance of the disease. Still, when the practitioner is driven to bay in this manner, the remaining cases are not by any means all of them cases for ovariotomy. Already in many such firm adhesions exist between the surface of the ovarian tumour and the contiguous surface of the peritoneum, that if the operation were begun it could not be finished. It is this circumstance that has, perhaps more than most others that I alluded to in my last lecture, prevented ovariotomy from being adopted by some surgeons into the class of legitimate operations. Out of the 292 cases of ovariotomy collected by Fock, in only 200 instances was the operation completed, with the result of 111 recoveries to 89 deaths. But out of his collection of 292 cases, there were no less than 92 instances in which the operation, after it was begun, was not completed; and in 71 out of these 92 the cause of failure was the extent or strength of the old inflammatory adhesions of the sac to the contiguous peritoneum. Those practitioners that are in the habit of performing ovariotomy do not regard now a considerable amount or extent of adhesions as an insurmountable barrier to its completion; but where the adhesions are very great, the proper plan of surgical proceeding—if any surgical proceeding besides tapping is adopted—is, as I have already tried to show you, the establishment of a permanent or fistulous communication between the largest cyst or cysts and the cutaneous surface. The cases, then, fitted for the operation of ovariotomy are specially, if not entirely, those in which the tumour is multilocular; in which tapping and other milder surgical measures have failed to stay the progress of the disease, or are inapplicable from the great number and small size of the component cysts; in which the disease is evidently going on steadily, and at no remote period to the destruction of the patient; and yet the tumour feels entirely or comparatively free from morbid adhesions, and the general constitution of the patient is such as to present no counter-indication to the performance of this formidable operation. Under such a painful combination of circumstances, the question comes to be, Are we to recommend our patient to linger on for a few weeks or months longer, and submit to the usual progress and fatal termination of the disease, and to die without any surgical proceeding at all, or are we to counsel her rather to submit by ovariotomy to an operation that certainly may very probably inflict speedy death, but which, on the other hand, if it prove successful, gives her all the best chances of renovated health and prolonged life? I must here emphatically repeat what I have stated to you before, that in this, as in other capital and dangerous operations for chronic disease, the patient herself, or her friends, ought as much, or more than the physician, to decide the question of surgical interference or non-interference.

Supposing that you had made up your mind as to the possibility of removing the ovarian tumor, and that the propriety of the operation had been determined, it only remains to consider how the operation is to be effected. Let me, therefore, as briefly as possible, explain to you

The Mode of Performing the Operation.—1. *Preliminary Measures.*—Before exposing your patient to the risk of this, as of any other serious operation, you are, of course, bound, in the first instance, to prepare your patient, as far as possible, by attempting to raise the standard of health, by means of tonics, and to diminish in some degree the chances of inflammation, by putting her for a time on antiphlogistic diet and regimen. Some have recommended that the heat of the room in which the patient is to be operated on should be raised some degrees above the average temperature, under the idea that the cold air might prove irritating to the exposed peritoneum. But such a precaution seems hardly to be necessary; for there is no direct proof that the peritoneum is more likely to be inflamed in consequence of being exposed to the atmosphere at a low temperature, than it is when exposed to heated air. If the room be heated, care must be taken to prevent the air from getting too dry by impregnating it with the vapour of water. When the operation used first to be performed, indeed, the greatest care was taken to have heated cloths in readiness to apply to the peritoneal covering of the intestines, when by chance they were protruded; but those who are in the habit of operating frequently, apparently lose all such great fears in regard to the free exposure and rough handling of the peritoneum. Even where inflammation has existed, and adhesions have been formed between the surface of the tumor and the abdominal wall and viscera, these adhesions have in numerous cases now been freely torn up, and the peritoneum roughly sponged and wiped, without giving rise to any such dangerous consequences as the former prejudices of the professional mind would have led us to expect.

2. *Instruments required.*—The instruments necessary for operation are but few, and easily enumerated; all that are required being a scalpel or bistoury for making the incision and dividing the neck of the tumour; a trocar

Fig. 4.

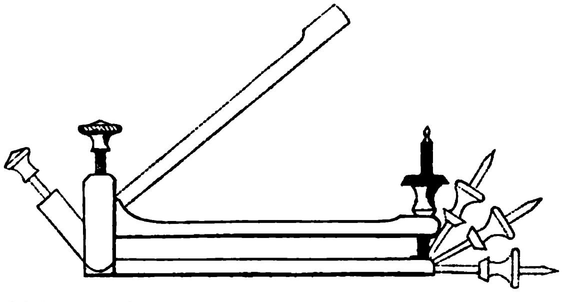


Fig. 4.—Mr. Wells's clamp, for securing the pedicle of the tumour, in cases of ovariotomy.

and canula, for evacuating any large cyst; a clamp, for securing the pedicle, such as I show you (Fig. 4) or perhaps some acupressure-needles, instead—which would be of use also for arresting the hemorrhage from any vessels that might be divided in large adhesions—or you may prefer to use the artery forceps and ligature, or torsion; and, finally, a needle and iron wire will be required for sewing up the wound.

3. *Position of the Patient.*—It has been the common custom to place the patient near the foot of a bed, the thighs separated, and the feet supported by footstools, the surgeon sitting or standing between the legs of the patient. But there are obvious disadvantages in such a position. The surgeon is awkwardly placed, both for operating, and for seeing and securing bleeding vessels. So are the assistants. It is also a fatiguing position for a patient—one in which it is difficult to keep her covered; and one in which fainting is more likely to occur than in the recumbent position. The latter is much to be preferred. In hospitals, the ordinary operating-table answers very well; but in private practice you are not likely to find in many houses the sort of bed or couch which is altogether suitable. You want a narrow table, which can be readily moved, and Mr. Spencer Wells tells me that he finds the table which he generally uses answers the purpose remarkably well. It is a couch which he keeps in his consulting-room, and it is so made, that, by unscrewing the legs and folding up the frame, it can be readily taken to a patient's house, as the old lying-in chair still is in some parts of Europe. The pillows are so arranged that they can be fixed at either end, so that they cannot slip off; and there is a moveable tray beneath to hold the instruments. The table is brought to an angle of about 45° to the window, so that the light falls well both for the operator and assistants. The gentleman who administers chloroform stands at the head of the bed. The operator is placed on the right side of the patient. An assistant may stand on the left of the operator, and two or three on the opposite side of the patient, all seeing well. The contents of the cyst are carried by the tube trocar to the pail below. The patient is carefully protected from cold by blankets folded round the feet and legs, and others round the chest, her own night-dress being pushed up under her arms, to keep it dry, so that there may be no necessity for changing it when she is lifted from the couch to her warm, dry bed.

4. *The Operation.*—There are several stages in the operation, each requiring our special attention. We begin with,

a. The Incision.—The operation is begun by making an incision with an ordinary scalpel or bistoury through the abdominal walls in the line of the linea alba, from the region of the umbilicus, downwards towards the pubes. Cases of ovariotomy were at one time divided into two classes, called respectively the operation by the short and by the long incision, according as this incision was small in extent, and begun below the level of the umbilicus, or was much longer, and stretched considerably above the umbilicus; and the relative dangers or advantages of the two modes of operation were at one time warmly discussed. It was very generally believed, at first, that the smaller the opening in the abdominal parietes, the safer the operation would be; but further experience has shown that the greater liability to peritonitis and displacement of the bowels which was supposed to attend the operation by the long incision, is more than counterbalanced by the greater liability to laceration of some of the tissues and escape of the cystic contents into the peritoneal cavity, and by the greater difficulties which present themselves in the course of the operation by the short incision. So that now most operators seem to prefer to open the abdomen freely, so as to be able more easily to separate adhesions and bring out the tumour, believing, at the same time, that the risk of peritonitis being set up is almost, if not altogether, as great when an opening is made, barely sufficient to allow of the tumour being dragged through it. The incision is not made at once through all the thickness of the wall, but only down to the outer surface of the peritoneum, which is first punctured and then divided in the rest of its extent by means of a probe-pointed bistoury or a herniotomy knife.

b. Evacuation of Cyst or Cysts.—The tumour will be found pressing forward in the opening, and tending to bulge through it; and if there be one or more cysts of large size, it is well at once to tap them, as by this means the bulk of the mass to be removed will be much diminished. In this case, perhaps, there is no better instrument for withdrawing the fluid than the trocar and canula I have already shown and described to you, with an india-rubber tube attached to it to carry away the fluid to a receptacle on the floor, without exposing the patient's person and clothes to the discomfort of being soaked by it in running off. Here, however, such an effectual emptying of the cyst need not to be insisted on, and therefore the instrument I refer to may be used for the sake of the decided advantages which it offers in other respects.

c. Separation of Adhesions.—The tumour thus diminished in size is to be drawn out through the opening in the abdominal walls; and to be able to do so you will require sometimes to separate any adhesions the tumour has contracted with the neighbouring organs. These adhesions can usually be most safely separated with the fingers or the handle of the scalpel. Sometimes, however, they are too firm and of old standing, and require the use of the blade for their division. Occasionally, a vessel bleeds, or the inner surface of the abdominal wall on the severance of some well-established adhesion. In such a case you must endeavour at once to arrest the hemorrhage, and I believe you will succeed in doing so with ease and success by means of an acupressure-needle entered from the cutaneous surface, and passed through the thickness of the abdominal wall till its point finally emerges in the cutaneous surface, at a distance of one and a half or two inches from its point of entrance. The head and point of the needle will thus remain exposed on the surface of the abdomen while the vessel on the inner surface will be compressed between the resistant tissues of the wall and the short bridge of the needle that lies exposed towards the peritoneal cavity.

d. The tumour, then, having been brought out through the wound, you have next to proceed to cut across the pedicle, and use some means to prevent the flow of blood from its divided blood-vessels. And here, perhaps, we meet with the question of greatest importance in connection with the operation. The earlier operators used to tie the pedicle tightly at its narrowest part with a piece of whip-cord, and then to divide it close to the tumour. But as it sometimes happened that the cord lost its hold and slipped off the cut extremity, thus giving rise to hemorrhage internally, this means was speedily given up. Then, for a time, operators adopted the plan of passing stout threads or pieces of thin cord through the neck of the tumour at one or more points, and strangulating it in the same way as we do when operating for the cure of internal piles, or an erectile tumour of the skin, by means of the ligature. By this means it was found possible to control the hemorrhage most effectually, and when the threads had been duly tightened, their ends were brought out through the lower angle of the wound in the abdominal parietes. But as these threads acted the part of irritant substances in the peritoneal cavity, and lighted up in many cases inflammation of this membrane; and as the gangrenous and ulcerating stump of the pedicle within the grasp of the ligatures was also a further source of irritation and of danger, all who have thought much upon the subject have felt anxious that some other means should be devised by which these dangers might be avoided. Dr. Marion Sims has proposed to substitute metallic ligatures for cord and other organic ligatures for tying and securing the ovarian pedicles. He did so under the hope that the metallic would prove less irritating than the organic thread, and might be left lodged *in situ.* But the pedicle on the free side of the ligature would be sure to die and slough, if the ligature were drawn tight, whatever was its composition. Mr. Spencer Wells, some time ago, suggested the division of the pedicle by the *écraseur*—a plan which has been followed successfully by Drs. Atlee and Pope in America in some cases, but in others it has not afforded that comple im nunity against hemorrhage which is necessary for the safety of the operation. In London they now make use in most cases of a clamp (see Fig. 5) such as I have already shown you, which is screwed firmly across the pedicle, and after the tumour has been cut away, the stump, tightly embraced by the clamp is brought out, and made to lie exposed on the surface of the abdomen transversely to the line of incision.

You might not be able at once to conceive how the comparatively short stump of an ovarian tumour could thus be brought and kept on the surface of the abdomen, without producing an injurious straining on the organs and vessels with which it is connected. But if you will only bear in mind that the abdominal walls, so long and so much distended during the growth of the ovarian tumour, must now remain collapsed and yielding after that mass has been removed, like the loose and flaccid walls of the abdomen after parturition, the difficulty will disappear. That a certain degree of dragging, however, is still kept up—particularly in cases where the pedicle or stump happens, and sometimes occurs, to be very short—we can hardly doubt; and although the clamp has in a number of cases been found to answer well, yet its use is attended with certain dangers and drawbacks, that render it still desirable to find out some simpler means of arresting the flow of blood from the cut ends of the vessels in the divided pedicle. I believe by the application of acupressure we may yet be able to check the hemorrhage effectually and more safely than by any of the methods hitherto employed for the purpose. A needle, or needles, such as I show you might, before the pedicle is divided by the knife or *écraseur*, be passed through the abdominal wall, and through one margin of the pedicle from without; and after being made to cross nearly the whole breadth of the pedicle, it could be made to pass through its other margin, and then outwards again through the abdominal wall, so as to compress the pedicle between the exposed portion of the needle and the abdominal parietes with a degree of force sufficient to arrest the flow of blood from its injured vessels. The pedicle being thus pinned against the abdominal parietes in the iliac fossa, there would be no dragging ar displacement of the pelvic organs, and no danger of incarceration of the bowel, as has just happened in one case in London, where the pedicle was fixed to the wound; and the risks attendant on the sloughing and ulceration of the stump would be avoided, for the acupressure-needle could be removed after fifty or sixty hours, or perhaps even less—after the vessels had become occluded, but before the pressure exerted by the needle had been kept up for such a length of time as to produce that ulceration which we know to be the necessary result of too long-continued pressure on any portion of the animal frame. Should the resistance offered by the abdominal walls not be found sufficient to enable the single acupressure-needle to produce efficient compression, a second or third could be introduced, either parallel with the first, or embracing only a limited portion of the pedicle, or crossing the others as they enter and emerge from the pedicle. If we can succeed in restraining the bleeding from the divided neck of the tumour by some such means as this, we shall be able to do away with some of the greatest sources of danger in connection with the operation, and so to perform it with better hopes of ultimate success.

Fig. 5.

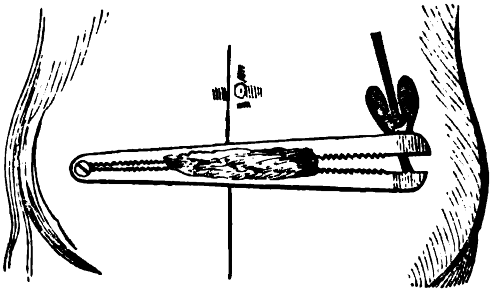


FIG. 5.—Sketch showing the manner in which the pedicle is secured with the clamp, and the position in which the clamp and pedicle are kept when the operation is completed. (Wells.)

Closing the Wound.—Having secured the pedicle of the tumour, all you have now to do is to wipe the peritoneum thoroughly clean, and then to proceed to close up the external wound. This you may do either by means of needles like those used in the operation for hare-lip, or, what is equally efficacious, by means of simple metallic stitches—either of silver or of iron. If you are operating on a wealthy patient, or on the happy inmate of some overflowing wealthy institution, you may, perhaps, prefer to use the suture of silver, which certainly has this superiority over the iron thread, that it is more expensive. But usually, I believe, you will rather employ the iron wire, which is at once cheaper and stronger, and as efficacious and manageable. A variety of needles have already been invented for stitching up wounds with metallic threads; but, however ingenious, they are all quite superfluous, for the instrument that you will find the most convenient and the best for sewing wounds with wire-threads is the common needle in a fixed handle, generally spoken of as a "tumour needle," or "pile needle," and constantly used for passing ligatures through the base of hemorrhoidal and erectile tumours which we desire to strangulate. By means of such a needle, with a considerable curve, you can easily take a deep hold of the abdominal wall; and in closing up wounds in the abdomen it is advisable always to pass the suture deeply, so as to keep the divided margins of peritoneum in perfect contact. The needle is passed unarmed through both sides of the wound—close down upon the peritoneum, but without wounding it. I should say, however, that Mr. Spencer Wells is strongly in favour of carrying the sutures through the peritoneum, as he thinks the cavity is then more effectually protected from the pus and secretions of the wound. When the point has protruded through the further side, the end of the wire is passed to the distance of half-an-inch or so through the eye of the needle, which, as you know, is situated near its point: and when the needle thus armed is withdrawn, it leaves in its track the wire-suture, which may then be twisted or tied across the line of incision, and which may not be removed till the wound has completely united.

5. *The After-treatment.*—When the operation has been completed, the patient is put back to bed, and kept thoroughly clean, quiet, and undisturbed. No special kind of dressing is required for the wound; and, indeed, you will find it best to keep it altogether undressed, and merely protected by means of an arched frame placed over the patient's body. Only in cases where the clamp is made use of and the sloughing stump of the pedicle is brought to the surface of the abdomen it is sometimes found necessary to apply a poultice of charcoal, or some other disinfectant, to counteract the disagreeable odour arising from the putrescent discharges. I would further enjoin on you the propriety of keeping the patient under the influence of opium, with the double view of dispelling the pain and depression which almost necessarily succeed such an operation, and of checking, in some degree, the action of the bowels,—an indication of paramount importance, when we remember how essential it is to the ultimate success of the operation to have all the parts concerned kept for some days at most perfect rest. At the same time the patient may be allowed to partake freely of ice, which is useful at once in counteracting the sickening effects of the operation, and of the opium, and in moderating the feverishness to which she is inclined. Where there is a tendency to sinking, the patient must have an allowance of wine or brandy. Peritonitis not unfrequently springs up; and, of course, you must be prepared to meet it, and to treat it by all appropriate measures. We may sometimes expect also to meet wih other

Results of the Operation.—I have already spoken at sufficient length of the degree of mortality attendant on the operation; but I have said nothing as to the ordinary sources of danger. These are, however, so obvious, that I need not do more than name them now. Thus, for example, many patients die exhausted from the shock of the operation. Sometimes hemorrhage occurs from some of the pelvic vessels, which it is found impossible to control. A case occurred lately in London where hemorrhage nearly proved fatal, which was found to be proceeding from an injured vein in the pelvis; and I have known a patient dying some hours after the operation, where there can be little doubt that the fatal issue was mainly due to the immense quantity of blood lost from a wound in some of these veins, which it was impossible to trace out and close. Fatal hemorrhage, of a secondary nature, sometimes, though rarely, occurs from the vessels in the stump, and carries off the patient. Perhaps the most frequent cause of death in connexion with the operation is peritonitis. Occasionally, patients survive all the more immediate perils of the operation, to be wasted away and finally to succumb under the effects of prolonged discharges and chronic inflammations. But sometimes, also, we have the happiness of seeing patients survive all the dangers of the operation—both immediate and more remote—restored to the active duties of life and the full enjoyment of perfect health, and some of them even giving birth to families afterwards.—*Med. Times and Gazette, Jan. 7, Feb. 4, and March 24, 1860, pp.* 1, 107, 283,

115.—OVARIOTOMY.

(Cases under the care of SPENCER WELLS, Esq., Surgeon to the Samaritan Hospital.)

[The first case was in a single woman, aged 29. The tumour had existed altogether a year, and had been tapped five weeks before admission, with but little diminution in size. The patient was pale, weak, and emaciated, but the tongue clean and appetite good. The circumference, at the umbilicus, was thirty-six inches. It was decided to perform ovariotomy without delay.]

On Oct. 12, chloroform having been administered, Mr. Wells removed the tumour by the small incision. He divided the abdominal parietes over the linea alba to the extent of about four inches midway between umbilicus and symphisis pubis, thus making an opening just large enough to admit his hand. Passing this all over the surface of the cyst, he broke down some extensive adhesions to the parietes, and then emptied several of the larger cysts through a large trocar, drawing them out one after the other as they were emptied. Some portions of semi-solid matter, and aggregations of small cysts were drawn out, and a short pellicle secured by a clamp, about an inch from the right side of the uterus. The left ovary was found to be healthy. The abdominal and pelvic cavities were carefully cleaned by sponging, two small vessels were secured by ligature, the clamp fixed on the surface of the abdomen, and the wound accurately closed by deep and superficial wire sutures; four of the former being passed through the whole thickness of the parietes, including the peritoneum. The tumour consisted of a few large and a great number of small cysts filled with a viscid fluid, the whole weighing about forty-five pounds.

Progress after Operation.—It is unnecessary to follow the daily notes of this case, as for the first week it was one of gradual recovery. There was so little pain that no medicine was given either by mouth or rectum for the first four days. The pulse remained feeble, ranging from 80 to 100, and perspiration was very profuse. There was very little vomiting. Hot linseed poultices were kept to the abdomen, brandy and soda water, port wine, beef-tea, and the brandy and egg mixture of the pharmacopœia, were given at short intervals. On two or three occasions, on the fifth and sixth days, when there was a little pain, twenty minims of laudanum in an ounce of water were thrown into the rectum. The bowels acted spontaneously on the fourth day. The clamp was removed on the same day, and the deep sutures. She went on remarkably well untill the tenth day, when the wound was perfectly united, with the exception of a small depression at the lower part, where a small slough on the stump of the peduncle interposed. On wiping away this piece of slough very free hemorrhage took place suddenly, and several ounces of blood were lost during ineffectual attempts to find the bleeding vessel. Mr. Wells then applied a saturated solution of the perchloride of iron in glycerine, which instantly arrested the hemorrhage. She did not appear worse for the bleeding, but did not improve during the next three days, and on the fourth day the bleeding recurred. The clot formed by the perchloride had been loosened, and the hemorrhage was rather alarming. On carefully wiping away all clot, Mr. Wells then discovered that the bleeding vessel was not in the peduncle, but was a small artery in the parietes which had apparently been wounded in passing the lowest suture. This was tied, and no further bleeding took place, but there was for some days rather a free discharge from the lower part of the wound of fetid sero-sanguinolent grumous fluid and broken-down clots of blood. She remained weak for some time and the bed-sore which she had before the operation increased and caused her a good deal of pain, but she gradually gained strength, and left the hospital in a very good state of health on the 7th inst. Dr. Jackson has since written to say that she bore the long railway journey to Barnsley very well, and was "in the enjoyment of capital health and spirits."

[The second case was a lady's maid, single, aged 27. The tumour first appeared four years ago, remaining, however, but small for a year and a half, then commencing to increase. She was tapped for the first time in August, 1858, and up to the time of operation Dec. 6, 1859, was tapped seven times more. The cyst was evidently multilocular, and a large solid mass could be detected on the left side. At the operation,]

Mr. Priestley administered chloroform. The hospital craft were present, and Drs. Sims and Westmacott. Mr. Wells commenced by making an incision over the linea alba, midway between umbilicus and symphisis pubis, just large enough to admit his hand, intending to do no more if the adhesions proved to be very firm; but as he found the greater portion of the surface of the cysts nearly free, and some adhesions at the anterior border of the solid portion yield readily before the hand, he determined to proceed, and emptied both cysts through a very large trocar. They were then drawn forward, and the wound enlarged up to the umbilicus. Some firm adhesions of the omentum to the upper and back part of the solid portion were then separated, and two or three small cysts burst during this part of the operation. The whole of the cysts and tumours were then drawn out of the abdominal cavity. The mass was attached to the left side of the uterus by a large broad peduncle, and in addition to this a thick vascular band of adhesion connected the lower part of the solid portion with the abdominal wall behind the sigmoid flexure of the colon. This band was divided by the erasure. The peduncle was secured by a clamp, and the tumour removed. Two vessels which were bleeding freely, far back on the abdominal wall, having been opened in separating adhesions, were then secured by ligature, the pelvis and lumbar fossæ carefully sponged out, and the wound united by deep and superficial sutures of iron wire. A pad of lint and a flannel belt were then put on, and the patient placed in a warm bed. She remained very low for about an hour, when she rallied and began to complain of pain in the abdomen. Half a drachm of laudanum was thrown into the rectum, and the injection was repeated in an hour, but only twenty minims were then thrown up. Some brandy and soda water were given and a little port wine. She was very comfortable in the evening. The pulse about 90. There had been some vomiting, but it was not very troublesome. The skin was warm and moist, and she was in good spirits. She passed a very tolerable night; had two injections of twenty minims of laudanum, the pain became troublesome; a moderate quantity of urine had been twice removed by the catheter. She had slept pretty well, and up to 10 a.m. on the day after operation seemed to be going on satisfactorily. She then began to sink rapidly, felt cold and faint, and the pulse became imperceptible, notwithstanding the free use of stimulants both by the mouth and rectum. She died twenty-three hours after operation.

The tumour consisted of two very large cysts, which contained upwards of forty pounds of fluid, and of a semi-solid mass of small cysts, which weiged eleven pounds and a-half. The whole removed, therefore, weighed about fifty-three pounds.

The abdomen was examined on the following day. The wound was found to be united on its peritoneal aspect by recent lymph, with the exception of

the spots where the peduncle passed below, and the ligatures on the vessels above. One loop of small intestine adhered by soft, recently effused lymph to the wound for the extent of about an inch. The portion of omentum which had been separated from the cyst was slightly congested. There were between one and two pints of bloody serum in the peritoneal cavity, but not a morsel of clot. There were signs of peritonitis about the broad ligament on the left side, and on the parietes near the wound, and on two or three coils of intestine which lay near it; but there were no marks of general peritonitis. The ligatures did not appear to have set up peritonitis in their track. The uterus was normal. The right ovary was about the size of a large walnut, and nodulated on its surface. The peduncle, consisting of Fallopian tube and broad and round ligaments, was securely fixed by the clamp outside the wound.

Remarks.—This was one of those desperate cases where the surgeon who looks to his own reputation as an operator whose results are numerically successful—or, less selfishly, to the credit of surgery in general, or of ovariotomy in particular—would certainly refuse to operate. In a case where the probabilities are generally against success an operator is not only likely to lose by a comparison with the results of others who only operate in fafavourable cases—but he may bring discredit on an operation, strengthen the belief in its excessive mortality, and deter other surgeons from recommending it, or patients from submitting to it, where the conditions were hopeful. And it becomes a moral question of no small magnitude how far these considerations shall prevail against the simple question,—"What is my duty to this patient?" It appears to us that if once the surgeon is convinced that he ought not to allow any patient to go on through a life of suffering to an inevitable death, when there is a possibility that an operation may restore that patient to health, it is his duty to cast aside all other considerations, and do his best for the poor creature who has confided in him. Then, of course, come the important practical questions.—Is the disease necessarily fatal? Is an operation necessarily fatal? Is there a moderately fair chance of recovery? Does the patient fully understand the risk? Is suffering so great and life so irksome, that she is anxious to be relieved even at so great a risk? All these questions were most anxiously considered in the above case, and, although the result has been unsuccessful, the mother of the patient, and those who knew her best and know what death by the natural progress of ovarian disease is, haves certain melancholy satisfaction in feeling that nothing has been left undone which might have been done, and that some weeks of hopeless, lingering suffering have been spared.—*Med. Times and Gazette, Dec.* 17, 1859, *p.* 605.

116.—HYSTERIC TYMPANITIS, OR PHANTOM TUMOUR OF VERY LARGE SIZE.

(Case under the care of Drs. Ramskill and Jones, Metropolitan Free Hospital.)

[The following notes of a very interesting case still under observation at the Metropolitan Free Hospital, were originally made in September, 1857. The patient is a girl, aged 19, unmarried, and of peculiar and morbid temperament.]

"Her abdomen presents pretty exactly the appearance of a seven months' pregnancy, and her father even went to the length of turning her out of his house on the supposition that such was her condition. She applied to several medical men, and very different opinions were expressed. On admission, it was found that the abdomen, although round and full, was of clear percussion note in every part, that the umbilicus was depressed instead of protruding, and that no tumour whatever could be felt. On vaginal examination the uterus was found small, and its cervix of due length. Respecting the absence of any tumour in the abdomen, some caution must be exercised in giving an opinion as she invariably holds the muscle so tense and board-like, that pressure cannot be made with any satisfactory results. At times the abdomen will appear to be larger on one side than on the other, and the external contour as a very large ovarian cyst is very exactly stimulated, but then there always remains the clear percussion note, which is inconsistent with such a supposition."

The girl has since been in several other hospitals; and she states that at one she was strongly urged to submit to the operation of paracentesis. Latterly she has again been attending at the Metropolitan Free, under the care of Dr. Jones. The prominence of her abdomen is now such that any one not having witnessed the trial of chloroform, might well be excused for entertaining the utmost skepticism as to the possibility of its disappearance. It exactly resembles that of a patient at the full period of pregnancy. The eye of a careful observer may, however, detect some differences between the girl's bearing, and that of a pregnant or dropsical person. Her back is more arched, and her gait has less of the swinging motion from side to side—"waddling"—a difference no doubt due to the fact that she has no real weight to carry. About two weeks ago, in order again to make a thorough and satisfactory examination of the state of things, she was put under the full influence of chloroform.

"The abdomen rises evenly in all directions, and is most prominent about two inches below the level of the umbilicus. A brown streak extends from the umbilicus to the pubes. The area of clear tympanitic resonance extends over the whole front of the abdomen. The measurement over the most prominent part is thirty-three inches and one-third. Posteriorly both loins are dull on superficial percussion, the left side being entirely dull, even on the deepest percussion, while on the right a forcible stroke elicits a decided degree of resonance. As unconsciousness was supervening, under the influence of chloroform, the abdomen at once flattened, until not the slightest appearance of tumour existed. It was soft in all parts, and the promontory of the sacrum could very easily be felt. No escape of flatus was noticed.

"The abdomen now measures only thirty-one and a half inches. Examined per vaginam the cervix uteri was found to be long, and in every way healthy; the uterus was freely moveable. Fæces are felt in the rectum, and a mass the size of a small egg, to the left side, about which some difference of opinion existed as to whether it was fæcal or otherwise. As the influence of chloroform was passing off, the abdomen quickly resumed its original state, and in a few minutes after, when she had regained consciousness, it was nearly as prominent as ever. She took the chloroform remarkably kindly, and recovered from its effects very quickly."

One of the interesting features in this case is its long continuance. All forms of anti-hysteric remedies have been exhausted upon it without the slightest result. Marriage is, indeed, in all probability, the only remedy which can be expected to influence a condition which partakes more of moral than of physical derangement. Several similar cases have recently been under our notice, in one of which the patient has been told by several surgeons that she is the subject of ovarian dropsy. In this instance, also, as in the above, the patient is unmarried, and the same peculiarities of aspect are exhibited as have been just described. The value of the exhibition chloroform as a means of diagnosis can be scarcely overrated. The cases are those to which mistakes in diagnosis are of the utmost importance, since they would inevitably lead either to unfounded imputation on the patient's chastity, or to the belief in the existence of incurable disease, or to the attempted performance of most dangerous operations. The records of surgery contain the narratives of not a few of such, in which either paracentesis has been performed, or the still more fatal mistake committed of laying open the abdomen, in order to extirpate an ovarian cyst. Against such errors the exhibition of chloroform affords entire security, and when any doubt offers about a case, it ought never to be omitted. As insensibility is induced, the board-like condition of the abdominal parietes, which had previously baffled all attempts to ascertain the exact state of the viscera, disappears, the diaphragm rises, the belly flattens out laterally, the anterior prominence dis appears and the hand can be passed back till it touches the sacral promontory.

Those desirous of further information on this subject, may refer to Dr. O'Ferrall's paper; to a series of cases published in this journal some years ago: and to a note by Dr. Priestley on Hysteric Tympanitis, in our first volume for 1858.—*Med. Times and Gazette, Dec.* 10, 1859, *p.* 579.

117.—DEODORISATION IN CARCINOMA UTERI.

By THOMAS SKINNER, M.D., Liverpool.

[After a few preparatory remarks, the author states in succession the means tried by him for the purpose of obviating the extremely offensive odour of the vaginal discharges in cases of Carcinoma Uteri. We give his observations upon the two remedies found to be the most effectual.]

Condy's Disinfecting Fluid (purple).—This fluid, which is, I believe, a concentrated solution of the permanganate of potass, was used in the proportion of two drachms to a pint of water as a vaginal injection three times a day. The proportions were rapidly increased up to one fluid-ounce to a pint of water. The result was in every way satisfactory. A gentleman digitally examined the patient with me a few days after it had been used, and the smell was very much lessened, as also the amount of the discharge. (At this period of my observations, a common pewter vaginal syringe was used, and I cannot but think that this accounts for the success being incomplete.) One great advantage in the use of the permanganate is, that it has no smell of its own, and that, being soluble, it may be used either in the form of a medicated pessary, or in that of an injection. Its lovely *mauve* colour renders it both fashionable and pleasing to our patients. From my experience of its use, I can confidently recommend it in the proportion of one fluid-ounce to a pint of water, as a deodorant in cancer of the uterus; a cupful to be used as an injection thrice daily, and the strength to be increased if found necessary.

Tar-Water. Aqua Picis Liquidæ, D. While reading a report of the discussion on deodorisation between MM. Renault and Velpeau lately, I observed that it was merely a matter of economy and of olfactory preference, which made the difference between the use of coal and vegetable tar in the manufacture of the disinfecting powder; and, as I was much inclined to believe that the tar, or one or more of its active principles, is the essential ingredient, I determined to try the effect of the injection of tar-water. I was strengthened in this idea by having read the eulogiums passed upon it by Bishop Berkeley and others, and in the pages of the Medical Commentaries, alluded to by Dr. Mackenzie at the meeting of the Medical Society of London, on October 24th.

My patient has now used it undiluted for a month or more, and is likely to continue its use, as its beneficial effects have far surpassed my most sanguine expectations. The discharge has much decreased, and is for the present at a minimum. The foul smell entirely disappeared on the first day she used the tar-water as an injection; and, in its stead, the apartment, the bed, and the patient, have a mild and really refreshing aroma of a shipbuilder's yard in the distance. The patient and her friends have become so accustomed to this slight, and rather agreeable odor, that they are not now sensible of its existence; and they, as well as myself, can bear witness that none of the many measures adopted for the purpose of deodorising the discharge, have been so eminently successful as the simple injection of tar-water.

Formula for Tar Water.—Tar-water was at one time officinal in the Dublin Pharmacopœia; but, in the last edition, the formula has been omitted:—"Aqua Picis Liquidæ, D. Tar, lb. j; water, con. j; mix, stirring with a stick for a quarter of an hour; as soon as the tar has subsided, strain the liquor, and keep it in well closed jars." (Neligan's Materia Medica, 3rd ed., p. 374.) Pereira gives a different formula:—"Tar, Oij; water, cong, j (wine measure):" otherwise the same as Neligan. My patient has always used the formula recommended by Neligan.

Properties.—Tar-water ought to be perfectly clear, and of a pale sherry or amber colour. It has a bitter, resinous, and considerably acid taste; and a slightly creasotish, but rather agreeable empyreumatic odour. It reddens litmus-paper, and has a density of about 1.005. "The volatile oil in tar-water is partly held in solution by acetic acid, which, as is well known, dissolves creasote. It" (tar-water) "consists of water holding in solution acetic acid and pyrogenous oil and resin." (Pereira's *Materia Medica*, p. 1205.) The late Professor Royle says, that tar-water contains "creasote and other matters in solution." (Manual of Materia Medica, p. 647.)

Mode of Administration.—By the aid of Higginson's invaluable syringe, a tepid solution of soap and water is first to be injected; and, immediately thereafter, a breakfast-cupful or more of undiluted tar-water. This should be done twice a day, if the patient's strength will admit: hitherto, a more frequent use of it has not been required. It is necessary that the tar-water should come into contact with the ulcerated surfaces: therefore, the recumbent posture is preferable, and a long vaginal nozzle to the syringe is of advantage. If there is an intelligent, handy nurse in attendance, and the ulcerated cavity is extensive, and its lips lobulated and lapping over each other, a piece of lint saturated in tar-water, with a piece of tape or thread attached to it, may be inserted within the cavity, or the point of the nozzle may be directed into the ulcerated cavity before injecting.

Lastly, as important in itself, and as bearing upon the action of many of the substances alluded to, I would briefly call to remembrance the well known properties of

Creasote, which literally signifies flesh-preserver (from κρέας, flesh, and σώζω, I preserve), is the most important of the products obtained from wood-tar by Reichenbach, in 1830. It dissolves several salts, particularly the *acetates*, the acetate of silver excepted, which it reduces. It is itself completely soluble in acetic acid, wood naptha, and alcohol, in most volatile oils, in the alkaline solutions, and in from eighty to one hundred parts of water. Like the chlorides of iron, mercury, zinc, and other caustics, it is a powerful coagulator of albumen, but, unlike these agents, the insoluble compound which it forms with the tissues *resists putrefaction*. In fact, it is the best known antiseptic; consequently, it is a powerful deodorant. Finlay Dun says: "It is believed to have been the essential agent used in embalming the Egyptian mummies. It is extensively employed in preparing various dried meats, and might be used for preparing subjects for dissection, by dissolving it in acetic acid, and injecting the solution into the veins. . . . Externally, it acts beneficially as a stimulant and astringent, an antiputrescent and deodoriser." (Veterinary Materia Medica, pp. 199, 200.) Of the use of creasote as a deodorant in carcinoma uteri, I believe that Dr. Copland was among the first who employed it. (Cyclopædia of Practical Medicine, vol. iii, p. 1283.)

In connection with the deodorising property of creasote, it is proper to observe that *carbolic acid*, which is undoubtedly the essential ingredient in Smith and McDougall's patent disinfectant powder, is supposed by M. Laurent to be really identical with *creasote*. (Royle's Materia Medica, p. 712.) The late Professor Gregory arrived at the same conclusion as M. Laurent, and about the same time. Professor Gregory stated to the Royal Society of Edinburgh: "If these two compounds be not identical, they are at least very closely connected, and in all probability contain the same radical. It is possible that creasote may be a definite compound of carbolic acid with some allied body. At all events, *it is very remarkable that these two compounds, described as different, should agree in density, taste, smell, antiseptic property, power of combining with bases, power of dissolving resins, indigo, etc., and finally, in composition*, although perfectly pure creasote has not yet been analysed." (Edinburgh Monthly Journal. 1845, p. 811.) Cooley, speaking of commercial creasote, says:—"The fluid sold in the shops for creasote is a mixture of creasote, picamar, and the light oil of tar; in many cases, it is little else than impure carbolic acid, with scarcely a trace of creasote." (Cyclopædia of Practical Receipts, 3rd ed., 1856, p. 665.)

It would appear from the above statements, that, to the best of our knowledge, the disinfecting agent in even Smith and McDougall's powder is none other than creasote, or its analogue carbolic acid; and, as carbonic acid is the acid portion of the volatile oil which is obtained by the distillation of *coal* tar (*Royle*, p. 712), I make no doubt that creasote or carbolic acid is the active ingredient in the powder and paste of MM. Corne and Demeaux, as prepared from *coal* tar. Creasote is doubtless the active ingredient in the powder and paste prepared from *wood* tar. There can also be no doubt that creasote is the active ingredient in woodsoot (*fuligo ligni*) and its preparations, which are so highly extolled by the Dublin and continental physicians. (*Negligan*, 3rd ed., p. 35; *Pereira*. 3rd ed., foot-note to page 2014. See also an interesting case of the successful external application of the *decoctum fuliginis*, in the case of a severe burn, by Dr. Ebers of Bordeaux, in the *Edinburgh Monthly Journal* for 1841, p. 755.) I feel much inclined to try the *decoctum fuliginis* of the Dublin physicians as a deodorant in *carcinoma uteri*, or the *tinctura fuliginis*, sufficiently diluted with water. For the best account of creasote, I beg to refer to Dr. Cormack's Harveian prize essay for 1836.

The *modus operandi* of creasote, or any of the substances spoken of in this paper as antiseptics or deodorants, I have purposely avoided, as I consider the subject of too speculative a nature to serve any practical purpose.—*British Med. Journal, Dec.* 3. 1859, *p.* 980.

118.—*Cylindrical Pencils of Tannin in Certain Affections of the Uterus.*—This form of application, pointed out by Dr. Bocquerel, seems likely to be of service in the treatment of lesions affecting the cavities of the neck and body of the uterus. In particular, in the fungous conditions of their mucous membranes, with consecutive hemorrhages, the tannin pencils might be advantageously substituted for the intra-uterine injections, which are not always free from danger. Dr. Beoquerel's formula is—

℞ Tannin, 4 parts; gum tragacanth, 1 part; bread-crum, q. s. to give the proper consistence.

These pencils are 5 millimetres in diameter, and 3 centimetres long. To use them, the neck of the uterus is exposed by means of the speculum; a pencil of tannin is introduced by means of the forceps into the os tincæ, and is then pushed into the uterine cavity, and secured there by means of a plug of lint soaked with a concentrated solution of tannin. Once in position, the pencil softens and dissolves, and modifies the tissues with which it is in contact. At the end of twelve hours, the plug of lint is withdrawn by means of a thread attached to it. Every three or four days a new pencil is introduced in the same manner: and after a month of this treatment, the fungous state of the mucous membrane progressively disappears, and the hemorrhages are arrested.—*Bull. de Thérap.*—*Edinburgh Med. Journal, Jan.* 1860, *p.* 664.

119.—ON THE DIAGNOSIS OF TUMOURS OF THE BREAST.

By John Erichsen, Esq., Surgeon to the University College Hospital.

In many cases of tumour of the breast, as well as elsewhere, it is quite possible to pronounce positively, and at once for good or for evil. If the patient be middle aged, if the tumour be hard and adherent to the skin, and more particularly if it be ulcerated; if there be a chain of indurated glands extending into the axilla; if the countenance be sallow, with glistening eyes, and the frame emaciated, the surgeon can seldom be wrong in at once deciding the tumour to be cancer; on the other hand, when the patient is young, the tumour hard but moveable, the constitution delicate but good, the surgeon may require time to perfect his diagnosis. This uncertainty with regard to diagnosis of the tumours of the breast is an inherent element of these diseases, and may even continue after the growth has been removed from the mamma. In many cases surgeons will be in doubt about its nature, and this doubt is not cleared away until the tumour is subjected to careful microscopical investigation. So great is this difficulty, that the most skilful surgeons will occasionally err. It is true that the progress of the disease and the state of the constitution of the patient will eventually throw light on the nature of the disease; but there is nothing pathognomonic in the early stage of the growth of these tumours, and it is at this period that the great difficulty in their diagnosis arises.

In the early stages of all tumours of the breast, the surgeon is at last compelled to form his judgment of their nature by the manipulation of the mass. You can readily understand that, however skilled the sense of touch, however much he may possess the *tactus eruditus*, he may easily be deceived. For instance, if you were to put half a dozen apples under a napkin, and by simple manipulation you were to determine which one was the Ribstone pippin, which a golden one, you might in a few cases be right, but in many you would certainly err. This is precisely what happens in tumours of the breast, though the history of the case will often reduce to a certainty the opinion we had formed of a tumour from its feel, and which otherwise would have remained obscure and unsettled. There is no class of surgical diseases in which errors of diagnosis are so likely to occur as in that of diseases of the breast. The most skilful surgeons may err, and have frequently done so; and, indeed, you would gain more instruction from an account, if it were possible to write such a one, of the errors of surgeons in these matters, than by reading any account of the diagnosis of these diseases. It is in a great measure in consequence of, and by taking advantage of these errors, that the cancer-curing quacks, who, whether lay, clerical, or medical, flourish in all large towns, derive much of their reputation from the public. They have this advantage over medical men. Suppose the "cancer-curer," as he calls himself, fail in effecting a cure, he is then no worse off than the regular practitioner, by whom the disease had previously been pronounced to be incurable. But if a medical man makes an error, and diagnoses cancer when in reality the tumour is a benign one, the patient, knowing that medicine affords her no relief, and that surgery only offers the palliative measure of extirpation, applies to a "cancer-curer"; he uses his caustics, and destroys a tumour, which, being non-malignant, though erroneously believed to be cancer, does not return. Such a case is ever after looked upon by her friends as an incontestable cure of a cancer, which had been pronounced to be incurable by medical men. Hence it is very necessary not to hazard a positive opinion in doubtful cases, or at all events, not to throw one out on the gloomy side.

So far as the breast is concerned, for all purposes of surgical diagnosis, its tumours resolve themselves into two classes; first, the simple; second, the malignant. The simple non-malignant tumours again resolve themselves into three classes; first, *abscesses;* second, *cystic tumours;* third, *solid tumours* of various kinds. These are the distinctions you may establish for the purposes of diagnosis. Now let us examine these different classes a little in detail.

First, with regard to the diagnosis of abscess. Now, there are four kinds of abscesses of the female breast; three of these are not difficult to distinguish as abscess; the fourth often is so. The first is the ordinary subcutaneous abscess between the skin and gland, generally near the areola; it is accompanied by the ordinary symptoms of phlegmonous and superlative inflammation—heat, pain, and throbbing. There is usually no great difficulty in diagnosing this variety; it is often met with at the age of puberty, in consequence of the increased action set up about this time. It occurs alike in single or married females, and usually without any uterine complications. In the diagnosis of this subcutaneous abscess, error will, however, sometimes creep in. For instance, a gentleman of considerable professional skill and acquirement, one day brought a patient to my house with "a tumour of the breast," which he said would require removal. On examining the bosom, I found one of these subcutaneous abscesses, with a good deal of œdema around it. I retired with my friend into another room, told him my opinion, and proposed to open it there and then. "For God's sake," said he, "don't do that. I have said it was a 'tumour,' and I shall get into a scrape if you open it." I told him not to make himself uneasy about that, for I would soon settle matters to his and his patient's satisfaction. I accordingly told the lady that I was glad to say that an operation would not be necessary, as we would be able to bring the tumour "to a head," and then disperse it. I accordingly ordered some resin cerate to be applied; and in a week the abscess had burst, and there was an end of the "tumour," to the great satisfaction of all concerned.

Two other of these varieties of abscess are of an acute and phlegmonous character. The first, that in the substance of the mammary gland; the second, that which forms behind the gland, in the cellular tissue lying between it and the pectoral muscle. These abscesses generally occur during lactation; and, by coupling the acute phlegmonous symptoms with the presence of lactation, there can be very little difficulty in distinguishing them.

The fourth form is that most frequently confounded with solid tumour. It is the chronic, cold, encysted abscess of the breast. This is a sort of abscess which forms deeply in the mammary gland, slowly and without any acute symptoms—usually, indeed, without any local symptoms at all. It is hard, deeply seated, and tolerably well circumscribed. This encysted abscess of the breast has been especially described by Sir A. Cooper and by Velpeau. Both these eminent surgeons record cases of it which have been mistaken and operated on for cancer. A story has been told me by a gentleman who was present, of a surgeon now dead, but one of the most eminent of the many great surgeons that France has produced, who, after lecturing to his class just before operating on a case of tumour of the breast, and explaining, with that minuteness of detail and that lucidity of arrangement which the French surgeons possess in so high a degree, the reasons why the tumour he was about to remove could by no possibility be any but a scirrhus, made his first incision, when a gush of pus took place, and the supposed scirrhus was resolved into an encysted abscess, deeply seated under the mammary gland. Now this accident, happening as it did to one of the most eminent of his day, may occur to others of less professional experience; it has, to my knowledge, occurred six or eight times. But it may generally be avoided by attention to the history of the case, and especially to four points of diagnosis. These points, to which I would direct your special attention, are the following:—

1. Encysted abscess is invariably preceded by some uterine disturbance. You will find that the patient has lately been in the family way, and has either been delivered or has miscarried; or, if not pregnant, has suffered from some other uterine derangement—in most cases, however, of a parturient nature—a few weeks or months previously.

2. You generally find that the tumour, if an abscess, though hard and defined, is not distinctly circumscribed.

3. You almost invariably find—and this is a most important point—œdema of the subcutaneous cellular tissue, near the outer or under border of the gland.

4. There is also usually a spot on the surface of the tumour softer and more tender than the rest, possessing, perhaps, a certain degree of elasticity.

Now, by attention to these four points—namely, pre-existing or concomitant uterine derangement, hard but non-circumscribed character of the swelling, the œdema around or beyond, and the soft tender point—you can rarely go wrong in making your diagnosis. In illustration of this, I will mention a case which occurred here some time ago. A woman, thirty-six years of age, miscarried the sixth or eighth week of her pregnancy. Eight months after this, she applied to the hospital for relief for a tumour of the breast. On examination, I found a circumscribed painless swelling, deeply seated in or under the mammary gland. There had been no pain or throbbing in it, and there was no redness over it. At the lower border, there was a spot which felt doubtfully elastic. I made an exploratory puncture into this, and let out about an ounce and a half of creamy pus. A tent was then introduced; and the induration of the walls, which were excessively thick, gradually disappeared, as the wound healed by granulation.

Beside these chronic abscesses of the breast itself, you may get another class of cases; that is, abscess in the neighborhood of this organ, quite unconnected with the mammary gland. If there is any doubt in your minds, after examining such a case, always put in an exploring needle or trocar; its puncture can do no harm, and it will always clear up the true nature of a doubtful case.—*British Med. Journal, March* 31, 1860, *p.* 239.

120.—*Excoriated Nipples.*—Dr. Pierce observes that he has seen the stramonium ointment, as well as the whole list of astringent and stimulating preparations used; but not one has been so universally followed by good results as that made after the following prescription. ℞. Acid tannic. gr. xx., glycerinæ. alcohol. aa, ℥j. M.—*Boston Medical Journal.*—*Med. Times and Gazette, Feb.* 29, 1860, *p.* 197.

121.—ON THE DIATHETIC DISEASES OF CHILDHOOD.

By WILLIAM JENNER, M.D., Physician to University College Hospital, and to the Hospital for Sick Children.

[The great diathetic diseases of childhood are four, viz., rickets, tuberculosis, scrofula, and syphilis. All are liable to produce striking and peculiar deviations from the healthy structure of particular organs and tissues. The following is a brief enumeration of the leading features of a typical case of each of these four general conditions:]

Tuberculosis.—Nervous system highly developed; mind and body active; figure slim; adipose tissue small in quantity; organization generally delicate; skin thin; complexion clear; superficial veins distinct; blush ready; eyes bright, pupils large, eyelashes long; hair silken; face oval, good-looking; ends of long bones small, shafts thin and rigid; limbs straight. Children the subjects of tuberculosis usually cut their teeth, run alone, and talk, early.

Leading Pathological Tendencies.—Fatty degeneration of liver and kidneys; deposits of formation of tubercle, and their consequences; inflammation of the serous membranes.

Scrofulosis.—Temperament phlegmatic; mind and body lethargic; figure heavy; skin thick and opaque; complexion dull, pasty-looking; upper lip and alæ of nose thick; nostrils expanded; face plain; lymphatic glands perceptible to touch; abdomem full; ends of the long bones rather large; shafts thick.

Leading Pathological Tendencies.—Inflammation of the mucous membranes of a peculiar kind; so-called strumous ophthalmia; inflammation of the tarsi; catarrhal inflammation of the mucous membrane of the nose, pharynx, bronchi, stomach, and intestines; inflammation and suppuration of the lymphatic glands on trifling irritation; obstinate diseases of the skin; caries of bone.

Rickets.—Mental capacity and powers small; muscular force deficient; mind and body inactive; figure short; closure of the fontanelles retarded; face small, but broad; skin opaque; often set with downy hairs. Children the subjects of rickets are late in cutting their teeth, in running alone, and in talking, and their teeth drop early from their sockets.

Leading Pathological Tendencies.—Softening of the bones; enlargement of the ends of the long bones; thickening of the flat bones, and deformities consequent on these conditions of the bones; so-called hypertrophy of the white matter of the brain; chronic hydrocephalus; pulmonary collapse; laryngismus stridulus; convulsions; albuminoid infiltrations of the liver, spleen, lymphatic glands, &c.

Syphilis.—Adipose tissue small in quantity; muscles flabby; cutis rough—deficient in contractility; complexion muddy.

Leading Pathological Tendencies.—Suppurative inflammation of the mucous membrane of the nose; ulceration of the mucous membranes of the nose and of the lips, mouth, throat, and anus; falling of the hair; eruptions on the skin of a peculiar character; induration of the liver; suppuration of the thymus, lungs, &c.

There are pathologists of high repute who regard rickets, scrofulosis, and tuberculosis to be mere modifications of the same disease. I and others hold them to be distinct affections. The whole difficulty of the question lies in the difficulty of determining what is necessary to constitute identical diseases. With reference to some diseases the grounds of separation are broad and unequivocal. Scarlet fever and measles are distinct diseases, because their specific cause is different. Syphilis and tuberculosis are undoubtedly not identical, because syphilis owes its origin to a specific cause, and tuberculosis does not. Rickets, tuberculosis, and scrofulosis are due, it is said, to mal-nutrition, and therefore it is urged they are essentially identical; but it is manifest that the term mal-nutrition is at once very vague and very comprehensive. All diseased action by which unhealthy structure is formed in the place of healthy may be called mal-nutrition. Pus is the result of mal-nutrition, cancer is the result of mal-nutrition, tubercle is the result of mal-nutrition,—and yet the purulent, cancerous, and tuberculous diatheses cannot be held to be identical.

I hold rickets, tuberculosis, and scrofulosis to be distinct diseases in the sense in which tuberculosis and cancer are distinct diseases, and for the following reasons:—

1. Because the general condition in each is perfectly different from that in the other.
2. Because the pathological tendencies of those who manifest these different general conditions are different.
3. Because we rarely see the pathological tendencies of the one manifested by those, the subject of the others, *e. g.*, rickets is absolutely unfavourable to tuberculisation—tuberculisation to strumous ophthalmia.
4. Because, tuberculosis being unquestionably hereditary, we do not find the children of phthisical parents specially prone to rickets or to scrofulosis.
5. Because, although we often find several members of the same family, the subjects of rickets, of tuberculosis, or of scrofulosis, it is comparatively rare for members of the same family to be the subjects of more than one of these diatheses.
6. Because rickets is not hereditary in the sense in which tuberculosis is hereditary.
7. Because the course, prognosis, and treatment of each of these diatheses are different.—*Med. Times and Gazette, March* 17, 1860, *p.* 259.

122.—ON RICKETS.

By William Jenner, M.D., &c.

[Though generally met with among the children of the poor, in large towns, rickets is occasionally seen in those very circumstances least likely to produce it, viz., in the children of the wealthy, living in good country air.]

Rickets is a general, or diathetic disease, manifested after it has existed a longer or shorter time, by certain lesions of the structure of the bones,—and, I say, manifested after a time, because in some cases the general disorder unequivocally precedes the local changes, and before these latter occur, we can predict that they will occur.

In some books, rickets is classed among diseases of the bones. This is a mistake; rickets is no more a disease of the bones, than is typhoid fever a disease of the intestines. Rickets leads to disease of the bones in the same way that typhoid fever leads to disease of Peyer's patches; but there is a general disease preceding and accompanying the disease of the bones in the one case, as there is preceding and accompanying the disease of the intestines in the other. The change in the bones is the anatomical character of rickets.

The most constant and striking anatomical lesions in rickets are,—

1. Enlargement of the ends of the long bones, of the parts where the bone and cartilage are in contact, *i. e.*, where the cartilage is preparing for ossification, and where ossification is advancing in the cartilage.

2. Softening of all the bones.

3. Thickening of the flat bones, *e. g.*, the bones of the skull—the scapula.

4. Deformities which follow from mechanical causes acting on the softened bones, *e. g.*, the deformities of the thorax, pelvis, spine, long bones.

5. Arrest of growth, not only of the bones, but of all the parts directly related anatomically and physiologically to the bones, *i. e.*, of the muscles, vessels, nerves, and teeth.

6. Certain lesions of the pericardium, lungs, and capsule of the spleen, the direct consequences of the thoracic deformity.

7. Less constant, but highly important changes, most commonly affecting the nutrition of the brain, spleen, liver, lymphatic glands, and muscles, and now and then of every organ.

[Dr. Jenner then passes on to examine at greater length the anatomical lesions of the bones, the consequences of this disease.]

1. *Enlargement of the Growing Ends of the Long Bones.*—[Dr. Jenner, under this head, by actual measurements in several cases, shows that the enlargement of the ends of the bones is actual, and not apparent only, as asserted by some.]

As to the intimate anatomical structure of the large ends of the bones: there is excessive formation of the structures which precede or form the nidus for ossification, there is retardation and incomplete performance of the process of ossification. In the healthy child the ends of the long bones measure more in circumference than the shafts, as the process of ossification is completed, the bone diminishes in girth.

In rickets there is an exaggeration of the condition we find in the first stages of ossification in the healthy subject, the completion of the process only is stayed. There is great development of the spongy tissue of the head of the bone, and of the epiphyses, and also of that layer of cartilage in which the primary deposit of calcareous matter takes place.

The layer of cartilage in which the cells are arranged in linear series instead of being half-a-line, is from a quarter to half-an-inch in breadth.

Again, the calcareous granular deposit is wanting at the boundary of ossification, and there the cartilage-cells calcify before the matrix; the consequence of this is that you see the cartilage-cells, being apparently converted into lacunæ and imperfect canaliculi.

You may easily trace all stages in the deposit of calcareous matter on the inside of the cells, from that in which it forms a mere ring to that in which it is so thick as to leave only a vacant space resembling an almost perfect lacuna. Köllikur has generalised from his observations on ricketty bone, thinking that in rickets the normal process by which the lacunæ are formed is visible. My observations lead me to quite another conclusion—the calcification of the cartilage-cells in the growing cartilage in rickets seems to me identical with the calcification of the same parts occasionally seen in enchondromata. It is a pathological process, a petrefaction. The spongy tissue is much more spongy in appearance than natural, and from the interstices of its meshes a deep red pulp is expressible. This pulp is composed of colourless nucleated cells usually containing only one nucleus, now and then two, and occasionally several blood-globules, and in some cases a very large quantity of free fluid fat. If, as Sharpy, Somes, and De Morgan have supposed, these cells play an important part in the completion of the process of ossification, and I am—from my own observation on ricketty bones, &c.—inclined to think they do, we see in their abundance in ricketty bones only a further evidence that in ricketty bone there is excessive preparation for the process of ossification and arrest of the completion of the process. The periosteum is thickened over the head of the bone as over the bone generally. It attains its maximum degree of thickening just at the point of junction of the bone with the cartilage.

The periosteum of the whole bone is often more vascular and thicker than natural, but in the many post-mortem examinations of extreme rickets which I have made, I have never seen any bloody fluid as described by Guerin beneath that membrane. A crimson pulp fills the canal, and all the interstices of the tissues of the long bones. It is composed of elements identical in appearance with those expressible from the spongy tissue of the head of the bones. In this pulp, however, I have never observed any such quantity of free fluid fat, as I have noted in the heads of the bones near the line of progressing ossification.

2. The softening of the bones is sometimes so great that the bones which in their healthy condition are the strongest, may be bent by the most trifling force, and those which naturally are the thickest may be cut by a knife with facility.

This softening of the bones is chiefly the consequence of the diminution of their earthy salts. It seems, however, from the experiments of Lehmann and Marchand, that the animal matter of ricketty bones differs, in some cases, from that of healthy bone; for, in some of their experiments, the bones yielded no gelatin on boiling.

Putting together the results of the analyses of several observers, we gather that the bones of healthy children yield about 37 parts of organic and 63 of inorganic matters; and that those of ricketty children yield about 79 parts of organic and 21 of inorganic matters.

3. The thickening of the flat bones is the consequence of thickening and increased vascularity of the periosteum, and of the abundance of the nucleated cells, which, with blood from the pulp, which occupies all the meshes of the bone. The thickening is usually greatest just within or at the growing margin of the bone, so that in the cranial bones it is greatest near to or at the sutures, and least at the centres of ossification.

4. *Deformities which follow from Pressure on the Softened Bones.*—The spine is bent; the cervical anterior curve is increased; the face is directed upwards; and the head falls backwards. This curvature is only strongly marked when the muscular debility of rickets (of which I shall speak hereafter) is very decided. The vertebræ being softened and the muscles weakened, the head is no longer supported, and it falls forwards or backwards as circumstances may determine. Usually the child favours the falling of the head backwards, in order that it may see what is going on around, just as when suffering from paralysis of the third nerves, the child sits or walks with the head thrown backwards, in order that it may see under the fallen upper eye-lids. There is a posterior curvature of the spine, if the child is unable to walk, commencing at the first dorsal and extending to the last lumbar vertebra. If the child walks, then the posterior curvature is limited to the dorsal region, and there is an anterior curvature in the lumbar region. This posterior curvature in the child yet in arms, is sometimes so extreme that it may be easily mistaken (and I have known it mistaken) for angular curvature. They are distinguished thus:—

If the child be held by the upper part of its trunk, the weight of the lower limbs will usually remove the ricketty curve, and it may certainly be straightened if the nurse hold the child by the upper part of the trunk and the physician raises the lower limb with one hand and at the same time places the other on the curved spine.

This curvature of the dorsal and lumbar spine in rickets is the consequence of the muscular weakness, and softening of the bodies of the vertebra; its direction is determined by the weight of the head, etc. It is an exaggeration merely of the curvature always existing when the child of three or four months old is sitting unsupported on the nurse's arm. Lateral curvatures in the young child are less common than the antero-posterior. Their direction is determined by the position accidentally assumed by the child. For example. if the child be carried on the left arm constantly, there is a disposition to lateral curvature, and the convexity of the curve will be towards the left.

The femur is curved forwards and outwards. This curvature is produced before the child walks, by the weight of the legs and feet. The child sits on its mother's lap, or on a chair, and the lower extremities hang pendant. The bone being flexible, yields. After the child walks, the weight of the trunk is the chief agent in determining the curvature of the femur. The curve which existed before walking will be exaggerated.

When the tibia curves before the child walks or has been placed on its feet, the curvature is almost always outwards—an exaggeration only of the normal curve in the young child, and is produced by the child sitting somewhat cross-legged, and bearing on the floor or bed with the outer malleolus. After walking, the weight of the trunk is the chief agent in determining the bending of the tibia, and the direction of the curve will depend on the circumstances which determine the point on which the chief amount of pressure is brought to bear.

The curvature of the ulna and radius has been attributed to muscular action—to the child placing its arm around the breast of the mother, etc.; these causes are altogether inefficient for its production. The curvature of the bones of the forearm is produced by the child, owing to want of muscular power to support itself in the sitting posture, throwing part of its weight on to its arms. The child places its open hands on the chair, bed, or floor, and throws a large share of the weight of its trunk on to the bones of the upper extremities; the bones of the forearm are twisted as well as curved outwards.

The humerus is sometimes bent at an angle just where the deltoid is inserted. This curve is produced by the weight of the arm when the limb is raised by the action of the deltoid, and is increased by the cause which determines curvature of the forearm.

The clavicles are often the subject of extreme angular curvature. The chief bend is always at the same spot, viz., just outside the part to which the sterno-cleido-mastoideus, and the pectoral muscles are attached. The second bend is about half-an-inch from its scapular articulation. The first curve is forwards, and somewhat upwards; the second backwards. The curvature of the clavicle is produced partly by the weight of the arm on the humeral end of the clavicle—the sternal end being supported by the muscles just mentioned, and by its ligaments—but chiefly by the force brought to bear on it, when the the weight of the trunk is thrown on to the upper extremities, the child being in the sitting posture, with the hands on the floor or crawling.

The deformity of the greatest interest to the Physician is that of the thorax. The back is flattened. The ribs are bent at an acute angle where the dorsal and lateral regions unite. At that part the lateral diameter of the thorax is the greatest. From it, the ribs pass forwards and inwards to the point where they unite with their cartilages; on that line the lateral diameter of the thorax is the least, the cartilages curving outwards before turning in to unite themselves to the sternum. The sternum is thrown forwards, and the antero-posterior diameter of the thorax is abnormally great. The consequence of the direction of the ribs being inwards, and of the cartilages outwards, is, that the thorax is grooved from above downwards on its antero-lateral from the 1st to the 9th or 10th rib; the deepest part of the furrow being just outside the nodes formed where the ribs and cartilages unite. This groove extends lower on the left than on the right side, but it is deeper on the 5th and 6th ribs on the right than on the left side; the heart and the liver respectively supporting, to some extent, their corresponding ribs. The points of maximum recession correspond to the 5th, 6th, and 7th ribs. A little below the level of the nipple the chest expands considerably, the chest-walls being borne outwards by the liver, stomach, and spleen. If we examine the thoracic walls from the inside, the appearance is most remarkable, where the ribs join with the cartilages there are much greater projections than on the outside; but the 11th and 12th ribs, which are not inflexed, have the same enlargement on the inside as on the outside.

The great determining cause of the thoracic deformity is atmospheric pressure; this is aided by the elasticity of the lungs. How is the pressure of the atmosphere brought to bear on the thoracic parietes? Suppose the thoracic external parietes were made of cast iron or any other unyielding material, then the diaphragm could descend only so fast as the air could enter at the orifice of the larynx, and overcome the elasticity of the lungs. The thoracic parietes, however, in their normal condition, are not absolutely unyielding; but then there is a due relation between their strength, the power of the diaphragm and the rapidity of its contractions, the size of the orifice of the larynx, and the elasticity of the lungs. The chest-walls being healthy, and the orifice of the larynx of normal size, if the young child sobs violently—*i. e.* contracts the diaphragm with abnormal rapidity and force—the most flexible parts of the thoracic parietes will fall in during inspiration.

If the orifice of the larynx be narrowed, and if the diaphragm contract with only normal rapidity and force, there will be recession of the softer parts of the chest-walls at each inspiration.

Again, if the orifice of the larynx remain normal, the diaphragm act as energetically as in a healthy child, and the chest-walls be softened, then, at each inspiration, there will be recession of the most yielding part of the thoracic walls.

It is this last condition which exists in the ricketty child. The part of the rib where ossification is imperfect and incomplete is so soft that, at each descent of the diaphragm it recedes, and the furrow of which I have spoken is the consequence. Just in proportion as the ends of the ribs are forced inwards the sternum is carried forward, and the consequence is the remarkable form of thorax of which a model is on the table, of which some examples are now in the Hospital, and of which it is rare to pass a day without seeing cases in the out-patient's room. Rokitansky has maintained that this deformity of the thorax is the consequence of the want of power in the inspiratory muscles. I have repeatedly dissected subjects in which this deformity was strongly marked, and find that there is no correspondence between the points of insertion of the muscles of inspiration attached to the outer surface of the chest-walls and the points of recession. The preparations on the table bear out my assertion. Weakening of these muscles would have as its consequence general want of expansion of the lungs, and the thorax would have the form that it obtains in senile atrophy—viz., long, with narrow antero-posterior and lateral diameters.

Again, the diaphragm is said to cause the circular recession by its direct action—by drawing in the receding parts at each contraction. On the table are some dissections, which prove, when compared with the cases and the model, that the line of recession does not correspond to the points of attachment to the diaphragm. But it does correspond to the upper margin of the liver, spleen, and stomach; and is produced—as the longitudinal furrow is—by atmospheric pressure: the parts of the parietes below being prevented receding by the organs I have just mentioned. The influence of the organs beneath in preventing the recession of the chest-walls is illustrated by the *apparent* bulging of the precordial region in every case of well-marked ricketty thorax. The chest-walls covering the heart do not recede so much as on the opposite side, and the consequence is that the left side is much fuller than the right; and, at first sight it might be supposed that there was abnormal fulness of the precordial region.

In excluding muscular action from all direct share in the production of curvature of the long bones in rickets, I am, so far as I know, unsupported by any authority.—*Med. Times and Gazette, March* 17, 1860, *p.* 261.

123.—ON HEMORRHAGE FROM THE BOWEL IN CHILDREN AS A SIGN OF POLYPUS OF THE RECTUM.

By Thomas Bryant, Esq., F.R.C.S., Assistant-Surgeon to Guy's Hospital.

[Polypus of the rectum is usually considered a rare affection; but within the last two years at least twenty examples have fallen under the author's notice. The connexion between hemorrhage from the bowel in children, and polypus of the rectum, is remarkably constant. Mr. Bryant says:]

In the majority of the cases which I have observed, the disease has existed for many months; they have all occurred in children under ten years, and in most the disease has been regarded and treated for piles. In some cases the discharge of blood from the bowel is constant, and its buttocks smeared with a bloody mucus. In these instances the polypus will generally be found to be within, if not protruding from, the sphincter. In other examples, occasional discharges of blood will be observed, although not to any very great extent, and this discharge will generally accompany and follow the act of defecation. In others, again, the hemorrhage willl take place independently of any such process. There will generally be some straining after stool, but I have never observed any prolapse of the rectum; and although this disease is troublesome to the child, and of course debilitating, by the repeated, if not constant hemorrhage, when once recognised it is easily treated, and rapidly cured.

The recollection that such a disease is not so uncommon, and that it is always associated with hemorrhage from the bowel, should at once lead the surgeon to examine the part with his finger, when the growth will probably be easily detected. It is generally situated about one or two inches up the bowel, and will be found to vary from the size of a pea to that of a large nut. In some cases more than one will be present; they are always very moveable, and easily slip away from the finger upon anything like pressure; and at times some little difficulty is experienced in fixing them for removal. Such a practice is the only correct statement, being invariably followed by a successful result. It may be done by means of forceps or ligature; and in many cases I have broken the polypus off its attachment by hooking my finger round its pedicle. No bad result has ever followed. The pedicle is always very slender, although it may be an inch or more in length.

The structure of the polypus is very simple, microscopically presenting the ordinary characters of the fibro-cellular growths. After removal no subsequent treatment is required, and recovery may confidently be expected; the rectum, however, should be carefully examined, so that a second polypus be not overlooked.

I have thus briefly brought this small but not unimportant subject before the notice of the profession, feeling confident that the existence of polypus of the rectum is not so uncommon as is generally believed, and that such an affection is usually mistaken and treated for piles. In children, the presence of bleeding from the rectum should at once lead the practitioner to suspect the existence of polypus; and, when detected, its removal is the only correct treatment. I have never had an opportunity of seeing a child suffering from piles, and believe that cases so described are generally mistaken, and that, in reality, they are cases of the disease now under consideration. —*Lancet, Nov.* 26, 1859, *p.* 530.

MISCELLANEOUS SUBJECTS.

134.—A TEMPERATE EXAMINATION OF HOMŒOPATHY.

No. III.—On the Statistics of Homœopathy.

By W. Braithwaite, Esq., Leeds.

In this pamphlet we shall endeavour to show how the practitioners of homœopathy have endeavoured to mislead the public with respect to the success of the regular practitioners of medicine. And, by-the-bye, we wish to make the distinction clear between the practitioner of homœopathy and the practitioner of medicine. We do not consider that the homœopathic practitioner is a practitioner of medicine at all, in the strict sense of the word. He only practises a particular creed, differing altogether from the general belief of the medical profession. We are borne out in this interpretation by Mr. W. T. Harding, the Registrar of New Brunswick, under the New Medical Act. It appears that the homœopaths are delighted to have a college of their own. It is proper that they should, and we hope they will immediately desist using and priding themselves on degrees obtained in the regular colleges of medicine in this country, and obtained by answering questions in a way which many at the time disbelieved, and which others have since repudiated. It is a pity they still wear honours thus obtained. The college of Cleveland, New Brunswick, is the college to which we hope the homœopath will in future resort. But Mr. Harding says that this is not strictly a medical college, but only a homœopathic college; and that the degree is not a medical degree, but only a homœopathic degree. He says that although a man from this college may be, and is called a Doctor of Medicine, "the degree is a homœopathic degree, and not a medical one. As well might a veterinary surgeon claim to be registered because his diploma contains the word 'surgeon.'" Mr. Harding adds, "I think from what I can gather, that should the matter be brought to trial, the law will sustain the view that a homœopathic degree is not a medical degree in the sense of the terms of our act." *(Lancet, Oct.* 8, 1859, *p.* 368.)

Before placing before the reader the various statistical tables which we have collected from various writers, we will give one published in Leeds by a homœopathic practitioner, and circulated widely, both privately and by post, to show the extraordinary success of his own mode of practice, when compared with that of his neighbours. When we first saw this table at the end of his pamphlet, we thought it resembled some of the advertisements in the newspapers, such as Du Barry's method of curing 50,000 cases without any medicine at all, and we could not but feel sorry that any individual should take such a step, when it did not at all refer to his arguments. This table is as follows (*Dr. Craig's Pamphlet, p.* 24):—

"PER CENT OF DEATHS IN ACUTE DISEASES.

Disease.	Homœopathic.	Allopathic.
Inflammation of the Lungs	5.7	24
Pleurisy,	3.	13
Inflammation of the Bowels,	4.	13
Dysentery,	3.	22
Typhus, (Abdominal),	15.	19
All Diseases,	5·8	12·4"

We may look upon this table as a fair specimen of the way in which the public has been misled for some years; and we will now endeavour to throw some light on its fallacy.*

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* In the compiling of this pamphlet we are indebted chiefly to Dr. Routh's work on the "Fallacies of Homœopathy"; Dr. Simpson's work on "Homœopathy"; Mr. Thomson on the "Statistics

We consider this and other tables published by homœopathists, with the view of throwing discredit on the regular practice of medicine by surgeons and physicians, are highly discreditable, and ought to be disowned by the more honourable of this sect. How can their tables be true, even without any more evidence of their fallacy, than the following respecting small-pox? Perhaps few diseases have ever been so fatal as this disease before vaccination was discovered. We may consider that almost every case was a dangerous one; and yet out of every hundred cases within the past century, 80 would, on an average, recover. The following table will show this:—

Observer.	Place of Observation.	Per centage of Deaths.	Years.
Haygarth,	London,	16	1759—1768
Watt,	Glasgow,	16·82	1783—1800
Dobson,	Liverpool,	18	1772—1774
Percival,	Manchester,	15	1769—1774
Nettleton,	11 towns in Yorkshire,	19	
Jurin,		18	
Heberden,	London,	8½–9	

(*Edin. Monthly Journal, Feb.*, 1860, *p.* 713).

We thus find that, in a disease of the most fatal description, less than 20 per cent. died in large towns; and at one period, calculated by Dr. Heberden, less than 10 per cent. So that if every one were attacked by small-pox instead of the common diseases of mankind, more would recover than what the homœopaths state as the recoveries by means of regular practitioners! This is so ridiculous, that it throws discredit on all their other statements.

After reading a good deal about the statistics of diseases, treated both by regular physicians and surgeons, and by homœopathic practitioners, we come to the conclusion that simple *numbers* of cures of cases are no criterion whatever of success in practice. We could prove that one physician who lost twenty-five cases out of every hundred which applied to him, was much more successful than another who lost only six. This can be easily shown to be possible. For example, one physician, celebrated in his town and neighbourhood, has passed a long career of professional success—a hundred patients apply to him, all *very severe cases, most of them likely to die*, and yet he is the means of curing 75 out of these severe cases. His death-rate in fact is 25 per cent. His apparent large death-rate is in fact owing to his severe cases, and to his celebrity and success. Another physician, known only in his own street or little neighbourhood, has likewise his hundred cases which apply to him in the course of a year. These are all mild cases. They may have the same *names* as the severe cases of the other physician, such as pneumonia, bronchitis, &c., but there was no likelihood, if proper treatment had been adopted, that *any* would die, and yet six out of the hundred die. The first physician, therefore, saves 75 bad cases out of his 100. The second physician loses 6 cases, when, in fact, he ought to have lost none; he not only did not save a single bad case, but he lost 6 cases, which, in all probability, the first physician would have saved—so that the first physician, losing 25 per cent., was much more successful than the second, who lost only 6 per cent. Mere *numbers*, therefore, can never prove success in practice.

This remark is applicable both to individual physicians and to hospitals. Success is proved by curing bad cases, and not those which would generally cure themselves in time. In calculating the death-rate in large metropolitan hospitals like Guy's, St. Thomas's, and Bartholomew's, in London, and the vast hospitals of Vienna and Paris, it ought to be remembered that they are often like immense poor-houses—the receptacles of aged and infirm persons, and most incurable cases. The splendid revenues of the London hospitals are well spent in thus relieving the worst cases which can afflict mankind. The hospitals of the Continent are generally paid by the state in which they

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of Hospitals"· Dr. W. T. Gardner's papers on "Homœopathy"; and Dr. John Taylor's "Cholera Report", and various journals, all of which we have endeavoured to acknowledge in due form and place.

are situated. The *beds* provided for the sick and aged alone, in Paris, for this year (1860) are as follow:—

For the sick, ...	7,172
For the infirm and old, ...	7,838
	15,010

When we think that in Paris alone above fifteen thousand beds are prepared this year for the sick and aged, we may easily imagine that in such a population a large death-rate may take place without any reflection on the skill and humanity of about 235 physicians and surgeons who are employed. (*Med. Times and Gaz., March* 10, 1860, *p.* 238.) Now we think that we shall be able to prove that the cases admitted into the hospitals of large cities as London, Paris, Vienna, &c., are much more severe than those admitted into homœopathic hospitals, and yet, taking the case altogether, that the death-rate is remarkably low. Our object, however, in these pages, is not so much to prove the success of regular practitioners of medicine as the fallacy of homœopathic statistics. When we find homœopaths publishing their statistical tables, and circulating them through the country in a boastful spirit, depending simply on numbers, and not on the severity of the cases, we might feel disposed to turn to the columns of the newspaper where Morrison and such like men advertise their cures, to see if these said homœopathic tables are not advertised. We find them not there, but in small pamphlets, or at the end of a pamphlet, placed in such a position that they appear to great advantage when compared with the tables published by large hospitals. Such a proceeding, however, is neither honourable nor dignified. It appears too much like quackery to meet with the approval of right-judging men.

We propose to show from tables to which the reader can refer for himself:—

1st. That hospitals of large cities, as London, Paris, Vienna, &c., have a large mortality, some more, some less, and variable from year to year, *owing to the great severity of the diseases admitted.*

2nd. That homœopathic hospitals admit a large proportion of mild cases, such as would never be admitted into regular hospitals, and yet have a large death-rate, amounting almost to the death-rate of the large hospitals of London, and a much larger death-rate than such cases ought to show.

3rd. That the provincial hospitals of England, including all kinds of cases, accidents of all kinds, and surgical operations, have a much smaller death-rate than can be shown by homœopathic hospitals.

4th That the army and navy reports (the most correct that can be given,) show a *much less* death-rate than homœopathic hospitals.

5th. That homœopathic statistics, even with respect to numbers, are not to be trusted, as some are known to have been *cooked*, and are here referred to in proof of this accusation.

In some of the calculations the numbers vary in the same hospital, and may thus seem to be contradictory, but this will be owing to the death-rate belonging to different years.

First, then, we admit that in the larger hospitals of London, Paris, Vienna, and other large cities, the death rate is larger than in homœopathic hospitals, yet this is no proof of the success of homœopathic treatment, but only shows the severity of the diseases admitted.

There is no single individual in this country more esteemed and beloved than Miss Nightingale. What does she say on this subject in her book just published? *

"No one, I think, who brings ordinary powers of observation to bear on the sick and maimed can fail to observe a remarkable difference in the aspect of cases, in their duration and in their termination in different hospitals. To the superficial observer there are two things only apparent—the disease and the remedial treatment, medical or surgical. It requires a considerable amount of experience, in hospitals of various constructions and varied administrations, to go beyond this, and to be able to perceive that conditions arising out of these have a very powerful effect indeed upon the ultimate issue of cases which pass through the wards.

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* "Notes on Hospitals," &c.

"It is sometimes asserted that there is no such striking difference in the mortality of different hospitals as one would be led to infer from their great apparent difference in sanitary condition. There is, undoubtedly, some difficulty in arriving at correct statistical comparison to exhibit this. For, in the first place, different hospitals receive very different proportions of the same class of diseases. The ages in one hospital may differ considerably from the ages in another. And the state of the cases on admission may differ very much in each hospital. These elements, no doubt, affect considerably the results of treatment, altogether apart from the sanitary state of hospitals.

"In the next place, accurate hospital statistics are much more rare than is generally imagined, and at the best they only give the mortality which has taken place *in* the hospitals, and take no cognisance of those cases which are discharged in a hopeless condition, to a much greater extent from some hospitals than from others.

"We have known incurable cases discharged from one hospital to which the deaths ought to have been accounted, and received into another hospital, to die there in a day or two after admission.

"Again, the sanitary state of any hospital ought not to be inferred solely from the greater or less mortality. If the function of a hospital were to kill the sick, statistical comparisons of this nature would be admissible. As, however, its proper function is to restore the sick to health as speedily as possible, the elements which really give information as to whether this is done or not, are those which show the proportion of sick restored to health, and the average time which has been required for this object; a hospital which restored all its sick to health after an average of six months' treatment, could not be considered as by any means so healthy as a hospital which returned all its sick recovered in as many weeks. The proportion of recoveries, the proportion of deaths, and the average time in hospital, must all be taken into account in discussions of this nature, as well as the character of the cases and the proportion of different ages among the sick.

"Hospital mortality statistics give little information on the point, because there are elements in existence of which such statistics take no cognisance. In one set of metropolitan hospitals, for example, I find the mortality about two and a half per cent. upon the cases treated, while in other metropolitan hospitals the deaths reach from about twelve to sixteen per cent. To judge by the mortality in these cases would be most fallacious. Because in the first class of hospitals every ailment, however slight, constitutes a title to hospital admission, while, in the latter class of hospitals, special diseases only, at all times accompanied by a high rate of mortality, are admitted. Hence the duration of the cases admitted, and the general course and aspect of disease, afford important criteria whereby to judge of the healthiness or unhealthiness of any hospital in addition to that afforded by the mortality statistics." (*Edinburgh Med. Journal*, *Nov.* 1859, *p.* 451.)

In another part of her book Miss Nightingale says:—

"I have heard a doctor condemned whose patient did not, alas! recover, because another doctor's patient, of *different sex*, of a *different* age, recovered from a *different* disease in a *different* place. Yes; this is really true. If people who make these comparisons did but know (only they do not care to know) the care and preciseness with which such comparisons require to be made (and are made) in order to be of any value whatever, they would spare their tongues. In comparing the deaths of one hospital with those of another, any statistics are justly considered abosolutely valueless which do not give the ages, the sexes, and the diseases of all the cases. It does not seem necessary to mention this. It does not seem necessary to say that there can be no comparison between old men with dropsies and young men with consumption, Yet the cleverest men and the cleverest women are often heard making such comparisons, ignoring entirely sex, age, disease, place; in fact, all the conditions essential to the question. It is the merest *gossip*."

In another place this eminent lady gives her opinion of homœopathy in the following words:—

"Homœopathy has introduced one essential amelioration in the practice of physic by amateur females; for its rules are excellent, its physicking comparatively harmless—the 'globule' is the one grain of folly which appears to be necessary to make any good thing acceptable. Let then women, if they will give medicine, give homœopathic medicine. It won't do any harm. (*Edinburgh Med. Journal, March* 1860, *p*. 847.)

These remarks of Miss Nightingale fully bear out what we have been stating, and show the utter fallacy of mere numerical statistics as proving success in treatment.

The opinion of Dr. W. Stone, medical registrar of St. Thomas's Hospital, is much to the same effect. Dr. Stone says:—"Thus it comes about, that wherever a Hospital is in the greatest demand, its occupants will be most dangerously ill, and its death-rate the highest proportionally to its admissions. A hospital death rate is thus not a measure of the cures performed, or of its internal healthiness, so much as of the number and intensity of cases of serious diseases in its district, and of the general anxiety among the lower classes to avail themselves of its benefits. It is physically what the House of Correction is morally—the '*sentina reipublicæ*,'—and in the present state of society, the more foul its contents, the more efficiently it is doing its duty.

"Indeed, largeness of death-rate, so far from being a sign of defective administration, or of bad sanitary condition in a hospital, on the medical side at least, is of quite an opposite import. This is exemplified in St. Thomas' by what are called the Clinical Wards, to which are brought at all hours, cases marked by the greatest urgency, requiring the closest attention, and affording the best school of instruction for the pupils. The death-rate in these wards is high, although it is obvious that in them more critical points of practice occur, and more real cures are brought about than can be attained in wards full of chronic disease, where the death-rate is much lower. But a far stronger evidence is found in the examination of death rates in specific diseases. For this purpose a few have been selected from the Report for the year 1858, which I have recently had the honour of presenting to the hospital authorities, and in which every case has been verified by personal observation of the reporter, independent and corroborative of the diagnosis appended to the bed-ticket.

"From this it appears that the death-rate of typhus was 18·1 per cent.; in typhoid, 20·0 per cent.; in dilirium tremens, 17·3 per cent.; in pneumonia, 17·0 per cent.; in diarrhœa, 16·0 per cent.; in erysipelas, 7·0 per cent.; and in poisoning cases, accidental or suicidal, 17·6 per cent. All these death-rates are very high, indeed; yet no person of any experience can deny that these are, of all others, types of disease which benefit by the accurate diagnosis, bold medication, liberal dietary, abundant stimulants, and careful nursing of a hospital. Probably the chief medical triumphs of the year will be found among the cited instances; undoubtedly more lives have really been saved among these patients than among any other equal number. And yet the death-rates are some of the very highest of any on the register.

"This seems to complete the appeal from blind figures to discriminating intellect. Abstract death-rate is evidently no fair method of testing the sanitary condition of a hospital, whatever be its value in estimating the healthiness of a district. And it is always to be regretted that fallacious reasoning should be put forward in defence of an object which actually stands on more solid grounds and offers some real advantages." (*Med. Times and Gazette, Nov.* 12, 1859, *p*. 490.)

The death-rate in these large hospitals, therefore, ought to be judged according to the *severity*, and not the *numbers*, of the cases. If you wish to judge by numbers only you ought to include the milder *out*-patients, and this would reduce the death-rate to a remarkably low figure. Thus in the Leeds Infirmary, the death-rate on all cases admitted since 1767, is only about 3·2 per cent, and in the Leeds Dispensary, 4·02.

Take also the statistics of Guy's Hospital for any one year, that for 1854, for example, which is now before us. We find the following numbers referring to in-patients.

Rate of mortality over all the cases (during the year), 8·8 per cent., including both surgical and medical.

Rate of mortality in medical cases, 5·9.—(But this is wonderfully reduced when we add the 8350 milder out-patients.)

The number admitted in 1854, were	In-Patients, . . .	5089
	Out-Patients, . . .	8350
Total, ...		13,439

Of these nearly all the in-patients would be severe, at least the severe cases are picked out for admission, and the milder are admitted as out-patients. Taking both together we make the death-rate to be only 3·9 per cent.—not even calculating 21,710 cases of a more casual and slight description, which were attended to. Now when we see that out of 13,439 cases admitted into Guy's Hospital, or attended to as out-patients, and including the worst cases that can be found anywhere, only about 2·9 per cent. could be registered as having died, we are struck with the success of modern medical and surgical practice! It will be seen how nearly the success of Guy's hospital and the Leeds Infirmary correspond, when we take all the cases together. But as it can be shown that the cases admitted into regular hospitals are the *worst cases* that can be picked out from the masses which apply, so we think that it can be shown that the cases admitted into homœopathic hospitals are the *mildest* that can be selected, and yet that the death-rate is very high for such cases. It is shown in a paper by Mr. J. Thomson, on the Statistics of Civil Hospitals, that the average mortality of 30 provincial hospitals in England is 4·46 per cent. while 14 hospitals of London average 7·79 per cent.; and some, such as Addenbrook's hospital, Cambridge, has a mortality of only 2·4 per cent., and Exeter 3·2. A great deal therefore depends on the *kind* of cases and not on the number admitted. The table on the opposite page, by Mr. Thomas, will explain this point.

This table on the following page embraces a long period, and confirms the opinion that statistics of hospitals must ever vary from year to year, according to the nature of the cases. It shows, however, that considering the reception of all kinds of cases, and exclusive of all out-patients, the mortality is very low. If the milder out-patients had been included, we believe the death-rate would be extremely low.

To show how variable may be the death-rate in hospitals, compare Dr. Routh's table [next page]. (*Fallacies of Homœopathy, p.* 85.)

Year.	Place.	Admissions.	Deaths.	Mortality per cent.
1828–31	Limberg:—All Diseases.			
34–35	General Hospital ..	....	....	2 to 3
1838–39..	Göttingen Poliklinik ..	....	....	2 to 3
1837–38..	Stuttgard Catherinen	....	....	3 to 4
	Military:—			
1850 ..	Grenadier Guards ..	1,320	29	2·02
	Coldstream Guards ..	878	17	1·09
	Scots Fusileer Guards	602	11	1·08
	Royal Ordnance ..	4,977	38	0·76
	Dreadnought Ship ..	2,121	78	3·68
	France:—All Diseases.			
1837–46..	Hôtel Dieu ..	11,130	....	8·06
	Annexe Hôtel Dieu ..	5,914	....	7·06
	Pitie	10,427	....	7·01
	Charité	8,010	....	6·08

TABLE

Showing the number of patients treated, the deaths, and ratio of mortality in the following fourteen Hospitals, at different periods, from data compiled from the British Medical Almanac and Dr. Hawkins' Satistics. (*Edinburg Medical and Surgical Journal*, 1843, *page* 355.)

Hospitals.	Periods.	First.					Periods.	Second.				
		Years.	Treated.	Died.	Mortal. pr. cent.	Av. yly. Admis.		Years.	Treated.	Died.	Mortal. pr. cent.	Av. yly. Admis.
Chester General Infirmary.........	1826 to 1835	10	5331	220	4·1	533	1839 to 1841	2	1214	88	7·2	607
Royal Infirmary, Manchester.......	1752 1836	84	74360	3876	5·2	885	1836 1837	1	1890	208	11·0	1890
Kent & Canterbury Infirmary.......	1793 1826	34	15697	655	4·1	462	1827 1836	10	4239	198	4·6	423
St. Bartholomew's Hos. London....	1790 1819	30	10186	903	8·8	339	1830 1834	5	4748	378	7·9	949
St. George's Hospital.............	1830 1834	5	9500	1075	11·3	1900	1835	1	2192	226	10·3	2192
London Hospital..................	1828 1833	6	13870	1622	11·6	2311	1835 to 1836	1	2517	260	10·3	2517
Westminster Hospital............	1833 1835	3	2535	260	10·2	845	1835 1836	1	992	100	10·0	992
St. Thomas's Hospital............	1764 1813	50	——	——	6·6	——	1835 1836	1	3000	261	8·7	3000
Exeter Hospital..................	1826 1835	10	10145	326	3·2	1014	1835 1836	1	1069	35	3·2	1069
Bristol Infirmary.................	1826 1835	10	15805	1325	8·3	1580	1828	1	1689	160	9·4	1689
Gloucester Infirmary.............	1829 1835	7	4253	241	5·6	607	1835 to 1836	1	646	33	5·1	646
Bath United Hospital.............	1827	1	551	30	5·4	551	1835	1	814	47	5·7	814
Lincoln Hospital..................	1826 to 1835	10	2762	133	4·8	276	1836 to 1837	1	*320	14	·4	320
Addenbrook's Hos. Cambridge.....	1826 1835	10	6414	157	2·4	641	1832 1833	1	620	17	·7	620
Total....................			171409	10823	6·31				25950	2025	7·79	

* Out and in patients.

Year.	Place.	Admissions.	Deaths.	Mortality per cent.
1837–46..	St. Antoine ..	.. 3,469	....	8·00
	Necker ..	.. 4,769	....	8·03
	Cochin ..	.. 1,932	....	8·04
	Beaujon ..	.. 4,310	....	9·01
	Bon-Secours..	.. ..	....	5·05
1834–46..	St. Louis ..	.. 8,240	....	4·06
	Enfans Malades	.. 3,604	....	12.00
	Accouchemens	.. 7,502	....	3·01
	Midi ..	.. 3,730	....	3·05
	Lourcine ..	.. 2,083	....	2·09
	Clinique ..	.. 2,030	....	3·01

The following table of *Continental Hospitals*, compiled from Dr. Hawkins' 'Elements of Medical Statistics,' (*Edin. Med. Journal*, 1842, *p.* 356), shows the number of patients treated, the deaths, and ratio of mortality per cent. in the following twenty-six hospitals:—

Hospitals.	Periods.	Years.	Treated.	Died.	Prop. deaths to the number treated.	Ratio Mortality per cent.
					1 in	
Hotel Dieu, Paris	1770 to 1780	10	......	....	4	25.00
Do	1822	1	......	....	7	14.25
The Hospitals of Paris	1822	1	47893	....	8	12 50
Hospitals at Lyons	1822	1	......	....	11	9.00
Hospital at Montpelier	1822	1	......	....	10	10.00
The General Hospital, the Charite at Berlin	1796 to 1817	20	......	....	9	16.66
The Great Hospital at Vienna, (2000 beds)	1824	1	......	....	6	16.66
Civil Hospital, Pesth	1826	1	......	....	6	16.66
City Hospital at Dresden	1816	1	......	....	7	14.25
Hospital at Munich	1819	1	8500	....	9	11 11
Hospital at Heidelberg	1825	1	285	. ..	21	4.76
Imperial Hospital at Petersburgh	1808 to 1817	14	......	....	4.5	22.22
Do	1816	1	2043	461	4.4	22.56
The Cantonal Hosp. Pays de Vaud	1825	1	844	16	21	4 68
St. Pierre at Brussels	1828	1	......	....	9	11.11
St. Pieter's Gasthuis at Amsterdam	1798 to 1817	20	......	....	8	12 50
General Hospital at Genoa	1821	1	1 dth. in	6 reco	veries.	...
St. Giovanni at Turin	1821	1	1 dth. in	7 reco	veries.	...
The Great Hospital Milan	1823	1	18273	1901	7	14.81
San Matteo della Pieta. at Pavia	1828	1	5287	....	10.5	9 52
Hospital at Padua	1820 to 1821	1	149	10	15	6.66
——— at Bologna	1816 1819	8	453	85	13	7.78
——— at Leghorn	1818 1825	7	....	....	7¼	18.69
——— at Palermo	1823	1	4221	515	8	12.25
The two principal Hospitals at Madrid	1814	1	15230	1187	13	7.80
Do	1818	1	14500	1283	11	8 85
Hospital at Valencia	1786	1	4800	689	7¼	13.81
The Hospital Santa Cruz, Barcelona	1821 and 1822	2	8205	1293	6¼	15.75

At one time, therefore, the death-rate in hospitals may be very large and at another time small, owing entirely to the nature of the cases. Now it appears at first sight that the death-rate in these hospitals is large, and especially in continental hospitals. But we maintain,

Secondly.—That the cases admitted into homœopathic hospitals are much milder than those admitted into regular hospitals, and yet have a larger death-rate than we find in English provincial hospitals.

If the reader will take the trouble to refer to an admirable letter of Dr. Gairdner's, in the Medical Times, for 1852, page 340, he will find this subject very candidly discussed. Dr. Gairdner takes the number of patients admitted into Fleischman's hospital, Vienna, during 8 years, from 1835 to 1843, and compares these cases with nearly the same numbers admitted into the Edinburgh infirmary in 2 years, viz., 1842–3. In the Vienna hospital were admitted 6,501; of these 407 died, a mortality of 6·26 per cent.: while in the Edinburgh hospital 6,369 cases were admitted, and 758 died, the mortality being thus 11·9 per cent. How is this, it will be asked, that the Edinburgh hospital lost nearly double the number of cases? We will proceed to show the reason, and a more satisfactory one cannot be given, that mere *numbers* are fallacious. Out of six of the most fatal diseases which can attack the human body, let us compare those which were admitted into these two hospitals.

	Edinburgh.	Vienna Homœopathic.
Consumption - - -	276	98
Heart Disease (organic)	159	15
Bright's disease of kidney	82	0 (!)
Paralysis - - -	103	5
Apoplexy - - -	14	9
Disease of Liver (organic)	33	1
	667	128

As Dr. Gairdner says, "all commentary upon this is unnecessary!"

"In order, however," says Dr. G., to make the conclusions which legitimately spring from this investigation more completely irresistible, I have had the curiosity to make a few further selections from the returns. I give below the remaining instances in which the Edinburgh proportion of cases materially exceeds the homœopathic. As if by magic, they turn out to be, with but two exceptions, the most intractable enemies with which the practitioner has to deal. They are as follows:

	Edinburgh.	Vienna Homœopathic.
Neuralgia	14	0
Internal Aneurism	18	1
Diabetes Mellitus	17	0
Amaurosis	15	0
Caries and Necrosis	57	5
Malignant Tumours	55	0
Other Tumours	36	0
Bronchitis (acute)	118	15
Rheumatism (acute and chronic)	343	188

"Of these last items, I have more to say immediately; meantime, by way of contrast, and to show, if possible, still more conclusively the principle on which homæopathic cases are selected for treatment, I shall reverse the picture, and give a list of the diseases which predominate to a large extent in Fleischmann's hospital.

	Vienna Homœopathic.	Edinburgh.
Chlorosis and amenorrhœa	90	48
Cholera	24	2
Colic	45	10
Diarrhœa	114	28
Dysentery	44	16
Erysipelas and Erythema	212	82
Gout	140	0
Hæmoptysis	50	8
Headache	61	37
Herpes	20	1
Inflammation of brain	17	8
Endocarditis	29	(!)
Pneumonia	300	83
Pleuritis	224	32
Peritonitis	105	19
Sore throat	301	34
Influenza	52	0
Chicken Pox	110	2

"The predominance of influenza is evidently owing to the period embraced by the returns having included an epidemic visitation of this disease. Possibly the number of cases of cholera, diarrhœa, and dysentery, (or what may have passed under the latter name), and of erysipelas, may have a similar explanation. But what is the rest of this list, which forms the staple of the homœpathic experiment? Is it not composed, without an exception, of the *curable*, often of the *easily and constantly curable* diseases of the economy? Nay, is it not plain to the most ordinary allowance of common sense, that cases have been admitted by dozens, probably by hundreds, for no other purpose than to contribute to the success of the experiment, and to swell the triumph of homœpathy? I cannot imagine to what purpose else we have 300 cases of sore-throat, and 20 of herpes, diseases which are rarely, except in the most special cases, admitted into any of our great hospitals in this country, on account of the pressure of the more severe and fatal diseases to which, as shown above, our doors are thrown open, while our experimentalists turn their backs on them, or at least give them the cold shoulder! To be sure they are ugly subjects for curative experiments, these same phthisical cases, and organic diseases of heart, liver, and kidney; and, whatever one may think of the honesty, no one can doubt, the prudence of giving the preference to sore-throats and shingles, as well as to catarrh, dyspepsia, colic, headache, and a host of the minor ills which will be found to be numerically strong in the returns.

"And now I assert, without fear of contradiction, that the homœpathic returns are not only void of triumph to the system, but that they cover it with disgrace. With such a selection of cases as I have shown above, I maintain they ought to have reduced their mortality to a far lower point than they have done. It is of no use to quote alleged cures of pneumonia or pleurisy, and to demand comparisons with 'the best hospital physicians who use allopathic remedies.' I think we are justified in believing that the cases of individual disease, like the general returns, are a sham and a fraud; and that the contrast between pneumonia at a homœpathic hospital, and pneumonia in the Edinburgh Infirmary, would be, if we could get to the root of the matter, as great as between the general lists in the one and the other hospital. Every one who has gone about the wards of an hospital in search of crepitant râles and dulness on percussion, knows that there is nothing so easy to find or so often cured as the slighter degrees of what may be technically called pneumonia; and as to pleuritis, if we may treat the evidence of *post-mortem* examination, its simpler forms must be of immense frequency; so that if our scrupulous experimentalists choose to place everything which we commonly term rheumatic stitch under that convenient and formidable-looking designation, it would not be easy to prove them wrong. They have, however, betrayed themselves in one point;—in giving the cipher of 300 to pneumonia, and only 15 to the far more frequent disease, bronchitis, they have committed what, according to Napoleon, is 'worse than a crime—a blunder;' showing that it requires a more adroit management than even that of our experimentalists, to manufacture statistics of plausible and serious aspect from the miniature types of disease by which they (very judiciously) think proper to test the efficacy of their system.

"I feel that it is useless to enter into any further details as to this statistical fraud. It is, I hope, abundantly evident, that, even supposing the numbers to be correctly stated, and the docketting of the cases to have been free from objection, the character of them, as reported, is such as to imply selection; and, on the other hand, it is next to certain, that no dependence whatever can be placed on the statements of the reports, in regard to the nomenclature of the diseases. We have, therefore, only to deal with the fact, that an hospital in which there is reason to think that the vast majority of he cases were of the most trivial description, has a mortality of 6·26 per cent; and that the interested partizans of the system therein pursued, demand for this result the palm of an unquestionable superiority, or (in the words of one of their leaders), proclaim it 'far beyond the reach of any known method of treatment.' To this it is enough to answer, that many hospitals in England have an averaged mortality much below that above mentioned; I have already instanced three of these, (Canterbury, 2·0 per cent; Cambridge, 2·4;

Exeter, 2·3.) I might add, that of eight district general hospitals in Scotland, noticed in the article by Mr. Thomson, already referred to, three have a smaller mortality than the homœopathic institution, viz., Dundee, 5·10 per cent.; Aberdeen, 4·66 per cent.; and Inverness, 4·36 per cent. All of these hospitals are, like that of Edinburgh, recipients of a considerable proportion of incurable cases, and I do not believe that any of them admit 5 per cent. of cases of cynanche tonsillaris. I am not so well acquainted with the class of cases admitted into English provincial hospitals; but in a list of thirty of these institutions in the paper above referred to (from materials in the *British Almanack* for 1836–7,) *there are only two whose mortality is not less* than that of the Homœpathic Hospital of Vienna, and the average mortality of the whole thirty (4·46) is less by nearly a third. So that the unprecedented success of homœopathic treatment is not only a very ordinary and moderate success as compared with hospitals in general, but as compared with hospitals of the size of Fleischmann's (fifty beds) it would be found to be a positive failure; and doubly, trebly a failure, when we take into consideration all the facts revealed in the preceding part of this letter.

"Before concluding, I cannot resist alluding to one other subject,—I mean the proportion of cures. In the record of a death, it is impossible to show any bias, or in any way to deviate from accuracy without gross falsehood, with correspondingly great risk of detection. But in the column of cures in this hospital may be read the character of the whole of its records. The alleged cures in the Vienna Homœopathic Hospital are 92 per cent. of the whole cases; and, as the deaths are 6·25 per cent., it follows that there is actually *scarcely any medium between death and cure!* To any one who knows what hospital cases are, or should be, this simple statement proves rather more than was intended. Compare it with the returns of any hospital which has no system to support—I choose Dumfries, simply because its mortality is identical with that of Fleischmann's Hospital:—

	Cures per Cent.	Deaths per Cent.
Fleischmann's Hospital	92·	6·26
Dumfries "	76·02	6·26

"Alas for the

'Vaulting ambition that o'erleaps itself,
And falls'

on the other side of truth and probability! In straining every nerve after this ideal and fictitious ratio of cures, Dr. Fleischmann unluckily forgot the following ugly dilemma: If, from the excellence of his art, or any other cause, he was enabled to cure 16 per cent. more than Dumfries, why was his skill not equally effective in reducing the mortality? There can be only two answers to this question, and we may give the homœopaths their choice of them. Either the cases were really curable in enormous proportion, and the homœopathic art is responsible for a mortality which may be considered, under these circumstances, quite appalling; or the alleged cures are a mockery and a delusion, inconsistent with nature and fact, and cunningly dressed up for the undiscriminating wonder of the multitude. To apply an uncharitable judgment of Dr. Fleischmann's to his own case, '*Curanter in libris—moriuntur in lectis.*'

"And now I leave the question of the results of homœopathic hospital treatment without hesitation to the judgment of 'common sense.' I only stipulate that 'common sense' will take the trouble to make herself acquainted with the facts of the case as stated and analysed above, and will protect and arm herself against sophistry and disingenuousness by an alliance with another equally useful personage, 'common honesty.'" (*Med. Times and Gazette, vol.* 25, 1852, *p.* 342.)

These statistics of Dr. Gairdner were called in question by some homœopathic writers, owing to some slight mistake. But we cannot find that Dr. Gairdner's argument is at all weakened, but rather strengthened. In another letter, entitled, "Further Remarks on Homœopathic Hospitals, being a Second Sequel to the Edinburgh Essay on Homœopathy," Dr. Gairdner very candidly corrects his clerical mistake; but, in doing so, he seems to us to have added increased force to his statistics. He says:—

"The number of cases of catarrhal disease in Fleischmann's Hospital is by no means easily discoverable from the returns. For, in the first place, 'Acute Bronchitis' is separately stated from 'Catarrh;' then 'Influenza' is distinguished from 'Bronchitis;' and 'Catarrhal Fever' from both. Besides these, there are 'Coughs' of every variety, without the least indication whether they are catarrhal or not; and a number of other even less definite items bearing on disease of the lungs; the whole of which, to prevent cavil, I transcribe below:

Catarrh, ...	44 cases.	Croup	1 case.
Acute Bronchitis	15 "	Emphysema of Lungs, ...	2 cases.
Influenza	52 "	Hæmoptysis	50 "
Catarrhal Fever	175 "	Hydrothorax	7 "
Cough (chronic)	130 "	Œdma of Lungs	14 "
Cough (spasmodic)	18 "	Spasms of the chest	3 "
Cough (not otherwise defined)	9 "	Ulcer of Lungs	44 "
Asthma	2 "		

"The above cases include *every instance* of pulmonary disease, or anything resembling pulmonary disease, excepting those hitherto accounted for, that can be fished up out of Fleischmann's grand total of 6551 cases, as I find them displayed at length in a table in Drs. Drysdale and Russell's book on Homœopathy. The homœopathic reviewer insists upon it that I shall receive the whole of the cases under the first seven headings above as being equivalent to 'Catarrh;' he accordingly adds them up, and finds, with perfect accuracy, a total of 443. I might very well take him at his word, and show that he does not thereby get rid of the anomaly which discredits the homœopathic statistics. For while catarrh in the General Hospital of Vienna, *outnumbers pleurisy and pneumonia, taken together, more than two-fold;* in the Homœopathic Institution there are only, by the calculation even of the reviewer, extreme as it is, 443 catarrhs to 524 cases of pleurisy and pneumonia. A more signal proof of the correctness of my position, when tried by the severest test which can be applied to it, I could not wish to have. But since the whole returns are now in my hands, I shall not let off my opponents so easily; but shall place them in a far more conclusively ugly situation than they have yet occupied, in their defence of these most indefensible and untruthful documents. To this end, I shall leave all the details with which we have hitherto been engaged, to shift for themselves. What 'Catarrhal Fever' may be, as distinct both from 'Influenza' and 'Catarrh,' I shall not undertake to decide; neither shall I venture to guess how many or how few of the cases of 'Cough' may be legitimately brought under the general designation of Catarrh. Perhaps it would be still more difficult to separate the cases of 'Chronic Cough' from those of 'Phthisis,' to which, no doubt, the former heading ought to contribute a considerable proportion. It is at least evident that the real number of catarrhs in Fleischmann's returns is not to be fixed without cavil or objection on the one side or other. I shall not, therefore, attempt to fix it at all, but adopt a different way of testing the question at issue.

"The reader will, I trust, recollect that the question raised by me was simply this—Do the returns justify the belief that the homœopathic cases of pneumonia and pleurisy were genuine cases; *i. e.*, like in kind and in degree to cases of pneumonia and pleurisy as observed in other hospitals? For, unless the cases be genuine and unquestionable, of course the alleged cures cannot be accepted as *real cures of the diseases alleged*—viz., of *Pneumonia and Pleurisy*. And observe, it is the homœopaths, and not their opponents, who in fact have brought about this issue, by putting forward their cures of these two formidable diseases as a great and unanswerable proof of the superiority of their system of treatment.

"Now let the reader observe carefully the principle on which the following comparison is founded. I apprehend that it completely removes all possibility of cavil, and goes at once to the very root of the matter.

"I propose to show, that in the homœpathic statistics cases of pneumonia and pleurisy abound to a degree quite incredible, and quite inconsistent with the experience of others—even in Vienna itself;—that while in ordinary hospital practice, pneumonia and pleurisy form a small minority of the cases of pulmonary disease, in the homœopathic hospital they immensely outnumber their due proportion. This will appear quite clear from the following statements.

"In the Vienna General Hospital, the diseases of the lungs and air-passages are comprised generally under five heads—Catarrhs, Emphysema, Pleurisy, Pneumonia, Tuberculosis of lungs. Dividing these diseases into two classes, of which pleurisy and pneumonia form one, and all remaining diseases the other, we arrive at the following statistical result:—

Pleurisy and Pneumonia - - - - -	1036 cases.
Catarrhs, Emphysema, and Tubercle of Lungs	3462 "

In other words, the sum of pleurisy and pneumonia, in the Vienna General Hospital, amounts to *less than one-third* of the remaining diseases of the lungs and air-passages; these diseases comprising catarrhs and emphysema, with tubercular affections of the lung.

"To compare this result with that of Fleischmann's statistics, it will be necessary to add together the diseases separately stated above. They amount, as the reader may easily find for himself, to 566 cases of miscellaneous disease, under a great variety of titles; but including all cases resembling, however remotely, those stated in the Vienna General Hospital List under catarrh and emphysema; including also probably a proportion of the tubercular cases. If the sum of the latter be now completed by adding the 98 cases given under a separate heading as 'Phthisis,' we shall have, beyond doubt, a list of all the pulmonary diseases in Fleischmann's returns, exclusive of pneumonia and pleurisy; and however little justice I may have done to my own side of the argument in this calculation, I defy the most dexterous manipulator of figures to show that the opposite side has not received the most ample consideration. What is the result?

"In the Homœopathic Hospital of Vienna, according to the preceding mode of calculation, we have

Pleurisy and Pneumonia, - - -	524 cases.
Catarrhs, Emphysema, Asthma, Phthisis, Hæmoptysis, Ulcer of Lungs, Œdema of Lungs, Coughs, &c., &c., - - - - -	644 "

So that while in the Vienna General Hospital the proportion of pleurisy and pneumonia to the remaining diseases of the lungs and air-passages is *less than one-third;* in the homœopathic hospital it amounts to *more than five-sixths!*

"To complete this view of the matter, I will shortly refer to the published statistics of the Edinburgh Royal Infirmary, during the years 1842–3, as drawn up by Dr. Peacock, and employed by me on a former occasion to test the homœopathic data. In these years I find that there were admitted—

Pleurisy and Pneumonia, - - -	122 cases.
Other diseases of the lungs and air-passages, -	643 "

In other words, the cases of pleurisy and pneumonia conjointly amount to a considerably smaller proportion than in the Vienna General Hospital; or *rather less than one-fifth* of the remaining diseases.

"And to show that this is not the accident of an epidemic season, I may refer to the last Report of the Vienna General Hospital, which I received only a few days ago. By consulting a chart at the beginning of that report, I find that pneumonia has on the whole rather declined in frequency since 1852; but that it has maintained a tolerably equable level during the years 1853 to 1855; over the last of which the report extends in detail. During the year 1855 we have

Pneumonia and Pleurisy, - - -	1064 cases.
Other diseases of the lungs and air-passages, -	3498 "

In other words, the proportion of pneumonia and pleurisy to the other diseases mentioned amounts, as before, to *somewhat less than one-third.*" (*Dr. Gairdner on Homœopathic Hospitals, 2d Sequel.*)

Dr. Gairdner thus shows that in the Vienna Homœopathic Hospital a much larger number of cases were admitted which were *called* pleurisy and pneumonia than are usually admitted into other hospitals, and more than on any average of cases ought to have been admitted, unless cases were *called* so which were not in reality such as we should term pneumonia and pleuritis.

From these facts we think every candid individual will see that the homœopathic cases are of the mildest description, such as 300 cases of sore throat, 114 cases of diarrhœa, 212 cases of erysipelas and erythema, 140 cases of gout, 61 cases of headache, and 110 cases of chicken-pox, all of which are generally easily and rapidly cured. What physician would ever think of admitting 110 cases of chicken-pox, or 300 cases of sore throat into an hospital? The reader will also see that the cases in the Edinburgh Hospital were of a much more severe character, attacking the lungs, heart, kidney, liver and brain. Numbers, therefore, are comparatively useless in calculating success in treatment; and yet even in the death-rate of regular hospitals Mr. Thomson and Dr. Gairdner have shown that, notwithstanding this difference in the *severity* of the cases, many of them, especially provincial hospitals and the army and navy hospitals, show a much less per-centage of deaths than can be found in the homæopathic hospitals.

But what will be thought of the boasting statistics of homœopaths, if it can be shown that their cases, although so much milder, were *much longer in recovery* than in regular hospitals? It will be very sad indeed if we can show these three things—That they have more deaths (except in the large hospitals of London, Paris, Vienna, etc.); that their cases are much milder; and, that those which do recover are much longer about it than in regular hospitals! We have already shown that in the Leeds Infirmary, the Leeds Dispensary, and 30 provincial hospitals, the deaths are much fewer than in homœopathic hospitals; and that in the hospitals of large cities the high death-rate is quite consonant with great success in treatment, considering the danger of the cases; and even in those large hospitals, as in Guy's, if we include the milder out-patients the death-rate is reduced to little more than 3 or 4 out of every hundred patients who are attended to, not including the casual and unimportant cases. But we think that it can also be shown that homœopathic cases are much longer in recovering than those under regular medical treatment. The following table of Dr. Routh greatly strengthens this opinion, although we by no means look upon it as proved. It would be difficult to prove such an opinion unless a regular physician and a homœopath had each cases *exactly alike:* but as we know that medical cases, like human faces, may bear a *general* resemblance yet never an *exact* one, so it would be impossible to *prove* that the homœopath was longer about his cures. This

	Treatment before Convalesence.	Residence in Hospital after	Total days Residence in Hospital.
Cases treated also Allopathically.	10·1 days	7·0 days	17·1 days
Ditto exclusively Homœopathically	14·0 "	15·1 "	29·1 "
Ditto Allopathically, admitted in 1st stage	9·0 "	4·0 "	13·0 "
Ditto Allopathically, admitted in 2nd stage	10·5 "	6·5 "	17·0 "
Ditto exclusively Homœopathically, 1st stage	20.5 "	11·5 "	32·0 "
Ditto exclusively Homœopathically, scarcely 2nd stage	7·0 "	17·8 "	24·8 "
Ditto exclusively Homœopathically, 2nd stage	13·2 "	17·0 "	30·2 "

table at any rate casts a *suspicion* over the homœopathic practice. Dr. Routh says :—"The very imperfect action of the homœopathic treatment is apparent from this table, setting forth the duration of the pneumonia and residence in the hospital after convalescence. So far then, as an analysis founded on so few cases is to be depended upon, it is unfavourable to homœopathy. Even in the bad pneumonia cases in the Glasgow Infirmary, the average residence was only 20 days, occasionally as high as 26 during the epidemic fever of 1847."

This opinion is confirmed by Dr. Lee, whom we quoted at the end of our Pamphlet No. 2, p. 40, and whose words we will repeat here. He says :—"In looking over the history of several of the cases treated at the London Homœopathic Institution, I found, what might be anticipated, that they were very analogous to the above, viz., the ordinary slighter ailments usually met with in dispensary practice, which seldom require a long treatment, though most of the cases reported in the Homœopathic Annals required two, three, or four months' attendance before the patients were dismissed. Again, we have had, I think, sufficient proof that disorders and diseases are of much longer duration under the homœopathic than under appropriate allopathic treatment, and also, that in acute and serious diseases the mortality of patients homœopathically treated greatly exceeds that of those treated by the ordinary appropriate means."

Dr. Balfour also, who witnessed the Homœopathic Practice in Vienna, says: "Of comparatively trifling cases, many remain for weeks, nay, months, in (Fleischmann's) hospital; while more acute or more interesting cases are hurried out too often with the cure incomplete. . . . Some of the following cases will be found to have been discharged too early, to enable us to be positive as to the ultimate result. . . . I have seen one patient refused admittance, and that, too, the very day after his discharge, without any very good obvious reason. It was a boy, with effusion into the right pleura, following scarlatina, which he had gone through at home. There was also a general anasarcous state of the body, which speedily disappeared, but the chief complaint remained obstinate; and, after thirty-three days' treatment with bryonia, the 2d dilution, four times daily, he was dismissed but slightly improved. This boy was denied admission, when he applied the following day on account of return of pain in the chest—not certainly for want of room, as his bed was empty for days afterwards. This is not the only effusion into the chest which has been dismissed unimproved during the period of my observations; yet this scarcely agrees with Fleischmann's returns, as, out of twelve with exudation in the pleura, occurring during ten years, he has, he says, *cured* all but three, who died. And a physician of the General (Allopathic) hospital has assured me, that many such cases dismissed by Dr. Fleischmann, and subsequently refused admission, have applied to him for relief."—*Dr. Simpson, p.* 109.)

It is candid of some of the homœopaths to acknowledge their faults. It is the first step towards a recantation of their errors. Dr. Herring, one of the leading globulists of Philadelphia, in the August number of the 'American Homœopathic Review,' observes, "We take as granted and admitted by the majority of the leading men as an uniform observation made in Germany, as well as in France and England, and here long ago, that in general the success of the homœopaths in our day is inferior to that of the earlier homœopathic practitioners." And on the authority of the 'Lancet,' a "leading homœopath" confesses "we all know that the numbers in our homœopathic ranks are not lessening; but it is the general observation that the number is, year after year, increasing, who, instead of deriving benefit from homœopathy, are made incurable by so-called homœopathic practitioners." (*Lancet. Dec.* 3, 1859, *p.* 570.) We do not see how this can be otherwise. If the homœopathic practitioner gives infinitesimal doses of medicine, such as the billionth of a drop; or, if he faithfully adopt the homœopathic principle and set up in the sick patient an artificial disease to counteract by its power the real disease; in the first case he leaves the patient to the unaided power of Nature—in fact, does nothing at all; and, in the second case he pretends to generate a disease which must be a little stronger than the original one, in order to overcome it, and which he does not know the end of. It is gratifying to learn that the more sensible followers of this delusion are beginning to find out their mistakes.

Thirdly.—It is quite evident that almost all the Provincial Hospitals of England and Scotland show a much less death-rate than the Homœopathic Hospitals, and yet admit more dangerous cases—accidents and surgical operations of all kinds assisting to swell the death-rate. What petty insignificant homœopathic hospital, or the humble homœopathic dispensaries in Leeds, and other large provincial towns, can be compared, for example, with the Leeds Infirmary and the Leeds Dispensary? Let any candid person who *sees* the *outside* even of these humble Homœopathic Dispensaries say whether the two kinds of institutions are to be compared. But let the same candid person examine the *inside* of the Leeds Infirmary and the Leeds Dispensary; let him examine the cases and see their severity and danger, with the accidents frequently brought in, and the capital operations almost weekly performed, involving the risk of life, and swelling the death-rate considerably. Now let this same person ask how many of the patients die, and he will, or can be answered, as far as can be calculated, only about three or four out of every hundred cases—including accidents, operations, patients brought in dying, and cases which were desperate from the first. Next examine the Homœopathic Hospitals. Go into them—examine the *kind* of cases, and you will find them generally mild and insignificant cases. You do not see beds occupied by consumption, paralysis, Bright's disease of the kidney, organic disease of the heart, cancer, &c. But according to Dr. Gairdner, whose paper we have before quoted, you find more cases of chlorosis, colic, diarrhœa, headache, sore throat, influenza, and chicken-pox, such cases as would seldom or much less frequently be admitted into a large general hospital; and yet what kind of death-rate do they show? According to their own statistics they have a mortality of five or six deaths out of every hundred cases. Now we look upon this as very discreditable, and surely can be no proof of the success of homœopathy, even when we look at mere *numbers*, and much less when we compare the *kind* of cases! Besides, how can you trust that the success which you hear of among the homœopaths is due to pure homœopathy, when you continually hear the advocates of this system say—"Oh, but Dr. ——— practises *both systems*, the old and the new!" Dr. ——— conscientiously believes that the *old* system is erroneous, but has no objection to violate his conscience now and then when in difficulties! So long as there is nothing to be done in the case but to leave Nature alone, Dr. ——— can practise conscientiously, but when something has to be really done for the patient, he quietly hushes the stings of his conscience, and does the best for his patient on the *old* system.—making use of any remedy which his judgment points out without any reference to homœopathy! Indeed, most of the medicines used by homœopaths have no reference to homœopathy at all, and do not act in the least on homœopathic principles. Hence we condemn the whole thing as a piece of deception.

Let the reader examine the Table on the following page, and (with the exception of the Edinburgh and Glasgow Infirmaries, which we reckon as like Metropolitan Hospitals, and with a high death-rate like the London Hospitals, for the reasons before given), let him notice the low death-rate in the Scotch Provincial Hospitals.

Next let him examine the Table immediately following it, of statistics of thirty English Provincial Hospitals, drawn out by Mr. Thomson, and (with the exception of the large hospital of Manchester, which is rather higher than the rest in its death-rate,) let him observe the small rate of mortality amongst their cases. These tables, we believe, do not include the out-patients; so that if these out-patients were added, it would reduce the mortality in the Leeds Infirmary and Guy's Hospital (as we have shown in a former page) to a remarkably low figure.

Statistics of Eight of the principal Civil Hospitals of Scotland, prepared from the Managers' Reports, and other authorised documents, by J. Thomson, Esq. Showing the number of patients treated; the average residence in hospital; the deaths, and ratio of mortality per cent., with the proportion of deaths to cures. (*Edinburgh Medical and Surgical Journal*, 1843, *page* 341.)

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Hospitals.	Pop. of respective towns in which the hospitals are situate Census 1841.	Period one year, ending	Under treatment during the year.	No. of cases treated to a termination.	Died.	Cured.	Relieved and discharged.	Discharg. from other causes, viz., by desire, irregular and improp., no benefit.	Aver. daily number in hospital.	Days in hospital for each cure.	Aver. residence in hospital in days	Ratio of deaths per cent. of cases terminated.	Ratio of cures per cent. of cases terminated.	Proport. of deaths to cures.	Days of residence in hospital for 1 death.	Ratio of admission per cent. of the population.
Royal Infirm. Edinburgh	164451	Oct. 1, 1842	3770	3529	443	2255	551	280	303	41	29	12.55	63.89	1 to 5.09	249	2.3
.. Aberdeen	64767	Dec, 31, 1842	1956	1822	85	1458	167	112	113	28	21	4.66	80.02	17.15	481	3.0
.. Dumfries	13353	Nov. 11, 1842	508	463	29	352	44	38	41	42	29	6.26	76.02	12.14	516	3.8
Infir. Montrose	13811	June 1, 1842	324	295	20	258	7	10	23	32	26	6.77	87.45	12.90	419	2.3
Royal Infirm. Glasgow	273147	Dec. 31, 1842	3612	3405	328	2646	226	205	228	31	23	9.63	77.70	8.06	254	1.3
.. Dundee	63825	June 1, 1842	916	882	45	762	52	23	52	25	21	5.10	86.39	16.93	422	1.4
Northern Infir. Inverness.	11592	Dec. 31, 1842	385	344	15	237	41	41	38	59	36	4.36	69.00	15.80	925	3.3
County and City Infir. Perth.	18281	Dec. 31, 1842	486	424	29	305	34	56	42	50	31	6.84	71.93	10.54	538	2.7

Table of the number of patients treated, and the deaths in thirty English Provincial Hospitals, (from material in the British Medical Almanac for 1836 and 1837), showing the average mortality in each. (*Edin. Med. and Surg. Journal*, 1843, *p.* 353.)

Medical Institutions.	Period one year.	Treated.	Died.	Mort. p. cent.
Aylesbury General Infirmary,	1835–6,	108	7	6·47
Bath United Hospitals	1835,	814	47	5·77
Bedford General Infirmary,	1835–6,	544	35	6·43
Birmingham General Hospital,	1828–9,	1556	68	4·37
Bristol General Hospital,	1835,	279	13	4·66
Bury St. Edmunds, Suffolk Hospital,	1833,	326	8	2·45
Canterbury Infirmary,	1835,	501	10	2·00
Cambridge Addenbrooke Hospital,	1832–3,	620	17	2·74
Chester General Hospital,	1835–6,	518	21	4·05
Colchester General Hospital,	1834,	284	9	3·16
Derby Infirmary,	1828–9,	584	18	3·09
Exeter Hospital,	1835–6,	1069	35	3·27
Gloucester Infirmary,	1834,	646	33	5·10
Hereford Infirmary,	1828–9,	300	11	3·66
Kingston-upon-Hull General Infirmary,	1835,	528	32	6·06
Leeds General Infirmary,	1832–3,	1298	77	5·93
Manchester General Infirmary,	1833–4,	1852	139	7·50
Newcastle Infirmary,	1828–9,	1100	28	2·54
Norfolk and Norwich Hospital,	1835,	774	46	5·94
Northampton General Infirmary,	1833–4,	756	32	4·23
Nottingham General Hospital,	1835–6,	743	46	6.19
Oxford, Radcliffe Infirmary,	1835,	853	25	2·93
Salisbury Infirmary,	1833–4,	887	27	3·04
Salop Infirmary,	1834–5,	834	30	3·59
Sheffield Infirmary,	1835–6,	894	31	3.46
Stafford Infirmary,	1828–9,	524	13	2·48
Winchester Infirmary,	1833–4,	782	31	3·96
Worcester Infirmary,	1828–9,	680	31	4·55
Suffolk Hospital, Bury St. Edmunds,	1828–9,	250	7	2·80
York County Infirmary,	1835,	305	19	6·22
Total,		21209	946	4·46

Have the public anything to complain of in these statistical tables? Have not their liberal contributions in support of these noble institutions been well and successfully applied in alleviating the miseries of their fellow-creatures, and generally of their poor neighbours? Is it true, therefore, that regular physicians and surgeons are less successful than the practitioners of homœopathy? In this table are included the large hospitals of Bath, Birmingham, Bristol, Leeds, Manchester, and Sheffield, any one of which we suspect contains as many beds as all the homœopathic hospitals in the whole British empire; and yet the average of deaths is only about 4½ out of every 100 cases admitted, and not even reckoning the out-patients; while the homœopaths acknowledge that they lose nearly six out of every 100. We will not waste any more paper and ink upon this subject, as we consider that any candid examiner will feel at once satisfied that we have proved our case.

Fourthly. We acknowledge that all hospitable statistics may be more or less fallacious, but those of military hospitals are less so, as they have been calculated for years, and without any object but truth. Homœopathic statistics have evidently been got up with the object of drawing a comparison between them and others. This is the general impression in the medical profession, and there are many suspicious evidences of this. One suspicious fact we have already related as given by Dr. Balfour, in a preceding table, and a still stronger one we shall have to state when we give the table of cholera cases which occurred at Huddersfield some years ago.

Of all hospital statistics, with respect at least to the death-rate, the Army and Navy Reports must be the most correct. In a general hospital a patient may be dismissed apparently cured, but may die in a month after from a return of the disease; but in the military hospitals every patient is watched to the end of his case, and every death must be accurately known, as all the patients are not only in hospital during sickness, but under the eye of the surgeon during health, and for months after dismissal from hospital. It may be said that soldiers are all picked men; so they are, but they are subject to as dangerous diseases as civilians, and have many disadvantages, such as crowded barracks, and sometimes severe work in camp, and in other ways. Moreover it cannot be said that the *families* of the Russian sailors are *picked* men; they are women and children, and yet even in them when treated by the skilful Russian surgeons, the death-rate in sickness is very low, especially when we consider how many children die in infancy, and what disadvantages the women must labour under in such a climate as Russia. These army and navy returns, both of England and Russia, show the present success of medical treatment by surgeons.

In the Army and Navy Returns, where young and old persons are excluded, the following is the rate of mortality obtained (*Dr. Routh*, *p.* 45):—

ARMY. 1818—1837.	Admissions.	Deaths.	One in	Per Cent.
Gibraltar	58,227	1,291	45·1	2·2
Malta	44,639	666	70·1	1·4
Ionian Islands	84,438	1,775	47·5	2·1
Bermudas	15,356	338	45·4	2·2
Nova Scotia and Brunswick	36,174	649	56·8	1·8
Cape District	25,506	311	72·0	1·2
Cape Frontier	5,740	65	88·0	1·1
St. Helena	4,360	150	29·0	3·4
Mauritius	38,108	835	45·0	2·1
Canadas	66,957	982	681·0	1·4
Total ..	341,397	8,068	42·0	2·1

NAVY.	Admissions.	Deaths.	One in	Per Cent.
Home Service, 1830—36 ..	25,586	229	11·1	0·9
Cape	14,858	263	5·6	1·7
South America, 1837—43 ..	25,361	191	13·2	0·7
Mediterranean	97,081	996	9·6	1·6
Variously employed, 1830—36	17,532	171	7·2	0·9
Total ..	180,418	1850	9·7	1·0

Here are returns, then, which forcibly contradict the statements that regular surgeons are not so successful as the practitioners of homœopathy.

Compare these English tables with the Russian (*Med. Times*, *Oct.* 22, 1859, *p.* 401), which, although not quite so successful, are very satisfactory.

Table showing the general number of sick of all branches of the service treated in hospitals, lazarettos, and on board ship:—

Under treatment on Nov. 1, 1857 - - - - -	3201
Cases during the year - - - - - - -	45824
Recoveries - - - - - - - -	43163
Deaths - - - - - - - - -	2120
Transferred to other hospitals - - - - - -	1497
Under treatment on Nov. 1, 1858 - - - - -	2245
Ratio of Recoveries to sick - - - - - -	1·1
" of Deaths to recoveries - - - - - -	1·20

This is highly creditable to the Russian surgeons of whom we entertain the highest opinion, and when we compare this table with the next, in which the *families* of these Russian sailors are calculated, we are still more pleased, as it shows considerable success among a class of people who must have many disadvantages.

Table showing the sickness in the families of the seamen of the Russian Navy, between Nov. 1, 1857, *and Oct.* 31, 1858:—

Number of cases under treatment on Nov. 1, 1857 - -	183
Number of cases under treatment during the year - -	6875
Recoveries - - - - - - - - -	6348
Deaths - - - - - - - - -	457
Remaining under treatment on Nov. 1, 1858 - - -	253
Ratio of Recoveries to sick - - - - - -	1·1
" of Deaths to recoveries - - - - - -	1·13

From the above we make it out that the death-rate in the Russian Navy was 4·6, and among the families 6·6; whereas in the British Army and Navy returns, which we have placed before the reader, we find the astonishing fact that in the Army out of 341,397 cases *only about* 2 *out of every* 100 *died*, and in the Navy out of 180,418 *only* 1 *out of euery* 100 *died!* Now here there can be no deception as to numbers. While the homœopaths *acknowledge* that they lose nearly six out of every 100, we find that the Army and Navy surgeons, not one of whom we ever heard of as being a homœopath, only lose one or two out of every 100, and yet every death must be accurately known.

Even in the Indian army, in a climate and with habits which must greatly aggravate disease, we find the following table (*Dublin Quarterly Journal Nov.* 1859, *p.* 453), which refers to the Army of Bombay:—

Stations.	Periods of Observation.	Treated per 100 of Strength.	Deaths per 100 of Strength.
Kalapore - - - - -	20 years ending 1849	300·34	2.05
Sholapore - - - - -	" "	226·95	2·22
Kirkee - - - - -	" "	177·34	2·64
Belgaum - - - - -	" "	172·74	2·75
Punjaub Army - - -	" "	197·82	2·89
Aden - - - - -	" "	137·98	2·99
Deesa - - - - -	" "	171·82	3·40
Ahmednuggur - - - -	" "	233·15	3·45
Bhooj - - - - -	" "	209·99	3·83
Poona - - - - -	" "	230·33	4·11
Kanark - - - - -	" "	100·07	4.37
Mhow - - - - -	" "	182·54	4.97
Indus Army - - - -	" "	155·56	7·36
Kurrachee - - - -	" "	203·43	9·02
Bombay and Colaba - - -	" "	254·16	10.92
Hydrabad and Scinde - -	" "	369.48	23.52

As nearly as we can calculate, this table shows a death-rate of about 2·7 per cent.

Let the homœopaths dispute these tables if they can. We have given accurate references to the books from which we have taken them. These numerical calculations may be here and there slightly erroneous, but not to our knowledge, and not so as to alter the force of the argument that the homœopaths ought to be ashamed of boasting of their success in treating diseases; and that it is undignified and unworthy of a liberal profession to be circulating their statistical tables in pamphlets and other ways for the sake of drawing comparisons between themselves and regular practitioners of medicine.

Fifthly.—We have now to make a grave charge against homæopathic statistics, which we shall do without giving names, as this might be considered personal. It will be remembered, that some years ago, the cholera prevailed in this country, and amongst other places at Huddersfield. In the 'Homœopathic Times' for 1850 is a report on cholera by a gentleman then practising homœpathy at Huddersfield, but now in a neighbouring town. Well may this gentleman say, "if statistics afford valuable aid in the advancement of science they are no less useful to those who wilfully essay to pervert facts." Now let us examine this homœopath's statistics, and ascertain whether his object was to advance science or to *pervert facts.* He published the following Table as a Report of the Cases of Cholera which occurred at Huddersfield, and for the accuracy of which he pledged himself:

Name.	Age.	Residence.	Treatment.	Results.
William Moss.........	28	Paddock Head	Allopathic ..	Died.
Hannah Taylor.......	55	Ditto ..	Ditto ..	Ditto.
Sarah Tomlinson......	78	Ditto ..	Ditto ..	Ditto.
A. Micklethwaite......	17	Ditto ..	Ditto .	Ditto.
Sarah A. Taylor.......	24	Ditto ..	Ditto ..	Do. (pregnant).
Thomas Dyson........	1¼	Ditto ..	Ditto ..	Ditto.
Jane Dyson...........	4⅓	Ditto ..	Ditto ..	Ditto.
J. Micklethwaite......	44	Ditto ..	Ditto ..	Ditto.
John Alpin...........	—	Ditto ..	Ditto ..	Ditto.
Charles Potter........	44	Ditto ..	Ditto ..	Ditto.
Jas. Micklethwaite....	—	Ditto ..	Ditto ..	Recovered.
Mrs. Micklethwaite....	—	Ditto ..	Ditto ..	Do. (supposed cholera.
William Leonard......	88	Quay-street ..	Ditto ..	Died.
James Atkinson.......	49	Fountain-st. ..	Ditto ..	Ditto.
Elizabeth Wilson......	50	Lane ..	Ditto ..	Ditto.
George Jagger........	4½	Ditto ..	Ditto ..	Ditto.
Benjamin Berry.......	66	Folly Hall ..	Ditto ..	Ditto.
Mary Goodyear.......	89	Cropper's-row	Ditto ..	Ditto.
Abraham Smith.......	89	Ditto ..	Ditto ..	Ditto.
Eli Wilson............	58	Lane ..	Ditto ..	Ditto.
Mrs. Greenhough......	40	Paddock Head	Homœopathic	Cured.
John Briggs..........	50	Ditto ..	Ditto ..	Ditto.
John Briggs' Child....	7	Ditto ..	Ditto ..	Ditto.
Thomas Cliffe........	40	Ditto ..	Ditto ..	Ditto.
Mrs. Richardson......	85	Marsh ..	Ditto ..	Ditto.
James France.........	17	Ditto ..	Ditto ..	Ditto.
Mr. Bearers..........	85	Sheepridge ..	Ditto ..	Ditto.
Mrs. F-— tt..........	60	Clare Hill ..	Ditto ..	Ditto.

"Results: number of cases treated allopathically 20; recoveries 2; deaths 18. Number of cases treated homœopathically 8; recoveries 8; deaths 0."

We will now add the remarks of the Reviewer of these Cholera Reports, suppressing the name of the practitioner, for fear of being considered personal. He says:—

"Will our readers credit us when we assure them that this table is a fabrication—ingenious, it is true, but false from beginning to end? Yet it is so. Few literary members of the profession are ignorant of the zeal, industry, and truthfulness of Dr. John Taylor, formerly professor of clinical medicine in University College, and now a physician in Huddersfield. Dr. Taylor has published a report on the cholera, which is characterized in the Report on Cholera in England lately issued by the registrar-general as 'a model which the local inquirer should consult.' To secure as complete accuracy as possible, he obtained personally the particulars of the cases from the families of the sufferers and the practitioners in attendance; they amounted in all to 93, of which 39, or 40 per cent., were fatal. Dr. Taylor drew up a table of the cases and their termination; and on reference to it he finds, that in or near the places named in the above table, there were FORTY-SIX RECOVERIES. The homœopath pledges himself (what the pledge is worth may be inferred from that damning fact)—the homœopath 'pledges' himself for the accuracy of his report; yet of 46 recoveries he could only hear of TWO! Was the homœopathist blind and deaf when he inquired as to the fate of John Goodyear the husband, and John Goodyear the son, of Mary Goodyear, whose death he chronicles from 'allopathic' treatment? Could he possibly remain in ignorance, that these two persons were attacked on the day following that on which Mary Goodyear was attacked, and that they both recovered? How did it happen he could state that Mary Goodyear died, but could not say, also, that her husband and son recovered? And these are the cholera statistics of homœopathy! Perhaps the reader may think this *suppressio veri* a solitary instance, and possibly an error. It is neither the one nor the other. The reader will observe that the homœopathist mentions 12 cases of cholera as occurring under 'Allopathic' treatment at Paddock Head, of which, two are reported recoveries, although, with a cunning pretence to great accuracy, he adds the remark, 'supposed cholera,' to one of the recoveries, positively making the mortality 99 per cent. Yet, in reality, there were 25 cases in that locality, treated by three or four practitioners, and by Dr. Taylor himself; the deaths were 11, the recoveries 14, several of the latter being in the same houses as those cases of which the homœopathist recorded the fatal result!" (*Medico-Chirug. Review, April* 1852, *pp.* 479, 480.)

It seems almost incredible that this gentleman should have have published such a statistical table, when he must have been aware of numerous other cases of cholera which existed in his neighbourhood, and even in the same family. Although he publishes the death of Mary Goodyear, yet he omits to mention that John Goodyear, the father, and John Goodyear, the son, recovered under regular medical treatment. To show the extraordinary fallacy of this homœopathic report, we append a Table of cases attended by the regular medical practitioners of Huddersfield, drawn up by the late estimable and talented Dr. John Taylor.

Number and Name of Patient.	Age.	Died or Recovered.	Medical Attendant.
1 Francis Donoghue	50	Died.	Mr. Clarke.
2 Mrs. Thomas	88	Recovered.	Mr. Booth.
3 Josh. Whitaker	27	Do.	Mr. Clarke.
4 William Leonard	83	Died.	Mr. Rhodes.
5 Mrs. Hardy	65 (?)	Recovered.	Mr. Tatham.
6 Mrs. Gibson		Do.	Mr. Sissons.
7 Mrs. Kershaw		Do.	Do.
8 James Atkinson	50	Died.	Mr. Tatham.
9 Grace Bake	29	Do.	Mr. Machill.
10 Mrs. Hirst	58	Do.	do
11 Jonathan Booth		Recovered.	Mr. Sissons.
12 William Rhodes	45	Do.	Mr. Allatt.
13 Abraham North	85	Do.	Mr. Greenwood.
14 Mrs. Ganter	85 (?)	Do.	Mr. Tatham.
15 John Leonard		Do.	Mr. Sissons.
16 Mrs. George Crosland	80	Died.	Mr. Machill.
17 Josh. Thornton		Recovered.	Mr. Booth.
18 Martha Walker	60	Do.	Mr. Allatt.
19 Mary Craven	35	Died.	Mr. Machill.
20 ——— ——— F.	72	Do.	Dr. Robinson.
21 Grace Bake's Child	1½	Do.	Mr. Booth.
22 Henry Beaumont	27	Do.	Do.
23 ——— ——— M	70 or more	Do.	Dr. Robinson.
24 John Stocks	80	Recovered.	Mr. Maxon.
25 Sarah Tomlinson	78	Died.	Mr. Allatt.
26 Josh. Hinchliffe	61	Recovered.	Do.
27 Mrs. Hirst	53	Died.	Do.
28 James Leonard	52	Recovered.	Mr. Sissons.
29 Hannah Taylor	55	Died.	Mr. Allatt.
30 William Moss	23	Do.	Do.
31 John Alpine	40	Do.	Do.
32 Sarah Taylor	24	Do.	Do.
33 Mary Tomlinson	20	Recovered.	Do.
34 Allen Micklethwaite	18	Died.	Do.
35 Hannah Micklethwaite	46	Recovered.	Do.
36 John Micklethwaite	44	Died.	Do.
37 Thomas Wood	41	Recovered.	Mr. Booth.
38 Aaron Taylor	56	Do.	Mr Allat.
39 Ezra Whiteley	2	Died.	Do.
40 Charles Potter	29	Do.	Do.
41 Thomas Dyson	1	Do.	Do.
42 Eli Wilson		Do.	Mr. Sissons.
43 William Byrom	48	Recovered.	Mr Allatt.
44 Mary Ann Byrom	7	Do.	Do.

Number and Name of Patient.	Age.	Died or Recovered.	Medical Attendant.
45 Jane Dyson	4	Died.	Mr. Allatt.
46 James Dransfield	14	Recovered.	Do.
47 Sally Byrom	2	Died.	Do.
48 Patrick Connell	26	Recovered.	Mr Clarke.
49 Rachel Dransfield	18	Do.	Mr. Allatt.
50 Mrs Kaye		Do.	Mr. Sissons.
51 Martha Brown	19	Do.	Mr. Tatham.
52 Ann Taylor	23	Do.	Mr, Allatt.
53 Samuel H. Micklethwaite	21	Do.	Do.
54 J. Taylor		Do.	Mr. Greenwood, Mr. Clarke and Mr. Booth.
55 Ann Byrom	28	Do.	Mr. Allatt.
56 Mrs. Wilson		Died.	Mr. Sissons.
57 William Eastwood	39	Recovered.	Mr. Tatham.
58 John Robinson	29	Do.	Mr. Clarke.
59 Mary Ann Ferguson	30	Do.	Mr. Tatham.
60 James Jagger	5	Died.	Mr. Booth.
61 David Taylor	29	Recovered.	Mr. Allatt.
62 Josh. Shepherd	42	Do.	Mr. Tatham.
63 Richard Berry	62	Died.	Do.
64 Mary Goodyear	39	Do.	Mr. Clough.
65 John Goodyear	45	Recovered.	Mr. Clarke.
66 Mary Walshaw		Do.	Mr. Sissons.
67 John William Taylor	3	Died.	Mr. Rawcliffe.
68 John Goodyear	5	Recovered.	Mr. Clarke.
69 Robert Whitehead		Do.	Mr. Sissons.
70 Harriett Marriott		Do.	Do.
71 Abraham Smith	39	Died.	Mr. Booth.
72 Mary Quarmby	74	Do.	Mr. Dean.
73 Hannah Pick	17	Recovered,	Mr. Tatham.
74 Margaret Heaton	37	Do.	Do.
75 Maria Moxon	32	Do.	Do.
76 —— —— M.		Died.	Dr. Robinson.
77 Ruth Berry		Recovered.	Mr. Sissons.
78 Emma Wilcock		Do.	Do.
79 James Charlesworth	61	Do.	Mr. Tatham.
80 John Saville	57	Died,	Dr. Robinson.
81 Josh. Gledhill	60	Do.	Mr. Roberts.
82 —— ——	18 months	Do.	Do.
83 Charles Varley	36	Do.	Mr. Dean.
84 Susan Shaw,	41	Recovered.	Mr. Tatham.
85 Hannah Clarkson	42	Do.	Do.
86 Mrs. Varley	57	Died.	Mr. Roberts.
87 Mr. Varley	60	Recovered.	Do.
88 Mrs. C. Varley		Do.	Do.
89 Mrs. Shaw		Do.	Mr. Sissons.
90 Jasper Dawson	36	Do.	Mr. Booth.
91 Richard Thorpe		Do.	Mr. Sissons.
92 David Sykes	50	Do.	Mr. Clarke.
93 George William Taylor	2	Died.	Mr. Allatt.

The reader will find a report of *each case* in the 'Medical Times' for 1851, at pages 259, 340, 399, which we believe the Registrar-general considered as a perfect specimen of a medical report. Any one who thinks fit to question the accuracy of these remarks, can now turn to the same papers which we have used, and call them in question if he like.

It thus appears that instead of the regular practitioners having 18 deaths, and 2 recoveries, they hand 39 *deaths and* 54 *recoveries!* There can be no deception here. The names are all before the public, with the medical practitioners who attended them. After reading these reports we ask any candid person whether or not he would again trust to the reported statistics of homœopathy. We find that not only is the *severity* of the cases concealed from the public by the homœopaths, but that the actual *number* of cases and deaths are erroneously reported, and therefore we cannot have the least faith in what they say. We honestly confess that we totally disbelieve them in all their statements.

With respect to the statistics of the Vienna Homœopathic Hospital, let the reader first remember that this is a private one, in the convent of the Sisters of Charity, and next, that Dr. Fleischmann could admit and discharge the patients without any control. Under these circumstances we do not hesitate to say that any man with any particular object in view could regulate his reports so as to increase their plausibility. Dr. Balfour, who for some time witnessed the practice of this hospital, writes as follows:—

"The patients are admitted and discharged by the physician, without any control, so that, to say the least, it requires a man to be very conscientious to decide impartially between temporary improvement and perfect cure, especially when he recollects that the fate of his creed and his institution may depend upon the nature of his returns to government. These returns are made monthly, with a yearly *resumé*.

"Some of the cases will be found to have been discharged too early to enable us to be positive as to the ultimate result. Again, these cases, or others discharged apparently cured, may apply for readmission. and be, under some pretext or other, refused; while, to disarm suspicion, a few whose relapses seem more manageable may be readmitted. Such may not be the case in point of fact, still it is very possible. I have seen at least one patient refused admittance, and that, too, the very day after his discharge, without any good obvious reason. . . . . . . And a physician of the General Hospital has assured me that many such cases dismissed by Dr. Fleischmann, and subsequently refused admission, have applied to him for relief, and which relief they have obtained by the use of purgatives and baths. Then again, there are, I may say, hundreds of trifling cases admitted here, which would not have been admitted into any hospital in England. Many of the patients get no medicine; a few a single dose; and even of comparatively trifling cases many remain for weeks, nay months, in the hospital; while more acute or more interesting cases are hurried out too often with the cure incomplete.

"The whole process of the admission and discharge of patients is mysterious. Still so much is certain, that most of those admitted have been previously visited at their own houses by the assistant. Many cases not improving, or apparently not likely to improve, are got rid of very summarily. During most of the time, I visited in the morning along with Dr. Fleischmann, and latterly, for some weeks, in the afternoon along with his assistant, it not being then permitted to visit in the morning. I was told the cause of this restriction was that the students might have an opportunity of taking a course from the assistant.

"I feel convinced that the secret of Dr. Fleischmann's great seeming success lies in the fact of the admissions and dismissions being entirely uncontrolled, and their being no check on the diagnosis. Rarely other than well-marked cases have their diagnosis written on the board at their bed-head, the others being left blank, and entered in his book, of course, as he pleases." (*Brit. and Foreign Medical Review, Oct.* 1846, *p.* 572)

So that these reports may be as fallacious as the homœopathic cholera reports of Huddersfield, and totally untrustworthy!

"Not only is Fleischmann's hospital in Vienna a private one, but there is no large public hospital in Austria, Prussia, or France, where this system is sanctioned by Government. In answer to notes addressed to the American ministers at Vienna, Berlin, and Paris, the following is an abridgment of the replies:—

"Count Buol—Minister of Foreign affairs in Austria, answers, '1st, that in Austria, Homœopathy is taught not by publicly appointed professors, but only by private teachers. 2d. That this mode of cure is practised, not in public hospitals, but only in cloister, criminal and private hospitals. 3rd. That the private practice of Homœopathy is permitted to any physician who has a Diploma.'

"Ramner, the Prussian Minister, replies, 'I have the honor to inform your Excellency, that Homœopathy in Prussia, is not admitted into the Universities nor Hospitals, nor other public Institutions. Physicians are allowed, if they please, to exercise Homœopathy in private practice.'

"Rouland—Minister of Public Instruction in France, writes, 'the exercise of Homœopathy is not legally authorized in France. My administration has not authorized me to exercise any measure having reference to the teaching of Homœopathy.'" (*Brit. American Journal, March,* 1860, *p.* 141.)

What will the homœopaths say to Count Buol's assertion that their system of practice is adopted, "not in public hospitals but only in cloister, criminal and private hospitals"? It must be rather a *let-down* to be thus described by a public minister.

With respect to the statistics of particular diseases, such as pneumonia, we have as little confidence in them as we have in other homœopathic reports. It is well known that the stethoscope was not used by Dr. Fleischmann in the Vienna hospital, and yet he pretends to distinguish cases of pneumonia. In this way he might put down almost any common cold as a case of pneumonia. Besides, Dr, Balfour detected Dr. Fleischmann in a deliberate attempt to make the report on pneumonia better than it really was. It may seem a trivial fault to miss out a case or two in a medical report, but when the object in view is to deceive the public, it becomes a grave fault and quite unpardonable. At page 107 of Dr. Simpson's work on Homœopathy, is the following: "There is a striking and instructive episode connected with the averment made by Fleischmann, that in 1846 he only lost *two* patients in his wards from pneumonia. To save the credit of the alleged success of homœopathy in pneumonia, the Editors of the British Journal of Homœopathy, in reviewing the Vienna cases, watched and reported upon Dr. Balfour, and apparently unaware of being preceded in their tactics by Fleischmann himself, attempted to deny that one of the *three* fatal cases seen and recorded by Dr. Balfour, was a case of pneumonia. The Editors remark,—

'Dr. Balfour saw nineteen cases of pneumonia. He observes: 'These cases of pneumonia give an average of twenty-four, an average treatment of 12·6 days, and a mortality of 15 per cent.; three out of the nineteen having died. Skoda's cases of pneumonia, during the same time, amount to forty-five, his deaths to three; giving an average of 6·6 per cent.' This comparison, we think, is incorrect, as we cannot consider Case VI. to have been a case of pneumonia at all; which being omitted, there would be eighteen cases, and two deaths; or 11·1 per cent.'"—(Vol. v. p. 92.)

"Dr. Balfour assures me that this case (No. VI. in his report of the cases of pneumonia), was most unmistakably marked as an instance of that disease; and was, as already stated, written down as such on the patient's board by Fleischmann himself. Dr. Balfour's published report explicitly mentions, that the patient had pain in breathing,—rusty blood-stained expectoration,—the percussion on the right side was dull, as high as the centre of the mammary and scapular spaces,—that over these dull spaces the respiration was bronchial, and elsewhere vesicular, &c.; all of them forming amply conclusive evidence that the disease was pneumonia; and he died on the third day after admission. (*See Dr. Forbes' Review*, vol. xxii. p. 584; and also vol. xxiii. p. 611.) Is it not truly significant, and in itself portentously suspicious, to find Dr. Fleischmann and his British homœopathic brethren trying,—and both in the same way,—to change the statistics of these pneumonia cases, in order to make them *apparently* conform to the alleged success which homœopathists have had in the treatment of this disease? When we find two writers, independently of each other, thus adopting the same improper tactics regarding the same cases, does it not show that these tactics are a part of the general system of their medical or statistical creed?"

We need make no comment on this system of deception.

We must say that the homœopaths completely mistake the number of deaths from this disease occurring in regular medical practice. It may be very fatal in some of the large London and Continental hospitals, owing to the severity of the cases when admitted and the age of the patients, but in general this disease is far from fatal.

We believe that the cases of pure pneumonia are comparatively rare: and when we see numerous cases recorded, we consider that they have been mixed cases, and often complicated with typhus, fatty heart, or valvular disease, &c. No statistics, therefore, of this disease can be correct. You may call a hundred cases admitted into a hospital cases of pneumonia, and perhaps a little pneumonia may be mixed up in the cases. They may all be cured, and given out to be cures of pneumonia, but this is quite untrustworthy, and every candid practitioner, whether of medicine or homœopathy, must acknowledge this to be true. We have enquired at the Leeds Infirmary, and find very few cases of death from pure pneumonia. But the best corroboration of our views is in a paper by Dr. W. T. Gairdner, of Edinburgh, who has taken pains to examine into this subject. During five years Dr. Gairdner lost only ten cases in the Edinburgh Infirmary, although he has generally the charge of fifty beds for different diseases, and "only one could fairly be said to be a death from idiopathic or uncomplicated pneumonia." (*Edin. Med. Journal, March* 1860, *p.* 834.)

Again, in Dr. Routh's pamphlet we find that, "In 1824, Laennec did not lose one pneumonia case. In 1825, out of 28 pneumonia cases, simple or double, with or without pleurisy, one only died, and this an old man of seventy. In 1825, out of 34 pneumonic cases, 3 died; two females brought *in articulo mortis*, and an old man above seventy. Altogether, Laennec concludes that out of 57 pneumonias, he in reality lost but two patients above seventy. Louis lost, out of 20 cases, 3 patients, all from sixty to seventy. In 1831-2, Trousseau lost 2 patients out of 58. None of these were bled in the hospital; but 5 had been so before admission. Since that period, owing to a change of type, he had not been so successful. Professor Bang, out of 54 cases of pneumonia treated at Copenhagen, lost only 2. These were generally bled at the onset. Dr. Wolf did not lose any out of ten cases (Grisolle). Out of 75 cases treated by Bouillaud, recovery was observed as follows:—55 cases of single pneumonia, 2 died; 16 cases of double, 11 died. Mr. Husson, out of 43 cases, bleeding each patient from one to eleven times, lost only 3 cases. Grisolle collected 44, for the most part severe; in 2 cases the pneumonia was double, 6 only died. The fatal cases occurred among weakened individuals, who were generally above fifty years old. Were I to imitate in this compilation the homœopaths, I could conclude I had collected some 300 cases with a mortality of about 3 per cent. In justice, however, such a computation and comparison must be condemned; the type and peculiar treatment of each being so different, and admitting of no general conclusion." (*Dr. Routh, p.* 57.)

There can be no doubt, however, about the value and accuracy of the Army and Navy Reports. "The official Army Returns show 12,271 cases of pneumonia among the British troops stationed at Gibraltar, Malta, Ionian Islands, Bermudas, Canada, Cape, Mauritius, and St. Helena. Out of these 12,271 patients attacked with inflammation of the lungs, 413 died; or, in other words, 1 in every 29 cases of the disease was fatal; or, about 3 in every 100. Out of above 2000 cases of pneumonia at Gibraltar, only 1 in 45 proved fatal; or, about 2 in every 100 attacked with this disease.

The official Navy Returns show 3,099 cases of pneumonia among the sailors in our fleets then stationed at the Cape, in South America, the Mediterranean, &c. Out of these 3,099 patients attacked with inflammation of the lungs, 136 died; or, in other words, 1 in every 23 cases of the disease was fatal; or, about 4 in every 100. Out of 295 cases of pneumonia in the ships placed on the South American stations, only 1 in 59 died; or, less than 2 in every 100 attacked with this disease died." (*Dr. Simpson, p.* 113.)

In conclusion the most charitable opinion to arrive at with respect to the fallacies of homœopathy, is that the writers on this system become muddled, and at last begin to think that their fallacies *are* truths. Some will write in such a way that each word is correct enough, and perhaps each sentence, but somehow when you have read the whole paragraph or speech, you can make neither *head nor tail* of the subject. For example, M. Granier, a celebrated homœopath, writes as follows:—"I would ask if in the middle of the 19th century, an age eminently favourable to the development of *fluidism*—any one should object to consider diseases as the *fluidic derangement of our vital fluid?* which is perhaps our *specific electric fluid.*

"Medicines are *fluidic powers.* They are *beings* that man may create at his will. I wish I could say they are occult powers forming the chain of fluidic communication between this world and that beyond the tomb. But I am convinced in my own mind, that placed on the limits of *fluidic dynamism*, our observation might cast a scrutinizing glance into the unseen world." (Conferences on Homœopathy by M. Granier.) (*Lancet, Feb.* 11, 1860, *p.* 148.)

The works of Hahnemann are nonsensical enough, but M. Granier is a recent writer. and one who is supposed to be *well up* in all the modern improvements of homœopathy.

It has often amused us to see the difference of opinion amongst homœopaths: take for example the opinion of this said M. Granier respecting camphor. Every one who has examined homœopathic medicines and tasted them (such as can be tasted) will see how often *camphor* is prescribed. But, says Mr. Granier, the very smell of camphor "forms an antidote to homœopathic medicines." He calls it the "*oidium* of the pharmaceutical vineyard," and declares that a piece in a patient's room destroys the influence of his remedies. This may account for the want of success of some homœopaths, who, we know from our own personal knowledge, have been giving strong doses of spirts of camphor with a solution of morphia when the bowels have been blocked up for several days, and no attempt made to relieve them.

We think that we have now in these three pamphlets written enough to prove that the practice of homœopathy in the selection and doses of their medicine is absurd—that its principles are unscientific and untenable—and that its statistics are untrustworthy. We shall hereafter decline troubling ourselves further on this subject.

135.—DESCRIPTION OF THE NEW STAPLE SUTURE WITHOUT A LIGATURE.

By James Startin, Esq., F.R.C.S., Surgeon to the Hospital for Diseases of the Skin.

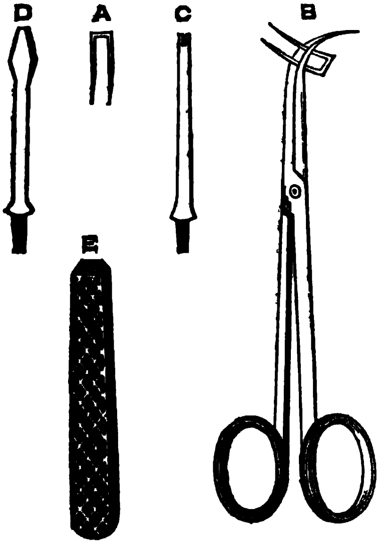
[The following method of uniting wounds has been lately adopted by Mr. Startin in some operations for the removal of scars on the face and neck, and also in fissures of the soft palate and hare-lip.]

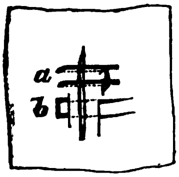
A glance at the annexed diagrams will at once render evident the nature and the simplicity of the apparatus:—

A is what may be called the Staple Needle, represented smaller and thicker than the ordinary size, though this, of course, can be varied according to the case. It is made of No. 30 steel wire, ground to a triangular or glover's point, and, with the exception of this point, afterwards softened so as to render the shank of the needle as flexible as iron wire.

B is a pair of curved forceps, with the exception of the curvature, resembling the common dressing forceps, the bent ends of which are made circular, and coated with lead, so as to meet each other by their circumferences only, and grasp firmly the staple needle, previously bent into the required curve, just above its hard or steel portion (see diagram), so as to push it through the lip or lips of the wound to be united. After the points of the needle have passed out through the skin, the forceps may be again employed to draw it into the proper position and adjust it for the fastener.

C, D, E, are the fasteners and handle; E consists of a handle into which C or D may be screwed; C is a firm piece of iron-wire, in the end of which a narrow slit or fissure is formed to the extent of the eighth of an inch; which slit, to prevent the needle's point slipping, is tinned or leaded; one or both points of the staple needle being inserted into this slit, and the fastener being carried as close as possible to the needle's place of exit from the skin, half a revolution of the instrument by the hand fastens the suture neatly, and turns one or both of the protruding points or portions of the needle over the external wound, thus keeping its edges in apposition, and preventing their everting. If, as is most convenient, one protruding needle-point has only been acted upon, the other point or portion will require to be fixed in like manner; D, the other fastener, made to screw into the same handle, is adapted for the cavities where C cannot be conveniently used, as the roof of the mouth, for example; it is made by doubling a piece of soft metal into the shape indicated, so as to form a large eye through which the points of the staple needle are severally passed, when a half turn of the handle doubles the needle over the wound, as described as being accomplished by the other fastener; this form of fastener, when it has been pushed as close as possible up to the point of exit of the needle from the skin or mucous membrane, enables the operator to exert sufficient traction upon it to close the wound, and thus assist the action of the curved forceps. A pair of scissors may be used to cut off the points of the staple needle, if required, and a little pressure with a spatula or the handle of the fastener will adjust the wires so as to make them lie evenly and smoothly over the wound.





a, *b*, Diagram showing the staple suture applied. *a*, The completed suture, *b*, The suture before using the fasteners.

To remove the Staple Suture when required, the fastener D, or C, must be used to turn the wires which pass over the wound in a contrary direction to that which fastened them: this procedure will render the staple straight again, and enable it to be withdrawn by a pair of hooked forceps or a small hook passed under the transverse or curved end of the staple.

The advantages of this needle appear to me to be, firstly, its comparatively easy application; secondly, the absence of a ligature, whether of silk or wire, to disturb or irritate the wound in drawing it through the skin, with the subsequent trouble of knotting it, etc.; thirdly, the little pain attending the application of the staple needle, and the firmer and less-puckered stitch made, owing to the shape of the needle, forming as it were, a connected suture; and, fourthly, the action of the bent portion of the needle over the wound in forming a substitute for the shield, threads, etc. in ordinary use for keeping the external edges of the wound in apposition.

In very large fistulous wounds a single firm ligature may, in the first place, be advantageously passed through the centre of the fissure before the staple suture is employed; and this may be conveniently done by the tubular needle I have before described in your pages, and which has been adopted by my friends, Professor Simpson, Mr. Spencer Wells, Mr. Paget, Mr. Curling, and others; the stitch being fastened by the leaden clamp I invented with and for this needle, and which has since been described by Mr. Baker Brown in the pages of the 'Lancet.'—*Med. Times and Gazette, April* 21, 1860, *p.* 391.

136.—*Iodine Injections in Fistula in Ano.* By M. Bonnafont.—In the more recent cases of fistula in which the revival of the old, indurated edges of the aperture by means of the cutting instrument has not yet become necessary, iodine injections may lead to a rapid and durable cure. One of the greatest objections to this plan, however, is the severity of pain caused by the iodine coming into contact with the mucous membrane of the rectum,—pain which sometimes radiates with violence over all the neighbouring parts, and gives rise to abundant diarrhœa. M. Bonnafont, of the Invalides, adopts the following procedure in order to mitigate this inconvenience. The introduction of a tent of charpie or lint passed into the rectum, often enables us to determine whether a fistula is complete or incomplete, as in the former case when we inject a coloured fluid, as ink, milk, or litmus into the external opening of the fistula, the tent imbibes some of this fluid, proving beyond all doubt the completeness of the fistula, even when a probe has not been able to be passed through the internal aperture. After having by this means ascertained the fact and the distance of the internal aperture, M. Bonnafont passes a good-sized tent into the rectum and carries it to about two centimetres above the internal orifice of the fistula. When the iodine injection is thrown into the fistula, any of it which passes through the aperture is absorbed by the tent, and thus prevented spreading within the intestinal canal; and its irritating effects are thus greatly mitigated.—*Union Méd.—Med. Times and Gazette, April* 21, 1860, *p.* 401.

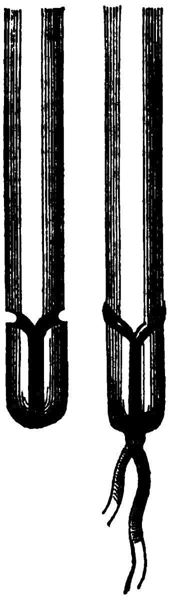
137.—NEEDLE FOR THE WIRE SUTURE.

[By R. J. Levis, M.D., Surgeon to the Philadelphia Hospital.

The present general use of the metallic suture in surgery will make acceptable a form of needle adapted to its peculiarities, and which, by facilitating its introduction through the tissues, will add to its convenience and efficiency.

For the free passage of the needle and wire, it is essential, first, that the wire be securely held; second, that it present, at its connection with the needle, no impediment to the transit through the tissues; third, that it should follow the needle in a direct line, not allowing an angle to form at the junction which will require traction to overcome. It is also important, for the convenience of the operator, that the needle should be readily threaded.

In using the ordinary surgical needle for the purpose, even though the needle be deeply grooved at the eye for its lodgment, the wire forms a ring-like attachment with the needle, which impedes its passage. A sort of hinge-joint is also formed at the junction, which will be moveable, no matter how tightly the wire be twisted, and there will be continually forming an angle with the needle, which is a great impediment to its use in delicate tissues, and in regions difficult of access, as the vagina, rectum, and fauces. Another inconvenience which has occurred with me in the use of the ordinary needle and the silver wire, is the liability of the wire to be pinched off by the forceps necessary to its introduction in the above localities.



These objections have induced some surgeons to introduce the wire by first passing the ordinary silk thread to which the wire is attached, and then drawn through.

For the purposes of overcoming these inconveniences in the ordinary use of the wire suture, I have devised a modification of the needle, which is so well illustrated by the accompanying proportionally collossal representations, as to render description almost unnecessary.

Its peculiarity is in what is usually the eye of a needle, though this is really without an eye, the attachment of the wire being accomplished by grooves, in which it rests.

A groove deep enough for the lodgment of the wire, encircles the needle obliquely near its extremity, and leads into another groove, which is vertical. The vertical grove is just wide enough at its entrance to admit the introduction of one wire at a time; but the inside of the groove being large enough to accommodate two wires, when both are introduced and twisted together they are securely held. An attachment is thus effected which is as firm as if the needle and wire formed one continuous piece, and the wire being entirely incased within the grooves it will traverse any tissue of the body without the least impediment.

There is a decided advantage in having the wire double for an inch or more following the needle, as any break in the wire invariably occurs very near to the needle.

This form of needle has been so extensively used in this city as to thoroughly test its efficiency, and it has been preferred by the instrument makers on account of its simplicity and the facility and cheapness with which it can be made.

Such needles, of sizes adapted to all uses, may be had from Mr. Gemrig, of Eighth Street, or Mr. Kolbé, of Ninth Street, Philadelphia.—*(Med. and Surg. Reporter, Dec.* 3, 1859, *p.* 224.*)—North American Review, Jan.* 1860, *p.* 176.

138.—GANGLIONS OF THE WRIST CURED BY SUBCUTANEOUS DIVISION, FOLLOWED BY COMPRESSION.

Henry H., seventeen years old, by profession a barber, presented himself at the clinic of the Jefferson college, Philadelphia, in November, 1859, on account of two small, ovoidal tumors on the extensor side of the wrist, which interfered somewhat with the motion of the joint, and the exercise of his profession.

The tumors, which had begun to form about seven months ago, were of a round shape, about the size of a marble, painless, unaccompanied by any discoloration of the skin, elastic, and movable. The patient could not remember having sprained or bruised the part at any time previous to their appearance. Professor Gross diagnosticated them to be *ganglions;* that is, small encysted tumors formed in the course of one of the extensor tendons.

In the *treatment* of the case a delicate tenotome was introduced subcutaneously, and the cysts cut up as minutely as possible. On puncturing the tumors, a yellowish, jelly-like substance escaped, and they became completely effaced. After the operation compression was made with a compress and piece of coin, confined by a bandage, and the patient ordered to keep his arm at rest in an elevated position, and to restrict himself to a light diet. The local inflammation following the operation was but slight, and soon subsided. Under the external application of tincture of iodine, in conjunction with the treatment here mentioned, the cure was completed within a few days, no trace of the swellings being left.

Remarks.—Professor Gross is inclined to ascribe the origin of tumors of this kind to inflammatory action, having observed them most commonly in hard-working people. He assumes that they are merely sacculated expansions of the sheath of the tendons, and not a new formation, as some pathologists suppose. The sac consists of a single layer, rarely thicker than the dura mater. While a common mucous burse usually secretes a fluid like ordinary synovia, a ganglion contains a yellowish, viscid, and transparent fluid, resembling white of egg, or, as in the case reported above, jelly; in old cases the contents are sometimes more solid, and consist of masses of semi-organized lymph, or small fibroid bodies, usually of the size and shape of cucumber seeds. In a ganglion of the hand, upon which Professor Gross operated a few years ago, he observed these bodies in a variety of singular shapes—round, oval, and spindle-shaped, with a number of prolongations; they were evidently merely masses of organized lymph, which had been originally adherent to the walls of the sac, and had become detached in consequence of the friction of the tendon against the sac. Ganglions are most liable to form on the extensor tendons of the hand or wrist, although they are occasionally met with in other parts of the body. Professor Gross operated, in 1854, before the medical class of the University of Louisville, upon a young woman who had two tumors of this kind, each of the size of a small bird's egg, upon the dorsal surface of the foot. There are cases on record—and one of them has come under Professor Gross's own observation—where a ganglion was situated over the radial artery, and was from the pulsation communicated to it, at first supposed to be an aneurism. Ganglions can be but seldom cured by stimulating applications; although the external use of iodine, and systematic compression may, in recent cases, sometimes produce absorption, and lessen the size of the tumor, the fluid in the cyst generally accumulates again. The best and most simple plan is to make firm pressure upon the sac, so as to rupture it, and allow the contents to escape into the surrounding cellular tissue; absorption will then take place, and may be rendered more speedy by the application of iodine; after the evacuation of the cyst, a compress and bandage should be applied to the part, to promote the union of the opposite side of the cavity by adhesive inflammation. In recent cases the rupture of the sac can be effected either by pressure of the thumb, or by striking the tumor with a large book; in cases of long standing, where the wall of the cyst is thicker and firmer, it is best to make a subcutaneous puncture. If the ganglion resists these measures, it is most effectually destroyed by introducing a bistoury subcutaneously, cutting it up into small pieces, and afterwards applying compression. It has been recommended by some surgeons to treat these cysts after the manner of hydrocele, by introducing a seton, or by injecting iodine; but the success which has attended this method does not speak much in its favor. As a general rule, free incision into the tumor, or excision of it should be avoided, as it is liable to give rise to a high degree of inflammation, the results of which might endanger the use of the joint; still there are cases on record in which extirpation has been successfully practised, and this plan of treatment has the support of old authorities, such as Celsus and Paul of Ægineta.—*North American Review, Jan.* 1860, *p.* 82.

139.—ARGENTI NITRAS FUSUS CONTAINING CHLORIDE OF SILVER.

This new form of lunar caustic is prepared by adding a definite proportion of hydrochloric acid to the crystals of nitrate of silver at the time of fusing, and pouring the fused mixture into moulds in the usual way. The proportion of the officinal acid which gives five per cent. of chlorid in the fused nitrate, is forty grains to two ounces of the crystals.

The sticks of lunar caustic thus made are of a darker colour than usual, in consequence of the more rapid effect of light upon the chlorid. They are harder, much less soluble, and of about double the strength or toughness of the pure nitrate. When fused nitrate of silver is perfectly pure, the sticks are grayish white, translucent, of a crystalline structure, and are very brittle—almost friable, and so soluble, that, in application to moist surfaces, more of the salt is left upon the tissue than is desirable.

The increased strength of the sticks in this new form, and the diminished solubility must therefore prove to be very valuable qualities in a large proportion of cases where the nitrate is used. While as yet there are no known disadvantages from the admixture.

This improvement in lunar caustic is due to Prof. J. Lawrence Smith, of the Medical Department of the University of Louisville, Ky., and an account of it was published in the 'Semi-Monthly Medical News,' of Louisville, for Feb. 1859, p. 65.

It is known in the commerce of lunar caustic, but not so well known in the medical profession, where this important article is used, that the manufacturers without any known exception sell two kinds, which are of different degrees of purity, and which are often designated on prices current as "No. 1" and "No. 2," or as "Pure" in the one case, and "No. 2" in the other. The "No 1." or so-called "pure" nitrate, commonly contains from 1 to 12 per cent. of nitrate of potassa, or other cheap salt added, as is alleged, for the purpose of toughening the sticks, and to make it run well in the moulds. The "No. 2" commonly contains from 25 to 33 per cent. of the cheaper adulterating salt. In "the trade" these two varieties are known and distinguished by the sticks being rolled in one case in white paper, and in the other in white paper of a blue tint—or, when both are rolled in paper of the same colour by the ends being sealed by wax of different colours. And when in half pound or pound bottles the figure "2," often in lead pencil, may be found in one corner of the label. These delicate distinctions do not, however, often accompany the article into the hands of medical men, and the effect is so unmistakeably bad that the writer, some time since, devoted much attention to the discovery of some simple easy test whereby the fraud might be detected. In this he was fortunately successful, and the test as now exhibited to the Society will easily and effectually discriminate between a good and bad article, and, if relied upon, will probably cause four-fifths of the lunar caustic in the hands of medical men to be rejected, taking the profession at large. A small fragment of nitrate of silver—say four times the size of a pin's head—crushed to powder with a knife-blade upon a piece of paper, the powder spread out over the paper, and the paper and powder then rolled up into a small match-like roll, set on fire, and burned, leaves a tasteless residue of pure silver. But if the nitrate contains even 1 per cent. of any saline impurity the residue, instead of being tasteless, will have the sharp alkaline taste of the base of adulterating salt. The little match burns rapidly with deflagration, and provided the quantity of the nitrate taken be not too great for the size of the paper, and provided the powder be well distributed through the paper, the carbon of the paper will in burning reduce perfectly all the silver, and in great measure the base of the adulterating salt also.

With this simple test no one need be deceived in the character of the lunar caustic he uses until some new and more fixed adulterating substances are found. The chlorid of silver in the new form does not interfere at all with the application of this test, since in that case also the residue is tasteless.—*New York Journal of Med., March 1860, p.* 165.

140.—*Treatment of Tetanus by injection of Tartar Emetic.*—[At a meeting of the Medical Society of London, when the treatment of tetanus by wourali was under discussion, Dr. Mackenzie stated that]

Mr. Ellis, in the nineteenth volume of the "Medical Commentaries," had related a case of the disease treated by the injection in large quantities of tartar emetic, two or three successive injections, containing a drachm each, having been used at intervals of four hours; the tetanic symptoms were completely removed, and the patient made a good recovery. Dr. Mackenzie then referred to a case which had come under his own care at the

Western Dispensary, and in which he had employed the same treatment, with the exception that he had added opium to the tartar emetic. The tetanic spasms much diminished, but the patient eventually sank from exhaustion. The tartar emetic seemed to develop the sedative powers of the opium in a marked degree.—*Lancet, Nov.* 5, 1859, *p.* 469.

141.—*Use of the Castor Oil Plant as a Galactagogue.*—Dr. ROUTH exhibited three preparations of castor oil leaves, a tincture and a liquor (dose of each, one drachm), and an extract (dose, five grains). The leaves were obtained from Australia; and the drugs had been prepared by Mr. Greenish, of New Street, Dorset Square, where they could be procured. Dr. Routh had read a paper on the subject of the galactagogue effects of this plant, the leaves of which, applied to the breasts as poultices, and as fomentations to the vulva, for three days at intervals, were used in Boa Vista, to induce milk in the breasts of any woman within catamenial ages, but particularly if these women had borne children, to produce milk. The milk, once produced, could be perpetuated by the simple irritation produced at the nipple by the suction of a child. These facts, related by Dr. McWilliams, had been confirmed in part by Dr. Tyler Smith. Dr. Routh had published his experience on the subject in a series of papers in the *Medical Times and Gazette* of this year. To lying-in women with a deficiency of milk, he had given the infusion, in combination with couger-eel soup; and the effect in determining a copious flow of milk has been remarkable. He had administered the extract to unmarried women within catamenial ages, and the effect had been to produce intense pain in the breasts; but, as he could not find anybody, in that case, who would try the effect of a child, he had not yet induced milk in the breasts of such; but, after three or four days, the symptoms were relieved by a copious leucorrhœa. As it was possible that a larger experience of this remedy might enable us to convert some married women (within catamenial ages) into wet nurses, and as it undoubtedly acted as a powerful lactagogue in suckling women, he was anxious that others also should experiment, and therefore to direct them where it could be procured.—*Brit. Med. Journal, Dec.* 17, 1859, *p.* 1,030.

142.—ON THE ACTION OF SOME OF OUR ORDINARY MEDICINES.

By C. HANFIELD JONES, M.B., F.R.S., Physician to St. Mary's Hospital.

There are few drugs (would that they were more numerous) which a large experience justifies us as endued with the power of arresting *sthenic* inflammation, when set up in certain tissues. Thus *antimony* arrests pulmonic and renal inflammation; *mercury*, iritis and peritonitis; *chlorate of potass*, buccal inflammation; *iodide of potassium*, periosteal; *colchicum*, synovial. Such remedies I have proposed to term tissue-sedatives, believing that, whatever physiological action they may have, their peculiar efficacy in disease is quite independent and apart from it. Our best knowledge respecting colchicum (Garrod), respecting antimony (Ackerman, 'Brit. and Foreign Medical Review,' April, 1859), and respecting iodide of potassium or chlorate of potass, does not explain to us in any measure the cause of their acting on certain parts and not on others, or how they produce a cessation of the inflammatory process.

Alkalies, including their vegetable salts, it seems probable to me, belong also to this class. They are efficacious in some cutaneous inflammations, as psoriasis; in some inflammations of the urinary passages, some bronchial and laryngeal inflammations, and in acute rheumatism. In the latter disease, it has been presumed that their beneficial effect depends on their neutralising the excessive acidity of the system. This, however, seems very questionable—(1) because it is not at all proved that there is any excessive acidity or diminished alkalinity of the blood; (2) because the alkalies fail to benefit in cases of rheumatism of asthenic and apyretic character; (3) because other remedies, of a totally different kind, as colchicum and aconite, occasionally cure acute rheumatism; (4) because alkalies are of no avail in the visceral inflammations of rheumatism; (5) because neutral salts, as the nitrate and acetate of potash, are curative of rheumatism, the former of which can exert no alkaline effect, while the decomposition which ensues of the acid of the latter proves that the system in acute rheumatism still retains its power of assimilating or digesting acid. and, therefore, that it is reasonable to suppose that it would also digest lactic acid, the presumed *materies morbi*.

Lead, in the form of strong Goulard lotion, has so marked a sedative effect on an inflamed cutaneous surface, that it seems to me to belong to the class of tissue-sedatives as much as to that of astringents. It is certainly a more sedative application than tannin, but, like it, exerts also an astringent action on the capillaries, which renders it efficacious in hemorrhages not depending on actual ulceration, and in serous profluvia.

Another class of medicines may be distinguished of an opposite kind to the preceding. These are the nerve-tonics. Among them I include quinine, arsenic, strychnine, digitalis, and to some extent, iron. Their action, so far as we trace it, is not eliminent in any degree, nor depressing, but invigorating. We conclude, that the nervous system is mainly influenced by these remedies, because they seem to operate much in the same way as exciting and invigorating mental influences, which can only act through the nervous system; because, while there is an evident resemblance of therapeutic effect in all, two of them, (quinine and strychnine) do operate most undeniably on the nervous system, and because they are all found beneficial in disorders whose chief character is depression of nervous power. If we consider the above-mentioned drugs separately, we shall, however, see that they do not all act alike.

Quinine seems to exert its influence both on the cerebro-spinal, and visceral sympathetic nerves, it slows the heart's movement, and prevents the hyperhæmic affluxest hat take place in malarious disease from vaso-motor nerve paralysis. In this latter case, as well as in the case of neuralgia, the beneficial effect probably depends on the nervous tissue being rendered more vigorous and strong, more able to resist injurious influences. On the same principle we must explain the protection conferred upon the system by the use of quinine in respect of malarious miasms. The protected system resists a poison it would otherwise succumb to. Such facts as these show positively that quinine is not an antiperiodic merely, as some are in the habit of calling it.

Arsenic, in many respects, has a similar action to quinine. Its efficacy in malarious disease has long been known, and has lately been especially tested by M. Boudin, in Algeria. In various neuralgiæ it renders good service, particularly in obstinate sciatica I have much reason to be satisfied with it. It exercises a very marked influence over the vaso-motor nerves of the skin in the cure of eczema and other vesicular eruptions, over the intestinal in some cases of chronic diarrhœa, and over the uterine menorrhagia. It acts as a tonic to the musculo-motor nerves in chorea, and to the bronchial in some cases of asthma, rendering them less liable to be thrown into spasmodic action. It is a remedy of much slower operation than quinine, and much more liable to cause tissue-irritation. It is therefore especially important if we employ it in inflammations, to be sure that the morbid action has lost its sthenic character.

Strychnine is notorious for its especial action upon the musculo-motor apparatus, affecting particularly the spinal cord. Professor Kölliker asserts it has no action upon the brain, but I am strongly disposed to question this statement from observation of its therapeutical effects. Trousseau and Bardsley both speak positively of its curative influence in cases of hemiplegia, and I have recently had myself a well-marked instance of recovery from facial paralysis under its use. In some cases of amaurosis, there seems no reason to doubt that strychnia has proved curative. In many cases where it has failed, it is very probable that the condition was incorrectly diagnosed, and that there existed grave structural lesions. From some personal experience of it, I should say that it acted certainly as a cerebral tonic, invigorating the hemispherical ganglia. In chorea it has often a very beneficial effect, steadying and invigorating the motor nerve apparatus. Its action on the sympathetic is demonstrated by its effect in arresting some forms of vomiting and chronic diarrhœa, as well as passive menorrhagia, and (as discovered by Dr. Vernon) in exciting the gravid uterus when inert to vigorous contraction. Its beneficial effect in ague and neuralgia has been observed by Mr. Wilkinson, and is quite in consonance with the view of its acting as a general tonic of nerve-centres, not only of the spinal.

Iron, in some of the various forms in which we administer it, has manifestly a nerve-toning and steadying action. Carbonate of iron is an approved remedy in neuralgia. Tincture of muriate of iron positively asthenic hyperæmias and inflammations, in fact, such controls states as we may fairly suppose to involve a paralysed condition of the vasomotor nerves, with languid tissue power. I can confirm, from my own experience, at least to some extent, the favourable reports given by Mr. Bell, of the excellent effects produced by tincture of sesqui chloride of iron in erysipelas and desquamative nephritis. The analogous pernitrate salt has a similar effect, according to Graves, in some cases of chronic diarrhœa. The efficacy of tincture of sesqui-chloride of iron in spasmodic stricture of the urethra is a remarkable fact, illustrating the special affinity of particular remedies for particular systems of organs, and the control of a state of spasmodic contraction by an agent which is neither contra-stimulant nor sedative. Recently I have observed a remarkable instance of most severe nocturnal spasm of the muscles of the back and loins, which was markedly controlled by the free administration of quinine and stimulants. Such instances show that in many cases it is quite possible for muscular spasm, as well as neuralgic pain, to depend *au fond* on a feeble and consequently over-excitable state of the nerve or nerves involved.

The knowledge we possess of the action of *digitalis* seems to justify us in ranking it as a nervine tonic. It slows the action of a weak heart, and at the same time strengthens the contractions, so that the pulse increases in force. It acts in the like manner with the uterine blood-vessels in passive menorrhagia. It stimulates the renal tissue to increased activity. In all these therapeutic actions, it appears as a stimulant or tonic. But it may be used also to diminish the excessive action of the heart in inflammatory fever and hypertrophy. In this case, at the same time that it slows the action of the heart, it renders the pulse feeble, and often irregular, and syncope is imminent. It is probable that in this case the arrest of action depends upon an inhibitory effect, the excessive stimulus producing an opposite effect to that occasioned by a moderate one. The result of Mr. Lister's experience is to the effect "that in a healthy state of the nervous system, very gentle irritation of the vagus increases the heart's action, while a slightly stronger application diminishes the frequency and force of its contractions." The following quotation from the same highly interesting paper, bears precisely upon this point. "When partial exhaustion has occurred, a much stronger galvanic stimulus is required to produce the same effect upon the heart, than at the commencement of an experiment; and thus an action of the battery which, when first applied, causes marked diminution in the number of beats, may after a while come to have the opposite effect, and increase the heart's action as decidedly as it had previously lowered it; while at an intermediate period it may seem to have no influence at all." Just in the same way digitalis tones and strengthens the action of a feeble heart, but lowers that of a vigorous one.

The only narcotic remedy that it seems necessary to consider with regard to the therapeutics of fever and inflammation, is *opium*. One remarkable circumstance is the extraordinary tolerance evinced by the system under such disease, of large doses of this drug. Patients in rheumatic fever will take as much as twenty-four grains in the twenty-four hours, without any notable result. In puerperal peritonitis, the same quantity has been administered by Dr. Churchill, with the best effects. Its action on the cerebro-spinal nervous system is evident in the calmer and quieter condition of the sufferers, it renders the sensorium less impressionable, and the system less liable to be exhausted by the disease. Its action on the sympathetic and vaso-motor nerves is probably similar, it will relax contracted arteries, and admit a freer transit of the blood. This is probably the *rationale* of its beneficial effects in the cold stage of ague, and in similar conditions; it does away with the contraction of the superficial vessels and skin. In many choleraic attacks which have much resemblance at first to an attack of ague, a dose of opium and chloric ether is of the greatest efficacy. So in exposure to severe cold and in gangræna senilis, the beneficial effect of opium probably depends on its preventing the arrest of the circulation in the limbs and superficial parts, by keeping the arteries, especially the smaller vessels, relaxed and patent. At the same time it protects the heart from the depressing inhibitory influence of the cold, and so enables the circulation to be well sustained. The latter remark will also apply to the case of peritonitis, and of the shock from burns. The sudorific effect of opium probably depends also on its relaxing the cutaneous arteries. In sthenic inflammation, we have no evidence to show that opium, *per se*, is of any avail. But in asthenic, it certainly seems to exert power in controlling exudation. What else can be said of the action of this drug in diarrhœa, but that it arrests the outflow of watery fluid through the mucous membrane? In common nasal catarrh, it has in some persons the same effect. Its "locking up" the secretions of the liver and other glands, is an action of the same kind. In all such cases we may presume that the drug affects the tissue of the part in a sedative manner, analogous, if not identical, with its action on the nervous tissue or the arterial contractile. It is therefore a tissue sedative. If the action of the heart be in danger of failing from asthenia, the administration of opium, in my opinion, is unsafe, from this very sedative effect. Yet where the depression is the result of violent irritation in another part, as for instance, in peritonitis, the action of the heart may greatly improve under the use of opium. The narcotic, by rendering the cardiac ganglia less impressionable, takes of the inhibitory effect of the peritoneal irritation.—*Brit. Medical Journal, Dec.* 17, 1859, *p.* 1018.

143.—ON OXYGEN AS A THERAPEUTIC AGENT.

By S. B. Birch, M.D.

[The author, after a few preliminary remarks, passes on to give a brief outline of the main results hitherto obtained from experimental research on the above subject.]

Dr. Gairdner's experiments on rabbits go to prove that the inhalation of pure oxygen increases the fibrine, and diminishes the corpuscles and albumen of the blood.

Dr. B. W. Richardson, following out this idea, in his admirable work on the *Cause of the Coagulation of the Blood*, details some experiments proving that a condition of hyperinosis, with tough fibrinous concretions in the blood, is induced by prolonged inhalation of pure oxygen.

Mr. Savory, *On Animal Heat*, states that he has found the temperature to be lowered, not increased, by an extra amount of oxygen. My own experience, however, tends to the conclusion that in healthy well-fed animals the animal heat is usually first increased, then lowered; while clinical observation has afforded satisfactory evidence that oxygen, when employed in disease, will raise or lower the temperature *under different circumstances*.

Lastly, I would draw special attention to the highly interesting experiments of Mr. Erichsen, detailed in his monograph on *Asphyxia*. He says:—

"In a considerable number of experiments that I have performed on this subject, I have never succeeded in re-exciting the contractions of the ventricles by means of the inflation of the lungs with common air, provided they had fairly ceased to act before artificial respiration was set up." He was then led to try oxygen, and in several experiments was successful in restoring the action of the ventricles after the entire cessation of the heart's action.

It would be tedious, and needlessly taking up valuable time, to give more than an epitome of such facts. To comment upon them *in extenso*, would at present be equally injudicious. I simply wish to intimate that physiology certainly does not negative oxygen, in a therapeutic point of view. With a few remarks, as brief as possible, I will pass on to the purely practical portion of this paper.

When we reflect on the acknowledged difficulties and trouble, and probably many fallacies, attendant upon tedious and lengthened experiments; when these are associated with at least uncertainty regarding facts based on chemical researches; when our still imperfect knowledge of animal chemistry in connection with the vital dynamics and the generation of nervous force is taken into due account; when the fact stands forth that we can, if we please, by means of pure oxygen, induce a state of hyperarterialisation and hyperinosis; when, further, Mr. Erichsen's experiments are brought into the field of mental vision; it must be acknowledged, that the *dicta* of even the most accomplished chemico physiologists as to the uselessness of oxygen in medical practice, ought to be received with extreme caution. Then add to this the recent views put forth by Professor Schönbein, which a few weeks ago were made the subject of an interesting lecture by Dr. Faraday, at the Royal Institution, and the therapeutic question becomes of still deeper import. It may now be fairly inferred that oxygen can exist in not fewer than three allotropic conditions—ozonic, antozonic, and neutral; the two former even possessing the power of assuming opposite polarities with regard to each other. This interesting discovery is highly suggestive, and opens out a new field for observation and investigation, which, it is earnestly to be hoped, may assist to elucidate this at present obscure inquiry as to the cause of the frequently unquestionable potency of oxygen, when employed in suitable cases *in very small quantity beyond atmospheric proportion.*

Without risking shipwreck upon an obvious yet somewhat vague hypothesis, nearly allied to that so energetically proferred by that acute old physician, Dr. Stevens, but partially illuminated by the discovery of Schönbein, I will here simply trust that sufficient obscurity and doubt exist in the scientific world, on this interesting subject, to afford ample apology for my differing from authority, while on the present occasion I deviate but very slightly from the practical experience of the clinical observer.

Whatever be the true *modus operandi* of small doses of oxygen (irrespective of the purest atmospheric air), when judiciously exhibited, in many intractible forms of disease, abundant evidence can be brought forward to prove beyond moral doubt that, in certain lowered conditions of the vital forces, this gas can not unfrequently exercise an alterative and tonic influence upon the entire animal economy, which no other medicinal agent at present known can exert. And further, waiving the therapeutic question instances, will from time to time present themselves, during extended employment of oxygen in practice, where certain unexpected, curious, and sometimes unpleasant and serious effects, will so immediately and decidedly manifest themselves as to render it impossible for even the most incredulous observer to impute, *post hoc ergo propter hoc*, expectancy, etc., without ignoring truth altogether.

One mode of casual and limited investigation, for the purpose of testing its action on the system, has often struck me as a fruitful source of error; viz., impulsive trials made by my professional brethren upon themselves and upon their friends, *in a state of health*, or at least where there is absence of any actual disease requiring its use, or of that susceptible condition occasionally met with, through disease or natural temperament, in which a *nidus*, as it were, is presented for the exhibition of abnormal sensitiveness to the action of the gas. Now, oxygen seldom evidences its special influence to any marked extent in persons enjoying perfect health, although rare exceptions are met with; and perhaps it may not be amiss here to add *en passant*, that this fact ought not to be lost sight of, when considering the therapeutic relations of oxygen in derangements of the animal frame. Be it understood, also, that my advocacy of oxygen is exclusively confined to disease otherwise incurable, imminently dangerous, or very intractable.

In employing oxygen in different diseased conditions, it appears necessary to regard its action from two aspects:—1. The alterative and tonic influence which it can exert on the nervous system (apparently irrespective of immediate chemical action *per se*, in the ordinary acceptation of the term), when exhibited in very small doses, and for a very limited period daily. 2. The augmented activity of the normal chemical changes in the animal organism directly induced by bringing a largely increased proportion of the gas in contact with the pulmonary cells; the duration and frequency of the inhalation being, in this case, an important consideration.

So far as relates to the first suggestion, I have, as before stated, had ample opportunities of observing that oxygen can in many susceptible temperaments, and in certain diseased conditions, exercise a peculiarly powerful influence, rarely met with in persons enjoying perfect health; and, by taking advantage of this peculiarity, many cases will quickly undergo a change for the better, where the most enlightened and judicious treatment had previously failed to produce any beneficial effect. In such instances, the gas ought to be used with great caution; for I have known serious and unpleasant (not to say alarming) symptoms arise from what would usually be regarded as a ridiculously small per centage added to atmospheric air. Most of these examples have occurred in sensitive nervous systems—in individuals possessing a very susceptible nervous organization; or otherwise in those affected with lesions of the brain or spine, from disease or injuries. Fanciful imagination, hysteria, *et hoc genus omne*, may very naturally create misgivings in the minds of those who have not personally witnessed such phenomena. Willingly do we grant a liberal discount to the incredulous, while we add that several of the best exemplifications have presented themselves in men of powerful frames, ignorant of the increase or diminution of the doses that they were being subjected to, and not only devoid of all apprehension of the treatment, but in the highest degree surprised that 'a little more pure air" could exert such a perceptible action on the system. The principal symptoms of a disagreeable character, here referred to as occasionally resulting from extremely small doses, are, a sense of constriction of forehead and temples; a feeling of weight over the centre of the parietal bones, and in the occiput; a rush of blood to the head; fulness, pain, or oppressive sensation, in the nape of the neck and base of the brain; sudden faintness, palpitation of the heart; spasmodic contraction of affected parts, *e.g.*, violent reflex movements in extremities affected with paralysis of voluntary motion. Moreover, I have seen, on two or three occasions, a state of unnatural excitement of the entire nervous and vascular systems, which has continued for several successive days after one moderate dose. The chief symptoms of a disturbing character observed from pushing very large doses of the gas, are, in thin anæmic persons, sudden or gradual disappearance of pulse, pallor of countenance, coldness, and partial collapse; in the plethoric and sanguineous, the reverse—viz., too excited circulation; full bounding pulse; intense heat of head, face and skin; severe, oppressive headache. I have also known the frequent and long continued exhibition of it, when not duly superintended, cause much emaciation.

For individuals, however, to be obnoxious to the extremes of the foregoing symptoms, is only occasional.

On the other hand, certain beneficial effects of oxygen may be mentioned as not unfrequently immediate and well-marked, where due judgment has been exercised in selecting cases, and in directing the doses and duration of the inhalation. Such are, complete relief from excessive oppression of the brain; sight improved in defective vision consequent upon venous congestion; genial warmth, even to the ends of the toes and fingers succeeding to extreme chilliness and collapsed condition; sudden departure of great nervous depression; permanent relief afforded to the uterus, ovaries, and spine, by sudden induction of long suppressed catamenia, particularly at the change of life, unexpected diarrhœa, of highly offensive character, with dark inspissated bile, in long continued torpor of the liver and portal system; cutaneous transpiration suddenly and freely produced. To guard against any possible misapprehension from dwelling upon these special and important points, it must be distinctly understood that, in very many cases, the beneficial alteration effected by oxygen takes place with no characteristic signs of action perceptible to the patient, or even to the practitioner, except general improvement in the constitution, such as may be observed in many debilitated frames undergoing a mild course of chalybeates. Speaking generally, there will be found, in suitable subjects for the treatment, improved appetite and powers of digestion and assimilation, a feeling of being much more "up to the mark," less lassitude, more ability to bear physical exertion, and (that to which ladies are preeminently partial, and some of the ruder sex not less so) a clearer, fairer, and softer skin.

The semeiological and pathological indications for the employment of oxygen, with a view to cure or palliation, must next receive some general consideration; and afterwards will follow certain points contraindicating its use.

The deviations from health in which it will be found most beneficial are those where there has been no very considerable reduction of what may be called intrinsic vital power. Depression or oppression may be extreme; the nervous and vascular system may be incapable of receiving more than temporary tone or stimulation by means of ordinary tonics, stimulants and attention to the best sanitary and dietetic rules; but, as a rule, there must not be present that permanently lowered condition induced by long continued, insidiously undermining nervous debility (so well known to us all), which, having become almost a second nature, has incapacitated the system for "making life" beyond such amount as is absolutely essential for the maintenance of Psyche in her terrestrial abode.

In some constitutions, with blue noses, conjected conjunctivæ and scleroticæ, semi stertorous breathing on the slightest exertion, hemorrhoids &c.—all vividly depicting to the mind the internal state of the matters—each dose will often cause a slow, laboured, full, but very compressible pulse, to become quicker, firmer, smaller. In others, with a small, weak, quick, and even irregular pulse (in the absence of much excitement, with hyperæsthesia), the quick conversion into a slower, fuller and firmer pulsation, is sometimes equally well marked. Examples of each (modified, of course, as to rapidity of production and duration of effect) have frequently occurred in my practice.

The diseases *par excellence* in which the gas has afforded me the most gratification are those attended with either local or general venous congestion,—a preponderance of the venous over the arterial, and torpidity of the capillary circulation. The good effects have been, as a rule, most decided in persons of a gouty or strumous habit, or otherwise in a state of general *malaise*, with sluggish circulation, either constitutional or superinduced by an atonic and oppressed condition through over-feeding and other luxurious or indolent habits, so prevalent in these artificial days.

The organs, which, specially effected, experience the most immediate and sensible advantage from oxygen, are the brain, lungs, liver, and spleen, including the entire portal and mesenteric system, and uterus and ovaries.

In plethoric habits, with chronic local or general congestion interfering with the functions of one or more of these organs. the commencement of the curative process has sometimes been ushered in by sudden and unexpected efforts of nature to throw out peccant matters—efforts for so long a period previously unattainable through the most judicious treatment, and so immediately following two or three large doses of the gas, as to afford almost unquestionable evidence of true sequence. The assistance urgently demanded by the system has been given; nature has thus had a starting-point, and a critical discharge has made its appearance. It has been my fortune to see this exemplified in cases of long-suppressed catamenia, in torpidity of hepatic functions with pent-up biliary secretion (as evidenced by sudden diarrhœa of most offensive character), and in gouty affections with much cerebral, nephritic, and other distress. In the last, I have known the urine, which had been for many weeks uniformly clear and limpid, become, to the horror of the patient, turbid, dense, and loaded with urates and phosphates.

I would venture particularly to draw attention to the ascendency of oxygen, over the cutaneous capillaries, not unfrequently evidenced from the commencement of its use in torpid and unhealthy conditions of the cutaneous function and cachexia arising therefrom. The benefit afforded will occasionally demonstrate itself in profuse perspiration, when a dry, harsh skin had been previously the order of the day; or a relaxed and moist state of the cutis, with constant chilliness and liability to colds, will give place to a warm, healthy, and comfortable state, to which the invalid had been long a stranger. With reference to extremes which occasionally present themselves, it may be added, although any prompt improvement or disappearance of certain eruptions, previously almost untouched by the treatment, will usually evidence the *suaviter in modo* as well as the *fortiter in re* operation of oxygen; yet I have met with a few instances where ladies have been disagreeably surprised to find some slight but chronic eruption become very unsightly after a few days exhibition of the gas. To calm the female mind is not very easy under such circumstances; but a bold assurance of the exacerbation being merely temporary—an effort of Nature to endeavour to rid the system altogether of the disease—has thus far not been falsified by the result.

Even inveterate skin-affections the history of which points to a congenital origin, and incurable in the permanent sense of the word, may nevertheless receive much benefit from an occasional resort to this remedial agency.

Rapidly spreading ulceration, with sloughing nearly allied to gangrene, has under my observation been suspended solely through the influence of oxygen in a few liberal doses. A similar result has followed its employment in malignant anthrax, the most serious symptoms being brought under permanent subordination. And, indeed, I firmly believe, with oxygen as an adjunct, surgeons might regard with much less dread the frequently alarming and even fatal supervention of erysipelas or gangrene in traumatic cases. Conjoined with proper sanitory measures and the usual treatment, abundant stimuli, beef-tea, cinchona, ammonia, &c., the inhalation of the gas, might, in some cases of the worst description, prove the salvation of the patient. Perhaps this suggestion might not inaptly be extended to the notoriously fatal effects of badly located hospitals.

The space to which I must necessarily limit myself in this paper, will admit of little more than a glance at those pulmonary and cardiac affections, in which oxygen merits cautious, careful, and judicious trial. I will simply remark, that the palliative effects of oxygen have kept hopelessly phthisical subjects in comparative comfort and freedom from dyspnœa and exhausting cough, up to their dying hour; while the necessity for opium and other sedatives has been very trifling, and comfortable sleep has been secured, *minus* any disagreeable feelings following the free use of the medicines. I know a lady in London who, with fatty degeneration, and probably attenuation of the muscular walls, and enlargement of the cavity of the heart, has been kept alive for the last three years by a dose of oxygen almost daily. On many occasions, in this lady's case, the heart's action, capable of only partial and temporary restoration by brandy and other stimulants, has at once been restored by oxygen in large doses. She lives by exercising great care and avoiding great physical exertion, to a certain extent enjoys life, yet cannot do for more than a few days successively without the renovation afforded by the gas. Again, in asthma, there is a tolerable percentage of cases in which oxygen can either cure or greatly relieve. As a rule, the subjects most benefitted are those in which we can trace no congenital or hereditary predisposition, and where the disease owes much of its origin to chronic bronchitis or partial congestions and indurations of the pulmonary parenchyma.—*British Med. Journal, Dec.* 24,1859, *p.* 1033.

144.—ARTIFICIAL ANÆSTHESIA.

[M. Azam, of Bordeaux, has lately published the result of some experiments which appear at present to be exciting much interest in Paris. The foundation of these is a work published in this country by Mr. Braid, in 1842. This gentleman's views on the subject were embodied in a paper in the Retrospect, but we think that it will refresh the memories of our readers, to give the subjoined remarks.]

The patient, either sitting up or lying down, is put in a convenient position. The operator then, standing either before or behind him, places before his eyes, at the distance of a few inches, but generally nearer than the point which allows of distinct vision, some bright object, upon which the patient should steadily and continually fix his eyes. The bright object should be so placed that the eyes, in looking at it, must be forcibly directed upwards, the contraction of the superior recti being carried to its maximum degree. In this position, the levatores palpebrarum and recti are strongly contracted, and convergent strabismus takes place. After this attitude, which is certainly fatiguing, has been kept up for two or three minutes, the pupils are noticed to contract, and soon afterwards to dilate; the eyelids quiver rapidly, then fall, and the patient is asleep. Two symptoms, almost always, present, are then observed; they are, however, in different cases, more or less marked and lasting: 1, catalepsy, exactly as described in books; 2, anæsthesia, which lasts from three to fifteen minutes, either complete or incomplete, but which allows of pinching, pricking, and tickling, without any feeling being aroused in the patient, and without any change in the cataleptic state being produced. This anæsthetic state is generally followed by a very opposite condition—namely, very remarkable hyperæsthesia, in which the senses, the feeling of heat, and muscular activity reach an unusual degree of excitability. At any moment of the experiment the symptoms may suddenly be stopped, by rubbing the eyelids and directing upon them a stream of cold air. When the patients recover their senses, they remember nothing of what has taken place.

Several experiments have been instituted in Paris by Messrs. Follin, Broca, and others; and M. Velpeau seems so convinced, that he has presented a short paper on the subject, by M. Broca, to the Academy of Sciences at the meeting of the 5th instant.

The *Gazette Hebdomadaire* of the 9th of December mentions the following case:—A woman, aged twenty-four, rather nervous and timid, had, in consequence of a burn, a large abscess by the verge of the anus, and was told that she would be narcotized before it was opened. A bright brass tube (a telescope made by Bruecke) was placed five inches in front of the nose. The patient was obliged to squint considerably in order to look steadily at the object, the pupils contracting very strongly. The pulse, which before the experiment was quick, became now quicker, but immediately afterwards weaker and slower. After a couple of minutes the pupils began to dilate, and the left arm being artificially lifted up vertically above the head, remained motionless in that attitude. Towards the fourth minute the answers became slower and almost painful, but perfectly sensible, and the respiration slightly irregular. At the end of five minutes, M. Follin pricked the skin of the left arm, which was still held up at a right angle with the trunk, but the patient did not move. Soon afterwards a puncture was made, which drew a little blood, but no feeling was evinced. The right arm was now placed in the same attitude as the left, and the region where the abscess was situated brought into view. The patient yielded willingly, saying, very quietly, that she was doubtless going to be hurt.

Finally, about seven minutes after the beginning of the experiment, M. Follin laid the abscess largely open, and freed a great quantity of fœtid pus. A faint cry, which lasted less than a second, was the only sign of reaction which the patient gave. No movement of the muscles of the face or the limbs was observed; and the arms remained in the same cataleptic state which they had previously assumed. Two minutes later, the attitude was still the same; the eyes wide open and a little vascular; the face motionless; the pulse as it was before the experiment began; the breathing quite free; and the patient insensible. The left heel was now raised, and it remained unsupported in the air, whilst the cataleptic state of the arms persisted. M. Broca at this period removed the bright object which had hitherto been constantly kept before the patient's eyes, gently rubbed her eyelids, and directed upon them a current of cold air. She now made a few movements, and was asked if she had felt anything; upon which she answered that she did not know. Both arms and the leg remained, however, in the artificial position in which they had been put. At this stage the left arm was again pricked, and no sensation thereby excited.

Eighteen minutes after the beginning of the experiment, and twelve after the operation, another friction on the eyelids and another current of cold air was used; whereupon the patient awoke almost suddenly, the cataleptic limbs all falling together. The patient then rubbed her eyes, came to her senses, remembered nothing that had passed, and was surprised that the operation was over. Her state was somewhat analogous to that of patients who wake from anæsthesia induced by ordinary means; though the waking was certainly more sudden, and without agitation or talking. The anæsthesia which had thus been artificially interrupted, had lasted from twelve to fifteen minutes.

Two attempts of the same kind have been made by MM. Azan and Follin, in the same hospital, upon a girl aged eighteen, who was affected with a slight wound of the foot; but the results have not been so satisfactory as they were in the last case. Two other experiments which were undertaken by M. Azam on the 8th instant were more successful. In a young woman, catalepsy began in a minute and a half, and in two or three minutes both catalepsy and anæsthesia were complete. With another woman, suffering from cholera, anæsthesia was well established in less than two minutes. A third experiment was tried in the presence of M. Trousseau, upon a girl who had been for some time in the hospital for epileptic vertigo. In a minute and a half by means of a pair of scissors held ten inches from the eyes, she was cataleptic and asleep; and when awakened, she complained of lumbago and much fatigue; altogether she remained in a state of hebetude and stupor much longer than happens after recovery from epileptic attacks.—*Lancet, Dec.* 17, 1859, *p.* 620.

145.—POISONING BY CARBONIC ACID GAS, WITH RECOVERY.

By J. B. Thompson, Esq., L.R.C.S., Edin., Resident Surgeon, General Prison, Perth.

[The subject of this report, a man of twenty years of age, was employed in his prison cell, picking oakum, when an accidental spark igniting the teazed material, the cell was immediately filled with smoke. After ineffectual attempts to extinguish the flame, he fell down stupified. An official hearing a cry made by the man opened the door within a minute and a half, when "the prisoner walked out of the cell without help."]

The prisoner assisted to put out the flames, which was done in a few minutes. He spoke to the officer, and explained how the fire originated. He walked about the length of 150 yards, carrying water in a pail. In about five minutes from the time of issuing from his cell, the prisoner sat down upon a stool, became sick and stupid, and fell on the floor in a state of insensibility. He was raised up and replaced upon the stool, had a drink of water;

and his face and hands washed; and a second time he fell on the floor, was seized with retching, foaming at the mouth, spasms, and total insensibility.

The following is the description of the patient's condition from my notebook at the time:—

Symptoms—Nov. 9th, 5.50 p.m.—Within twenty-five minutes after the accident of the fire I saw A. S. He was lying prostrate, rolling his body from side to side, tossing about his arms, convulsed as if by tetanus; when his body was set up, he showed an utter want of muscular power. The face was pallid, eyes shut, pupils fixed and contracted. His head, when lifted up, was drooping and fell down; retching constant, vomiting of frothy saliva in large quantities; pulse, full, languid, numbering 68; total insensibility. The vomitings were darkish, and contained carbonaceous matter. When raised to the upright posture, he had no locomotive power, and when pinched no sensation of pain.

Treatment.—I had him exposed freely to a current of fresh air, made him inhale ammonia, and bled him from the arm to the extent of ℥x. Cold affusion was applied to the surface of the body.

The blood appeared somewhat darker than normal.

The symptoms continued as described until past midnight without amelioration: during which interval he had leeches viii. applied to the temples and a purgative enema; cold applications to head. 12 *midnight.*—The retching and vomiting of frothy saliva, with stupor, unabated.

November 10th. 7·20 a.m.—Pulse 70, full and labouring; respiration oppressed, lays his hand on his breast as if to express his labour in breathing; respiratory murmur dull; cannot speak, cannot swallow anything; comatose The vomiting of frothy matter has partially subsided since midnight. A blister was applied to the chest.

This condition remained with some slight improvement throughout the day. The frothing at the mouth recurred for two or three hours occasionally; the stupor seemed less at times, then returned as profound as ever. Still some carbonaceous matter in the vomitings. On examining the nostrils and fauces, I found them much inflamed; at times made signs by opening his lips, but spoke none. Tried to swallow, but with evident pain and sense of choking; and the irritability of the stomach continued such, that, if anything was swallowed it was at once rejected.

Nov. 11th and 12th. With slow but steady amendment, all the symptoms become more favourable; cannot remember what occurred at the fire.

Nov. 13th. He was able to take and retain nourishment; having been previously kept up by nutritious enemeta. He now speaks a word or two and sinks down into a partial stupor. Complains of great pain in the nasal passages and the fauces, which are still slightly inflamed.

Nov. 14th. He is now so far well as to sit up a little; much reduced in strength, and, as it appears to me, his mind, never strong, rather enfeebled. Does not remember assisting to put out fire in cell.

Some of the features of this case seem very remarkable:—

1. *The interval from the time of exposure to the carbonic acid gas and its effects* was considerable. I have taken some pains to make out, as near as possible, the length of time that elapsed before the stupor and sickness appeared, and there must have been *at least five minutes*. During this interval, was the poison working its way into the system? In assisting to put out the flames, he may have imbibed more smoke, but this cannot have been of any conseqence.

Among the cases of poisoning from this gas, we find that the miners who worked in the burning mine belonging to the Devon Company sometimes wrought for a length of time before they were affected; and the only test of the poison being concentrated and dangerous was the effects produced. The air was pure enough to support light, but yet injurious to life, by its gradual and cumulative effect upon the system. In the black lung of miners I am satisfied that carbonaceous matter, in many instances, is thus *ab extra* carried into the circulation; and, from its extreme solubility in the blood, there is no difficulty in believing this theory. M. Nysten found that 64 cubic inches of this gas may be absorbed without proving injurious. It thus appears that the effect of carbonic acid gas is not always sudden and immediate; but by repeated and cumulative doses, it may, after a longer or shorter period of exposure to its influence, be absorbed into the system, and act as a positive poison.

2. The effects were decidedly narcotic. The symptoms of stupor, laborious circulation and respiration, with convulsions, all indicated deep narcotism. For several days after complete recovery, the patient remembered nothing of what occurred during the interval from leaving his cell until he became insensible.

3. The excessive frothing from the mouth was the most rare and striking feature in this case, and seems to be characteristic of poisoning by various of the gases. The amount of saliva discharged was beyond anything I have seen in the same space of time from the most severe salivation.

The history of this case seems to leave no doubt that carbonic acid gas was gradually absorbed into the system, and acted as a *positive poison.*

That gases may permeate freely through the human body cannot be doubted. We see this in cases of injury to the lungs when emphysema occurs and inflates all the tissues, the air again passing out by the lungs and skin. It is also well known that a stream of carbonic acid gas made to play upon the membrane of a frog's foot, stagnates the capillary circulation there. Is this a chemical or a mechanical effect? Is this asphyxia proper, or is it the action of a positive poison on the blood? The latter opinion is all but proved.

Frequent and fatal accidents occur from the feather-white wine (*der feder weisse Wein*), which from the heat disengaged in the stomach, evolves carbonic acid gas in large quantities by fermentation, and penetrating through the stomach, diaphragm, and other membranes, reaches the blood, and produces the very symptoms which occur in the case before us, and often ends fatally.

All such cases are explained more readily than by any other theory, when we assume the doctrine that carbonic acid gas acts as a narcotic poison.—*Edinburgh Med. Journal, Jan.* 1860, *p.* 642.

146.—ON IDIOSYNCRASIES.

By M. Claude Bernard, Member of the French Institute; Professor of General Physiology at the Faculty of Sciences.

All the phenomena which take place in the living body, whether in health or in disease, are, in almost every case, capable of being rationally interpreted, and distinctly traced back to the action of those laws which regulate the vital functions, both in the normal state, and in its various deviations.

We learn, however, from daily experience, that whatever their general effects may be, the causes of disease are far from acting with equal intensity upon the various individuals who are exposed to their influence. Cold, hunger, thirst, fatigue, and moral suffering;—are not these the constantly recurring causes of sickness? and are they not, in some measure, the lot of the whole species? How, then, does it occur, that among those who daily undergo their action, certain individuals are found to give way, while others resist? and, when epidemics are raging in given localities, how does it occur that some persons only are affected with the prevailing distemper, while others who live in constant communication with the victims escape unharmed? To the mysterious power which thus modifies, in each particular case, the influences of external agents, we give the name of Idiosyncrasy.

We may, I believe, take it for granted, that not only morbid, but also physiological predispositions exist in man as well as in the lower animals: even in a perfect state of health each individual retains his own peculiar habit of body, and is, in consequence, more liable to certain accidents than his neighbour. The various animals which serve for our experiments are far from exhibiting the same phenomena, under the influence of agents entirely similar in their nature. You are already aware that as we rise or descend in the scale of being, we find animals endowed with different degrees of sensibility to the action of certain poisons—those, for instance, which operate more especially on the nervous system. There exist, therefore, within the limits of health considerable differences between living beings; and, as we have previously established, these various properties are not merely the result of organization, but frequently depend on the condition in which the animal has been placed. In this manner, a rabbit may be brought down to the level of a bactrachian; and, by reversing the experiment, the inverse result may be obtained. Now these important modifications are almost invariably produced through the agency of the nervous system.

Not only do the various species of animals differ in this respect; but even individuals belonging to the *same* species are so far from resembling each other, that they cannot be submitted to the same experiments. So exquisite is the nervous sensibility of dogs of the higher, breed, that the slightest operations bring on fever, and are attended with alarming symptoms; they cannot, therefore, be employed in researches connected with the gastric juice, the pancreatic secretions, etc.; in fact, all operations performed within the abdominal cavity are liable to superinduce peritonitis in those highly sensitive animals, and generally prove fatal. In dogs of a more vulgar class, how different are the results of similar experiments? During the operation the animal hardly attempts to move, and scarcely seems to suffer; the appetite remains unimpaired, and the secretions normal; in short, the various functions of the economy pursue their natural course.

In the horse these differences are, if possible, still more strongly marked. The characteristics of certain breeds are, in colloquial language, attributed to *blood*; it would be more correct to attribute them to *nerves*; an irritable, sensitive, and highly organised nervous system is, in fact, the essential difference which separates a race-horse from one of those diminutive half-wild ponies which hilly countries so abundantly produce. Would not the results of the same experiment be entirely at variance in these different animals? and what comparison could we possibly establish between them? It is, therefore, indispensable whenever great powers of endurance are required for the purposes of scientific research, to select an animal of the lower breed; if, on the contrary, sensitiveness and nervous irritability appear desirable, none but the nobler kinds will afford the requisite qualities. Experiments on recurrent sensibility, for instance, which, in the greyhound and pointer, are generally successful, if tried on a shepherd's dog would fail in almost every case. Cold-blooded animals stand, of course, in this respect, at the very bottom of the scale. It will, therefore, easily be conceived, that a state which in certain animals would constitute actual disease, may be perfectly natural to others.

The difference between individuals may be naturally expected to be far more extensive in man, than in all other living beings; and, if we might venture to allude to a subject which actually occupies the public attention, is not hypnotism a peculiar state, which can only be superinduced in a small number of highly sensitive and nervous patients? and do not all the phenomena of mesmerism, somnambulism, and similar nervous symptoms, fall under the same general rule? It is, therefore, evident that idiosnycrasies are only peculiar susceptibilities which exist, in the normal state, in various individuals.

Up to this moment, however, we have only examined physiological and innate predispositions, so to speak; but as Medical men, we are far more deeply interested in the investigation of accidental, transitory, and morbid idiosyncracies. To ascertain the circumstances which may be supposed to give them birth, is, to the physiologist, a most important object of research.

If we compare an animal in a state of abstinence to one in full digestion, the most evident discrepancies will be noticed in the results of experiments simultaneously performed upon them. A dose of strychnia which almost immediately kills the second, will not act before a certain lapse of time upon the first. The powers of absorption have, of course, been called into account for so remarkable a fact, but we are aware that absorption, in a state of abstinence, is more active by far than during the process of digestion; the explanation is therefore unsatisfactory. The lowering of the physiological activity of the nervous system is, in reality, the only cause to which the difference can possibly be referred. When deprived of food, the animal gradually sinks in the scale, and acquires properties altogether foreign to its previous state. Is this a morbid condition? No; but the natural result of a well-known physiological process.

We therefore entirely deny the existence of a so-called Morbid Physiology, if by this expression a state of things entirely independent of the ordinary laws of life, is to be understood. Such expressions ought to be expunged from the book of science; they only serve to render our notions confused, and lead the student astray. When speaking of Medical Chemistry, for instance, we do not pretend to say that the chemical actions which take place within the living body are totally distinct from those observed without. Morbid physiology has, no doubt, its laws; but they are precisely the same as those which regulate, in the healthy state, the vital functions.

Not only abstinence, but cold and various other causes, modify the conditions of life, and alter the results of our vivisections; under a low temperature, cold-blooded animals grow less sensible to the action of certain poisons; a larger dose of strychnia is required to kill a frog in winter than in summer. But, chloroform, ether, and even mere ordinary inebriation produce similar results; and, in America, it appears to be generally understood that intoxication is a preservative against the bite of the rattlesnake.

These, however, are the physiological modifications of the system. Our purpose is to investigate those which pertain to the morbid state. It is a well-ascertained fact that medicines do not act on sick people in the same manner as on persons in the full enjoyment of health. Now, the biological conditions superinduced by disease evidently lie at the root of these irregularities. To adduce a well-known instance of this, wine, brandy, and ardent spirits—so freely used by certain American physicians in the treatment of low fevers, remain apparently without effect on the patient, even when administered in quantities which, in a state of health, would inevitably produce intoxication. A two-fold explanation of the fact presents itself—Firstly, the process of absorption is almost entirely suspended; Secondly, the nervous system is strongly depressed. You are, of course, aware that, in cases of typhoid fever, the absorbent powers lie dormant for a long space of time; a fact established by the following experiment—If small quantities of prussiate of potash are dissolved in the patient's drinks, no vestige of this substance is discovered in the urine, or in any other secretion. A similar state of things may be physiologically superinduced; for where secretion is over-excited absorbent surfaces lose their properties. The inner surface of the salivary gland, which in a state of rest, rapidly absorbs strychnia or woorara, ceases in some measure to do so when secretion is going on. Five cubic centimetres (one-third of a cubic inch) of an aqueous solution, containing one-hundredth part of strychnia, being injected into the parotid duct in a dog, the animal was almost instantaneously killed. The same experiment being tried on another, in which secretion was kept up by means of galvanism, life was protracted for the space of twelve minutes.

Cholera is evidently another instance of the same fact. No substance whatever is absorbed by the intestinal walls, as long as this abundant and characteristic serous discharge continues. But disease, it will be said, is the origin of all these modifications. True; but a physiological process takes place under its influence, and the facts observed are its natural results.

The deficiency of the absorbent power has equally been found to exist under circumstances entirely different from those we have just examined. It occurs, for instance, in mania. Nervous influence here appears to be the sole agent; for, as soon as the acute crisis is past, the process of absorption recommences, as in the healthy state.

Woorara has been used of late in the treatment of tetanus. Out of four cases two have recovered—the other two died; but, in both the successful cases, the ordinary—or, so to speak, the physiological—effects of the poison had been produced; while, in the other two patients, no such result was obtained. Doubtless, some peculiar condition of the nervous system, rendering all impregnation impossible, existed in these two latter cases; perhaps, if treated at an earlier period of the disease, the patients might have recovered. In this respect, the well-known effects of sulphate of quinine may be be adduced. When administered in large doses (*e. g.* in cases of acute rheumatism), it never abates the pulse nor relieves the other symptoms without previously occasioning deafness—a physiological effect intimately connected with its therapeutical agency, for when, after improving the patient's condition, the dose is too abruptly diminished, the deafness disappears, and all the symptoms previously kept down break out again in full force.

But we also find in animals various predispositions, which not only modify the action of medicines administered to them, but also render them liable to diseases entirely different, when suffering from causes entirely similar. Being about to perform certain experiments on animals kept fasting for a long space of time, I left some dogs without food for several days; but, during the late severe frosts, these animals died unexpectedly. In making the autopsy, we discovered pneumonia in one case, pleuritis in another, and inflammation of the bowels in the two last. Thus, under conditions perfectly identical, these animals were affected with totally different diseases. But similar results may be obtained at will by the physiologist. When rabbits are placed under total abstinence they generally live a fortnight or three weeks; but when certain branches of the sympathetic nerve have been previously divided, the animals die within a few days when deprived of food, through acute inflammation of the viscera connected with the nervous twigs that have been divided. When, some time ago, I commenced this series of experiments, I discovered that the section of large divisions of the sympathetic nerve was apparently unattended with the slightest inconvenience as long as the health of these animals remained perfect. Some of them even became pregnant and brought forth their young, but, as soon as a general debilitation of the system arose from want of proper nourishment, acute inflammation was produced in the organs deprived of nervous influence. We had, therefore, succeeded in artificially creating particular idiosyncrasies in these animals, and could predict with perfect certainty that as soon as health failed disease would arise on a given point.

Morbid predispositions must, therefore, be viewed in the light of peculiar physiological conditions, which, in most cases, depend upon the nervous system; and an immense progress would be realised in medicine, if it were possible to diagnosticate in a state of health the predisposition to disease, and foretell the coming danger. A Russian army physician, who had invented a new sphygmometer, and had applied it to the study of various diseases, stated some years ago, that during a severe epidemic of cholera, a peculiar slowness of the pulse existed, several days before the explosion of the disease, in those who were marked out to be its victims. I am not aware whether the reality of the fact has been ascertained by other observers; but it would be, at all events, a most precious boon to know beforehand, when epidemics prevail, what persons are more particularly liable to be affected with the reigning distemper; we should thus be far better able to adopt preventive measures, and prescribe hygienic regulations.

In conclusion, let me advise you not to consider idiosyncrasies in the light of mysterious powers residing within the depths of our organs, nor as entirely novel functions superadded as it were to those which already exist; they must be viewed as mere natural manifestations of the ordinary laws of physiology.—*Med. Times and Gazette, Feb.* 4, 1860, *p.* 109.

147.—*To make a Blister.*—Steep cantharides in æth. sulph. for a fortnight, or until the cantharides float upon the surface; skim it off. One drachm of cantharides, ditto of white wax, five ditto of olive oil, melted together, mix. With a brush paint it over some white bibulous paper, and hang it up to dry in a current of air. Take a piece of pink paper, form and size required, and paint the undercoloured side over with a weak solution of india rubber; cut your cantharides paper the form and size (less a margin) of the pink paper,—while the India rubber solution is still sticky place it on, when dry roll it up. It is unaffected by damp, is light, portable, blisters with certainty and without pain. The introduction of the caoutchouc varnish arrests the perspiration of the part and increases doubly the certainty while diminishing the time required for application. Before applying, the blister should be held over the steam of hot water. The same blister will be effectual for several times. *Dublin Hospital Gazette, Jan.* 2, 1860, *p.* 15.

148,—ON TOXICOLOGY.

By EDMUND W. DAVY, M.B., M.R.I.A., &c.

In Toxicology much advance has been recently made. The cases of Palmer, Dove, and others, have been the means of directing public attention to that fearful and subtle poison, strychnine, of which but little was known prior to those criminal cases; and a great amount of knowledge has been gained as to the chemical and physiological properties of that most active poison. Many new and most delicate tests have been discovered which are found capable of detecting with certainty the most minute quantities of strychnine. I shall only refer to a few of the most characteristic.

One class of tests depend on the colour produced by the action of different reagents on strychnine, and are therefore called the colour tests; of these by far the most striking and delicate are bichromate of potash and the ferrocyanide of potassium, which in conjunction with strong sulphuric acid produce with strychnine a most beautiful purple coloration. The first was discovered by M. Lefort, and the latter by myself a short time ago; and they appear to be the most delicate tests of strychnine yet discovered. According to the experiments of Professors De Vry and Der Burg, of Rotterdam, on the comparative delicacy of different tests for strychnine, they state that those are the most sensitive, and that they were able by their means to detect the one-sixty-thousandth part of a grain of that alkaloid.

When search is made for the presence of strychnine in the contents of the stomach or in the different tissues or organs of the body, we are obliged to isolate or separate the strychnine from the various organic substances with which it must necessarily be mixed in such cases, previous to the application of these and other tests; and many different processes have been adopted for that purpose, the details of which would be out of place on the present occasion; but I may observe that very recently chloroform has been used to separate strychnine from organic matter, and it appears to be a very efficient substance for this purpose.

Professor J. E. Rodgers and Dr. Girdwood, of London, succeeded by the use of chloroform in detecting strychnine in the blood, liver, stomach, and tissues of animals which had been poisoned by that substance; and in one case after the animal had been interred for a year; which latter is a most important fact, as it was an opinion very generally entertained, that strychnine underwent decomposition when it was taken up in the system, which was the reason why it could not be detected in the blood and different tissues after death, in several cases of undoubted poisoning with that alkaloid. From the experiments instituted by Professors De Vry and Der Burg, they are led to the following conclusions, which seem to be the most consistent with known facts: viz., that if death has been caused by strychnine, this poison can only be detected in the body provided it has been administered in a quantity more than sufficient to cause death; but that if the poisoning by strychnine has been chronic, and has resulted from a quantity not greater than just necessary to cause death, it cannot be detected by a chemical examination of the stomach and intestines; and that it is probable that that part of the strychnine which has destroyed life is decomposed in the living body.

The only other test I shall call your attention to is the beautiful test of Dr. Marshall Hall, which that distinguished physiologist proposed a short time before his death. It consists in producing the peculiar physiological effects of strychnine in animals.

He selected for this purpose a frog, which he placed in a very dilute solution of strychnine, when after a short time it became tetanic and exhibited many of the effects peculiar to this poison.

This test is one of considerable delicacy, for Dr. Hall found that a frog placed in six drachms of water containing only the one-four-hundredth part of a grain of the acetate of strychnine became, after three hours and a-half violently tetanic, and in two other experiments, the one-five-hundredth, and even the one-thousandth part of a grain of strychnine were thus detected.

Dr. Harley, of University College, London, says, that he considers the physiological test the most reliable one for strychnine; and the method of applying it appears to be by injection into the thoracic or abdominal cavity. He further remarks, that when the poison reaches the lungs it seems to act most speedily; which is easily explained by the rapidity with which the poison will be absorbed into blood through the pulmonary capillaries. He found that a solution containing so small a quantity as the one-sixteen-thousandth part of a grain of the acetate of strychnine, which is equal to the one-eighteen-thousandth part of a grain of the pure alkaloid, injected into the lungs of a small frog, caused it to become violently tetanic after nine minutes and a-half, and to die in two hours.

Though the physiological test for strychnine is one of very great delicacy, particularly when applied as Dr. Harley recommends, still it is not taken by itself perfectly conclusive, at least in the case of frogs, which are now known to be tetanised by different causes unconnected with the presence of strychnine; there can, however, be no question that this test may often be used as a most valuable adjunct to the chemical tests, and more satisfactory evidence may be thus obtained as to the presence or abscence of that poison in suspected cases.

I may observe in connection with this subject, that as a general rule, those substances which have been found to possess the most deleterious effects on man and animals, have by divine foresight been endowed with very marked and characteristic chemical properties, which enable us readily to distinguish and identify the presence of those bodies, as for example, arsenic, corrosive sublimate, prussic acid, and, as we have just seen, strychnine; and though no doubt there are many vegetable and animal poisons which appear to possess no decided chemical properties, yet there can be little doubt but that when they come to be more attentively studied, they will be found likewise to possess characteristic chemical effects by which they may be readily known. The identification of those substances which are destructive to human life, is of very great importance to society; and the public owe a deep debt of gratitude to chemists and other men of science, who have studied and made known the properties of many of those substances.

But to return to strychnine, the inquiries recently made in connexion with this substance, have not only been the means by which different methods have been devised for the detection of the most minute quantities, but, likewise, several antidotes have been proposed, by which the poisonous effects of strychnine may be more or less destroyed.

One of the most remarkable substances as yet proposed as an antidote for strychnine is nicotine, the active principle of tobacco, which was suggested by Professor Haughton, F.T.C.D. Mr. Haughton was led to try the effects of nicotine as an antidote from the opposite physiological properties of that substance when compared with those of strychnine (the former producing relaxation, and the latter contraction of the muscles), and he conceived that it was possible, from their opposite effects, that they might mutually neutralize, as it were, each other's action. To determine this important question he instituted several interesting experiments on frogs, which he placed in different solutions of strychnine and of nicotine, and the results clearly show the correctness of his inference as to nicotine being more or less an antidote to strychnine.

These interesting results Mr. Haughton communicated to the Royal Irish Academy in 1856, and he has recently published them in a separate pamphlet, together with an interesting case of attempted suicidal poisoning with strychnine, which occurred in St. Louis, Missouri, United States, where, acting on the suggestion of Mr. Haughton, the individual was successfully treated with an infusion of tobacco.—*Dublin Hospital Gazette, Dec.* 17, 1859, *p.* 371.

149.—ATTITUDE OF PERSONS KILLED BY LIGHTNING.

"That which particularly characterises the effects of lightning," writes M. BOUDIN, "is its suddenness. At one time the individual is killed dead on the spot, remaining in a sitting posture, or even on horseback. At another time he is thrown a long distance." The first mention made of this kind of death is in the history of Alexander: "During a dreadful tempest," says Quintus Curtius, "which destroyed a thousand men, some were found supported against trees, as if still alive and talking with each other, and just in the attitude in which death had surprised them." According to Carden, as quoted by Rivière, eight reapers taking their food under an oak were struck by lightning, and died preserving their attitude—one of a man eating, another drinking. In Lorraine a woman and one of her children were killed, and remained in a sitting posture. At Dover, a man killed with four horses was found sitting under a bush. A man of law at Troyes was killed by lightning when on horseback; the animal still continuing his journey, brought home his dead master, retaining the posture of a man on horseback. On July 11, 1819, at Château-neuf, three fire-balls fell on the church. Nine persons were killed and eighty-two wounded. All the dogs found in the church were killed, and retained the attitude which they previously had. On January 22, 1849, a goat was killed near Clermont, and found sitting upon his hind-quarters with a branch of green leaves in his mouth. Now this phenomenon is sometimes equally observed in persons killed by fire-arms. In the 'Morning Herald' of November 8, 1854, the following details are given of a visit to the field of Inkermann, immediately after the combat: "Many faces still seemed to smile; others had a threatening look; some bodies had a funeral *posé*, as though laid out by friendly hands; others still knelt upon the ground, convulsively grasping their weapon, and biting a cartridge. Many had their arms raised as if endeavouring to ward off a blow, or as if desiring to offer a last prayer. All their faces were pale, and the fierce blowing wind seemed to animate their dead bodies; one would have said that these long lines of the dead were about to rise to recommence the struggle." M. Boudin writes the same thing of the appearance of many of the Russians after the battle of the Alma: "Some seemed still writhing in the agonies of despair and death, but the most wore a look of calm and pious resignation. Some appeared to have words floating on their lips, and a smile as in a sort of high beatitude. One was particularly observed, his knees bent, his hands raised and joined, his head thrown back, murmuring his supreme prayer." At Magenta again many dead bodies, as we are informed by Surgeon-Major Armand, of the Army of Italy, maintained the attitude they had when struck, passing instantaneously from life to death, without agony or convulsion. A Zouave struck point-blank in the chest, still held his bayonet in the position of the charge, with the menacing aspect of a dead lion. His Majesty the Emperor is said to have remarked a similar case at Palestro. Near to the Zouave was an Austrian, dead from hemorrhage, lying in

a pool of blood. His face and eyes were turned to heaven, his hands joined and fingers stiffly interlaced, evidently in the attitude of prayer.—*Med. Times and Gazette, Feb.* 18, 1860, *p.* 167.

150.—ON THE CEREBRO-SPINAL FLUID.

By Joseph Williams, M. D.

Nearly five-and-twenty years have now glided by since my attention was first specially directed to the secretion and probable uses of the cerebro-spinal fluid, by reading in 'The Lancet' an account of the interesting experiments of Magendie. On the following session I attended a course of his lectures at the College Royale de France, and ever since that period my inquiries have been, from time to time, devoted to this abstruse and complicated subject.

Desirous that the views now promulgated may be duly tested, weighed, and sifted, I place them in the pages of 'The Lancet,' in order that other opinions may be elicited as to their validity. Some of the suggestions may at first appear somewhat startling, although upon patient investigation they will, I believe, gradually become more and more feasible.

The following statements, then, some of which we know to be matters of fact, while others are, in our present state of knowledge, *necessarily hypothetical*, I now venture to bring forward.

That the cerebro-spinal fluid occupies that space which exists between the inner layer of the arachnoid and the pia mater, and that, as pointed out by Magendie, the ventricles of the brain communicate with this sub-arachnoid cavity.

That the fluid in the sub-arachnoidal spaces, and points of confluence, freely communicate with each other, and also with the ventricles of the brain; and further, that the cerebro-spinal fluid readily passes from the cranial sac downwards to envelope the spinal cord, and also upwards from the spinal sheath into the cranium.

That the central canal in the spinal cord *invariably exists, and bears the most evident marks of special design in its peculiar construction.*

That the intra and extra-cranial sub-arachnoidal tubes, surrounding the cerebral nerves as they emerge from the cranium, communicate continuously and directly with the fluid in the large intra-cranial sub-arachnoidal spaces, and consequently, also, with the spinal sub-arachnoidal sac.

That the filaments of the roots of the cerebral and spinal nerves may be clearly seen floating in the cerebro-spinal fluid.

That the clear fluid which sometimes escapes so abundantly from the ear and from the nose in cranial fractures, is now proved to be cerebro-spinal fluid.

That the quantity of the cerebro-spinal fluid is infinitely greater than is usually imagined or described; and that it is not only rapidly secreted; but also rapidly renewed.

That it is doubtless being constantly secreted, there being a continuous *waste*, not only with each psychial, but with every physical and reflex action; and that a large amount of cerebro-spinal fluid is probably formed for psychial than for mere physical effort.

That it is probably being continously excreted by cuticular, but especially by renal action.

That the cerebro-spinal fluid becomes at once affected, tainted, impregnated, or poisoned, by alimentary, chemical, or therapeutic agents—by whatever, in fact, enters into the circulation.

That the cerebro-spinal fluid actually comes in contact with what we believe to be the most important portions of the cerebro-spinal mass; *and that it also probably is distributed throughout the whole of the brain and spinal cord, and even to the minutest peripheric terminations of the nerves.*

That the quantity of the cerebro-spinal fluid bears an inverse proportion

to the size of the cerebro-spinal axis; and that, in obedience to the hydrostatic law, the fluid instantly passes to the point where the resistance is least; but that intracranial compression may actually occur, notwithstanding those numerous and wonderfully designed safeguards for preventing it.

That the cerebro-spinal fluid is probably secreted by the cellular portions of the cerebro-spinal mass, or by the pia mater, and that it is being constantly diffused throughout the whole of the nervous system.

That the pia mater not only dips down into the convolutions, as usually described, but that it probably, as a highly vascular and delicately attenuated membrane, *penetrates into every portion of the cerebro-spinal mass, wherever the arterial blood is distributed.*

That sea-sickness probably depends upon the irritation caused by the irregular and unequal vibratory pulsations or undulations of the cerebro-spinal fluid, as also the *anti-peristaltic action* so frequently observed in hydrocephalus, as well as the less frequent intussusception.

That the alternate rise and fall of the cerebro-spinal fluid does not synchronize with the pulsations of the heart, but corresponds with the movements of *respiration*, there being an alternate compression and exhaustion, and hence the dynamic power for sustaining the circulation of the cerebro-spinal fluid.

That undulatory motions or currents exist in all the cavities of the brain and spinal cord.

That in addition to the cerebro-spinal fluid forming a "water-bed" on which the cerebro-spinal mass can float, and thus be protected from friction and concussion, and in which its nutrient bloodvessels can also be protected from any direct pressure, that it is also probably destined to pervade and moisten every portion of the nervous system.

That when this fluid is deficient, perverted, or dessicated, it may cause a partial or more complete paralysis, as also atrophic wasting of the muscles. *moisture being so essential for all nervous action.*

That the local pain of gout depends not only upon the irritation set up by the acid state of the blood, but probably also of the acrid fluid circulating through the nervous fibrillæ; and that this hypothesis appears to be considerably strengthened by the development of gouty concretions.

That convulsions, and the undulatory chain of shocks, probably depend upon the abrupt and wave-like motions or impulses communicated to this fluid by the irritated nervous centres.

That the aura in epilepsy slowly, steadily, and continuously, proceeds upwards from the peripheral point of irritation through the nerve to the great nervous centre, where an irrespondent or opposing action in the circulation of the blood probably takes place instantly.

That as in galvanic or electric operations the very moment the chain is dissevered, the electric or galvanic influence momentarily ceases; so in the animal economy, the very instant an irrespondency takes place as to the regular transmission of the cerebro-spinal fluid, that instant insensibility occurs, to be followed usually by convulsion more or less general and severe.

Such are the opinions I venture, with great diffidence, to submit, as to the uses of the cerebro-spinal fluid. Its importance has certainly not yet been fully recognised. Only recently, when conversing with a distinguished physiologist, he told me he considered "it played but a very subordinate part in the animal economy." I would now, however, fain believe that but few persons will read this synopsis without at least acknowledging that the cerebro-spinal fluid must play a most important part in the animal economy. *Lancet, Feb.* 18, 1860, *p.* 165.

151.—ON THE RELATIONS OF BELLADONNA AND OPIUM, AND ON POISONING BY BELLADONA.

By James Seaton, Esq., L.R.C.S.E., L.A.C., Leeds.

[In September, 1858, "two young men having gathered about a pint of the ripe fruit of the atropa belladonna, which they found growing in an old quarry a few miles from Leeds, on their arrival home distributed them among their friends, believing them to be innocuous." A great number of the poisoning cases resulting from this imprudence came under the notice of Mr. Seaton—of which he relates ten, only one being fatal, and concludes by the following remarks :)

The first symptom appears to have been dryness of the mouth and throat; next, indistinctness of vision and dilated pupil; and, afterwards, in the more severe cases, delirium supervened. I found in one man, who had only swallowed one berry, the dry mouth and fauces without any affection of vision. The indistinctness of vision was the most persistent symptom; in all the cases it existed to a greater or less degree for several days, and the boy C. C.'s vision continues defective up to the present time. The delirium was of a busy, restless, vivid character, but generally rather pleasing than otherwise. The patients appeared to think that they were pursuing their ordinary occupations; one boy appeared eager in flying a kite; another pulled chairs and tables about, thinking he was working in a coal-pit; while the woman E. W., appeared to be remarkably busy with her ordinary household duties. All their movements were of a quick, excited character, strikingly resembling delirium tremens. There was no very marked vascular excitement, the skin was, in most of the cases, moderately cool, and the pulse rapid, but without power.

The remarkable tolerance of opium in these cases would appear to bear out the conclusion at which Mr. Bell, Dr. Graves, and others have arrived: that opium and belladonna mutually counteract each other. In none of the cases in which delirium was present, were the symptoms alleviated until sleep was obtained; and, after sleep, the patients felt comparatively well. The pupils were widely dilated so long as the delirium continued, and when sleep was obtained, were either contracted or reduced to the natural size.

With regard to the fatal case, I may mention, that she was decidedly scrofulous, and had been under treatment for several months for disease of the bones of the face. It will be observed that she took much less opium than several of the cases which recovered, and that, at the moment of death, the pupils were very widely dilated

The post-mortem appearances are chiefly valuable for their negative testimony. The condition of the brain appeared to be as nearly as possible perfectly natural; as was the mucous membrane of the stomach and intestines. The only thing which was abnormal was the remarkable fluidity of the blood in every part of the body.—*Med, Times and Gazette, Dec.* 3, 1859, *p.* 551.

152.—*On Quinio or Rough Quinine.* By M. Batka.—A substance is known in the Brazils under the name of quinio, which is extracted from the fresh bark of the cinchona by lime and then from the lime by alcohol. It is very rich in quinine, and it is only necessary to boil it with dilute sulphuric acid to obtain an abundant crystallization of pure sulphate of quinine. Quinio is a yellow body of a resinous appearance and of a bitter taste. It is insoluble in cold and but slightly soluble in boiling water. It is very soluble in alcohol and ether, separating partially from the latter by exposure to the sun. Water precipitates the alcoholic solution. It is almost entirely soluble in weak sulphuric acid, from which soda precipitates it of a dirty white colour, the precipitate assuming the appearance of a resin. A beautiful white sulphate, however, may be prepared from it. Quinio is free from cellulose; when heated it gives off an oduor something like cinnamine, and burnt, leaves a light residue of carbonate of lime. It resembles a good deal the quiniodine of Liebig, but is much purer than quiniodine of commerce.—*Dublin Hospital Gazette, March* 15, 1860, *p.* 93.

153.—ON THE CHEMICAL AGENTS OF DISEASE IN THE LIVING BODY.

By M. Claude Bernard, Member of the French Institute; Professor of General Physiology at the Faculty of Sciences.

Animals debilitated by want of proper nourishment submit less readily to the agency of certain poisons than others in a vigorous state of health; but, as you are aware, it has been questioned whether similar modifications are due to nervous influence, and whether the diminished activity of the absorbent powers is not sufficient to explain them. In order to settle the question at once, I injected an aqueous solution of woorara into the veins of two rabbits, one of whom had been previously fasting, while the other was duly fed; in this manner, absorption was entirely dispensed with, the poison being at once conveyed into the blood. The result was such as might have been expected. To poison the fasting animal a dose larger by one-third was required than had been found sufficient to destroy the other. It is. therefore, perfectly clear that all this class of phenomena must be entirely referred to the agency of the nervous system.

But, while the animal is in some measure preserved from the noxious influence of certain poisons, through the rapidly-increasing debility of its nervous system, it becomes obnoxious to the action of morbid influences of a totally different character. It even appears to me, that in our nosological classifications this peculiar liability of the system might be turned to account, as regards the etiology of disease.

To adduce a characteristic instance of this: when frogs have been kept for a long space of time in captivity, their health declines, and ulcerations arise around the nose and mouth; the nervous system being in this case considerably depressed, the animal is of course found to resist much longer the action of strychnia and similar poisons, while parastitical affections spread with fearful rapidity. Frogs are subject to the growth of parastitical fungi, which, after a certain lapse of time, occasion the animal's death. Now, if a healthy frog is placed in a jar, containing others affected with the above-mentioned disease, the new-comer sets contagion at defiance; while if another frog, affected with ulcerations in the vicinity of the natural orifices, is introduced into the jar, the parasitical vegetation covers it at once.

It has been found that similar affections always have a strong tendency to arise in animals in a low state of health. The itch, a disease which frequently prevails among horses and sheep, is scarcely ever found to attack animals in good condition; and, in man, the lower classes are known to be a prey to vermin, especially in childhood and old age; while persons who live under more favourable circumstances are scarcely ever affected with this inconvenience, except towards the latter end of long and painful diseases; for it is generally in such cases that the *morbus pedicularis* has been observed.

The decrease of nervous power equally constitutes a predisposition to putrid, contagious, and virulent affections; the fact is well known to veterinary surgeons.

It would appear, therefore, that an apposition exists between the two great classes of disease we have just examined; in proportion as the animal grows more sensible to the action of neurosthenic poisons, the power of resisting the influence of putrid substances is increased. How is this difference to be accounted for? We shall attempt to give you a solution of the difficulty.

That the chemical composition of the blood should incessantly be modified, is one of the essential conditions of life; repairing, as it does, the daily losses of the economy, and renewing the elements of all the tissues which enter into the system, the blood may be compared to a torrent which continually pours on tnew substances, while other elements are flowing into it; and the stronger are the animal's vital powers, the more rapid are the successive changes of the blood; a fact principally observed in birds, which enjoy greater vital energy than any other class of animals. The uninterrupted continuance of circulation is, therefore, in such animals, of still greater importance than in others; the blood cannot stagnate without promptly acquiring septic properties. If the tributary vessels of a muscle are tied in a mammal or bird, it becomes a putrid mass within twenty-four hours; in a batrachian this change would not take place before a much longer space of time.

Now, you are aware that the nervous system presides over all the phenomena of life in which motion is concerned; as soon, therefore, as the nerves are impaired, circulation languishes, and the chemical composition of the blood becomes thereby liable to important changes. If, therefore, an animal being given, it is our purpose to preserve it from the action of woorara, or similar poisons, we must lower its forces. If, on the contrary, we intend to preserve it from contagious diseases, we must increase them by all possible means.

But these septic bodies, or specific poisons, are almost invariably organic substances, and are produced within a living organisation; here we have, no doubt, a peculiar and characteristic biological action; we need not, therefore, be surprised to see pathologists endeavouring to withdraw this class of phenomena from the domain of physiology, in order to make them the exclusive property of medicine.

We must not, however, in my opinion, give up all hopes of connecting, one day, these morbid phenomena with the laws of physiology. If at present unable to do so, we shall no doubt succeed at some future period. Is it not, in fact, quite possible that in animals certain physiological conditions may arise, which would give birth to virulent poisons? We are aware that in a perfect state of health, several creatures are venomous; that is to say, they possess a peculiar virus which Nature has given them for the purpose of killing their prey, and defending themselves from their enemies. Here, then, we have a physiological virus; how is it produced within the system? The difficulty is quite as great as with regard to morbid poisons.

It would appear that in several cases, the noxious substance prevails throughout the economy; in other cases we only discover it in certain fluids. The virus which occasions hydrophobia belongs to the latter class; it resides exclusively in the animal's saliva. We are not yet aware whether any one of the salivary glands is its peculiar seat, or whether it is indifferently secreted by all of them. No experiments have been tried on this point; but it has been experimentally proved that the peculiar venomous principle does not exist in the blood; transfusion does not convey the disease from a mad dog to a healthy one.

It is a singular fact, and one which pre-eminently deserves our attention, that in so general a disease the virus, which alone is capable of transmitting the affection, should be exclusively localized within one single apparatus, without existing in the blood at large. Yet, if we reflect upon the question, we discover in the physiological state, a great many similar dispositions; the principles which concur in a vast number of physiological functions; pepsine, ptyaline, and the active principle of the pancreatic juice, are they not created by special glands? and is not the venom of serpents, which does not exist within the blood, produced by a special apparatus? Viewed in this light a mad dog resembles a viper or a rattlesnake.

But, on the other hand, there exist several virulent diseases, in which the blood really appears to contain the morbid principle. This is the case with the glanders; and it is a well known fact that healthy animals may be infected with the blood of a deceased horse, as well as with the slimy matter that escapes from the nose and mouth.

But another particular, which will, perhaps, excite your astonisument, is that the normal secretions, bile, saliva, gastric juice, and so forth, do not appear to contain the slightest vestige of this poison; while, on the other hand, the pathological fluids appear to be impregnated with it, and possess the property of transmiting the disease to sound animals—a fact experimentally proved with regard to pus, the fluid contained in a hydrocele, and various other morbid secretions. For this reason, alone are the autopsies performed on animals that die of the glanders attended with so much danger; the virus pervades the whole system, and the slightest wound is sufficient to inoculate the complaint.

You need not, however, be astonished at this singular property; you have already witnessed the repulsion which the salivary glands evince for certain substances introduced into the blood; and why should not certain morbid principles be in this manner rejected from all the secretions in which the normal conditions remain unimpaired? The same thing appears to take place with respect to the contagious pneumonia of horned cattle. We are aware that volatile emanations transmit the morbid principle; but experiments have been tried (in Belgium) for the purpose of inoculating it directly to animals, as a preservative against the disease. Something similar to the process of inoculation in the small-pox was expected to result from this; it was then discovered that neither the animal's blood nor any of the fluids of the economy was endowed with the property of propagating the complaint. It appears to have chosen the lung for its exclusive seat, and the liquids therein contained, pus, lymph, etc., are alone endowed with the property of transmitting the complaint. The intense local inflammation which follows the operation sufficiently testifies to the noxious properties of this virus; and when, in order not to spoil the animal's flesh, the tail is selected as the point where inoculation is to be performed, the subsequent inflammation frequently causes it to mortify.

Here, then, we have another virus which exclusively resides in the tissue of the lungs, and is not found in the blood at large; but even in the normal state a great many substances are found in various tissues which do not exist in this fluid. Thus, muscular flesh contains a large amount of salts of potash, while scarcely any trace of them is found in the blood; in a word, the various bodies found in different parts of the economy are not invariably represented in the torrent of thecirculation.

The history of specific diseases offers, therefore, nothing which cannot rationally be explained; it now remains for us to discover the physiological process by which a virus may be originated. Nothing is easier than to produce putrid affections in sound animals. Thus, when transfusion is performed under the ordinary conditions—when the blood is conveyed directly from one animal into the veins of another—no accidents whatever are produced; but if the blood is allowed to remain for a short space of time in contact with the atmosphere, and if the serum is then injected into the vessels, all the symptoms of putrid resorption are observed, and the animals die after exhibiting all the characteristic symptoms of putrid infection.

The blood is therefore capable of acquiring toxic properties without the intervention of any foreign principle, merely through the modifications which take place in its composition when life is extinct. The same results may be attained to without even drawing blood from the veins. If the blood of a fasting animal is directly injected into the veins of a healthy one, the latter is poisoned exactly in the same manner as before; and yet the blood, in this case, has not undergone any previous decomposition.

The introduction of foreign principles, of course, acts upon the blood with still more intensity; nearly all the substances known under the name of *ferments*, are endowed with the property of communicating a deleterious influence to this fluid. When yeast is introduced into an animal's veins, passive hemorrhage, and other adynamic symptoms, are immediately produced, and death takes place within a few days. Now, if the animal's blood is transfused into another's veins, all the phenomena previously described take place in rapid succession, exactly as if yeast, and not blood, had been directly poured into the vessels.

It seems likely that in this case a series of decompositions take place within the blood, which give rise to other *ferments*. The well-known experiment related in Pringle's work on Army Diseases, appear to tally with the results of our own experiments.

(In order to prove the influence of putrid emanations, even at a distance, on the chemical phenomena of life, he plunged a thread into the yolk of a rotten egg, and then suspended it in a jar containing the yolk of another egg, and under these circumstances, decomposition took place with far greater rapidity than usual.)

We, therefore, perceive that all this series of phenomena hold intimate connection with that mysterious chemical process known under the name of *catalysis*. The theory of fermentations is at present so imperfectly known—and organic chemistry has in this respect made, as yet, so little progress—that it would hardly be fair to reproach Medicine with its deficiencies on this point. There exists a whole series of diseases which evidently result from the chemical actions which take place within the body. It is, therefore, chemistry alone, which, in its future progress, can teach us the physiological laws which embrace this particular branch of Medicine.—*Med. Times and Gazette, Feb.* 25, 1860, *p.* 183.

154.—THE AIM OF MEDICINE.

By John Cockle, M.D., F.L.S., Physician to the Royal Free Hospital.

The final end and aim of medicine is the cure of disease. The gulf between physiological pathology and therapeutics must be bridged over by clinical observation and experiment; that is to say, by what is known as "rational empiricism." Simple knowledge of the nature of the disease can no more, of itself, suggest the remedy, than can a knowledge of the nature of the rocks and mountains thrown up by volcanic agency suggest the mechanical means by which these may be removed, and the barren spot restored to its primeval freshness. Let us test the truth of what I here advance, by selecting a few of the best known and homeliest illustrations.

Look to the history of the virtues of cinchona bark. The untutored Indian, chance-directed to the healing stream, into which cinchona bark had been driven by the winds, found that the stream that slaked his thirst achieved his cure. And this, to him, was all sufficient. Some reasoning Jesuit traced the pregnant sequence, and added to our therapeutic treasury cinchona bark, one of its richest, most invaluable gifts. Since that poor Indian lived, how many pages have been written, both on the nature of the fever, and the action of the bark! But, I would ask, whether all this so-called knowledge has afforded one single element of explanation of the simple formula—*bark cures intermittent fever?*

Take another illustration. A thousand years ago, the Arabian Rhazes described the pathology of small-pox so accurately, that, even now, after this lapse of ages, it scarcely requires retouching. Barely half a century ago, the protective power of vaccination was discovered. Was this a deduction from the known pathology of the disease? I will not libel your historical knowledge by pausing for a reply. A country milk-maid, living in some remote district, where small-pox frequently raged in all its fearful intensity, learned from simple observation that, if from the udder of a cow, a certain pustule were produced upon the hand, immunity was given from disease. No observation could be more simply empirical than this. The fact in all its simplicity, was related to one of our greatest observers. Jenner's intuitive sagacity saw the deep import of the tale. He followed out the experiment in all its simple grandeur, verified it on the largest scale, and bequeathed to suffering humanity, for all coming time, one of its greatest blessings.

There is a disease called bronchocele, vernacularly *goitre* or enlargement of the thyroid gland; the cure of which was again discovered by simple observation of the purest kind, though the history is less generally known. It was found that burnt sponge cured the disease; and although more recent researches have shown that the virtue of the sponge was in proportion to the amount of iodine it contained, still the fact is in no way changed, and another practical truth is thus added by simple observation to practical medicine.

I must not dwell on more examples; these sufficiently establish the fact that the remedy here was not deduced from the nature of the disease.

I have now, I trust, more than verified the proposition that clinical experiment is still the most valuable source on which to base the healing art, especially when combined with that rational diagnosis which the sciences named, and that of pathological anatomy, supply. To avoid a cumbrous terminology, I have included the schools of pathological anatomy and physiology; but the above criticism applies with equal force to pure pathological anatomy. The proximate cause of disease may be, and has been, ascertained with the utmost precision by the labours of this distinguished school, and rational diagnosis has been further perfected, but an equal sterility exists with regard to therapeutic indications.—*Brit. Med. Journal Oct.* 29, 1859, *p.* 874.

155.—ON ATROPHY AND DEGENERATION OF THE ARTERIES.

By Edwin Canton, Esq., F.R.C.S., Surgeon to the Charing-Cross Hospital, and Lecturer on Surgical Anatomy.

[The arteries of the brain participate in the degeneration observable in the vessels of other parts of the body, but Dr. Huss has frequently observed in addition to this that]

"There is a dilatation of the arteries, both large and small, which ramify in the cerebral substance, so that on slicing the brain they appear of larger size than natural, while the coats of the larger blood-vessels likewise become brittle."

It is well known that an atheromatous state of the arteries strongly predisposes to the occurrence of aneurism; and in regard to the frequency of this latter disease in intemperate persons I may quote the valuable testimony of Dr. Crisp, who observes:—"Aneurism is, I believe, more frequent in England than in any other country. Temperature has but little influence in the production of the disease; but, when the habits of the people are considered—their great industry, the violent and continued exertions they are accustomed to use, as well as their greater addiction to the use of ardent spirits—the cause admits of a ready explanation. Soldiers and sailors, especially, are more liable to aneurism than any other class; and they are called upon, often, when the heart's action is increased by the use of alcohol, to make sudden and violent efforts. Amongst agricultural labourers, on the other hand, who (though subject to great bodily fatigue) generally lead a life of temperance and have their minds free from anxiety, the disease is comparatively rare."

When we remember the condition in which some of the important organs of the body are found after death in habitually intemperate persons; the general implication of the coats of the bloodvessels; the unhealthy condition of the blood itself; and, moreover, the change so frequently to be noticed of the fibres of the heart, can we wonder at the feeble chance of success presented to the surgeon who undertakes the treatment of an aneurism under such untoward circumstances? And, in the abnormal state described of the circulatory apparatus and its contents, can we be surprised at the occurrence of hemorrhage, arteritis, apoplexy, or sudden cessation of the heart's action? Can any condition be more subversive of all those processes whereby the nutritive functions should be duly exercised? The life of the drunkard hangs, indeed, on a thread, and his frail existence may be suddenly cut short by the last contractile effort of a fatty heart, or where the nutritive and reparative powers are so greatly in abeyance, the slightest injury may be rapidly followed by an equally fatal result. Dr. C. Wilson relates the case of a drunkard, in the prime of life, who had been engaged in his usual avocation till a few days previously, when he became indisposed and was found lying extended on the floor of his apartment, cold and rigid, having evidently been dead for several hours. On assisting his medical man at the post-mortem examination, the liver was found so much enlarged, especially on its upper convexity, that it extended as high as the fourth rib, its whole substance being of a yellow-ochrey colour, and friable in consistence, or in other words, presenting the characteristics of what has been termed the fatty liver. The more immediate cause of death, however, was attributable to the state of the heart, which was *softened*, *easily lacerable*, and contained fibrinous concretions. Dr. Ormerod has recorded the following instance:—A strong man, between thirty and forty years old, addicted to very hard drinking, received a slight injury of the head. He paid no attention to it, and continued his work for four days; then having headache, and feeling ill, he laid up, and left off all strong drink. In three days signs of delirium tremens came on, and he was brought to the hospital. In the course of the second day after the beginning of these symptoms, while they were pursuing an ordinary course, he took 140 drops of laudanum, but no beer or spirits until the evening, when, the opium seeming to be sufficiently administered, brandy was given him, and beef-tea. In about two hours he went to sleep, and remained dozing and apparently improving in condition all night. But, in the morning, a new nurse came to him, who wished to change his bed-linen. For this purpose she took him out of the bed and set him in a chair; but he had hardly been removed when he seemed dying, and he died before he could be again placed in bed. In his body, the only changes were fatty degeneration of the heart and liver. Another case is detailed by Dr. Ormerod, where a man of only thirty-four years of age, of notoriously intemperate habits, and who laboured under anasarca and ascites, died suddenly from fatty degeneration of the heart.

Dr. Watson says: "There is but one morbid condition which, since my attention was directed to it, I have found constant in persons dead of delirium tremens, and that is, a *remarkably soft, pale, and flabby state of the muscular tissue of the heart.*"

I have alluded to the unhealthy condition of the blood, in which Huss states oily particles to abound in that portion of it remaining in the heart and larger arteries in the bodies of drunkards; and this fluid, when rubbed between the fingers, gives the same feeling as though it were fat. This increased amount of fat in the blood (*pionœma*) is said by Wedl to confer on the serum a whitish, milky discoloration, and the floating fat-globules may be easily recognised under the microscope.

The blood, in addition to presenting these peculiarities in drunkards, is found to be defective in its usual amount of fibrine, and a lower than the normal grade of plasticity prevails; the corpuscles appear to be diminished in number, and the quantity of the aqueous parts is increased; there is less than the ordinary estimate of hæmatine present, and the carbon is superabundant. Scharlan has found as much as 30 per cent. more carbon in the blood of a drunkard than in that of a healthy man.

Beyond the fact of the direct passage into the blood of the alcoholic fluid received by the stomach, the accumulation of hydro-carbonaceous matter in it may be accounted for by the diminution of the amount of carbonic acid and water expelled from the lungs by those under the influence of this stimulus. "But," observes Dr. C. Wilson, "physical, and even mental exertion, increases the degree of exhalation of carbonic acid by the lungs, and therefore operates also by increasing the power of resisting effects. Hence the drunkard, reduced to inaction, loses yet another of his grounds of vantage. Fasting, on the other hand, causes less carbonic acid to be exhaled, and fasting increases the proneness to intoxication and the mischief of its results."

The general diseased state induced in persons addicted to intemperance has been so jealously cared for by those interested in life assurance, that statisticians have made the rate of mortality in regard to them an object of special attention; and Neison has found that at the term of life from twenty-one to thirty, or in the first vigour of manhood, the mortality of drunkards was upwards of five times, and from thirty-one to fifty upwards of four times, that of the general community of like ages; and he remarks—"If there be anything in the usages of society calculated to destroy life, the most powerful is, certainly, the inordinate use of strong drinks."

"It cannot be too widely known throughout the length and breadth of the land," observes Mr. Smee, "that though during the last few years the most severe competition has taken place between different life assurance companies for business, not one has dared to insure intemperate lives, or lives which have been intemperate, at any increased rate. No reasonable rate meets the case. It is in vain that the drunkard turns to propriety; he can never altogether erase the mischief; and although much may be done by great perseverance and skill, he never can altogether bring himself into a healthy state, or be regarded as a thoroughly healthy life. *The frame of the drunkard is dibilitated throughout*, and he sinks by pneumonia, disease of the liver, or other malady depending upon debility, which cuts off his career prematurely. Insurance companies abhor the drunkard, as a risk far too great and uncertain to be estimated and safely considered. If the career of the drunkard be watched, it is astonishing how soon he passes away, as if the voice of Nature exclaimed, "Cut it down! why cumbers it to the ground?"

In the cases I have related, where there was found after death marked degeneracy of the heart and arteries in the bodies of drunkards, and corresponding change in other organs, there existed in the corneæ well-developed circles of fatty degeneration; and when we consider the great involvement of the other tissues in this alteration, and the degraded nutrition of all, it might, not unreasonably, be anticipated that the eye will afford corroborative testimony to the general evidence of ravages so wide in extent and deadly in form—ravages which have rapidly reduced youth to age, and are hurriedly leading the "artificial old man" to premature decrepitude and the tomb. "The arcus, or circulus senilis of the eye," observes Dr. C. J. B. Williams, "almost always indicates premature age, brought on by the wear and tear of extensive mental anxiety, or by *fast living*.—*Lancet, March* 17, 1860, *p.* 269.

157.—ON EGYPT AS A RESIDENCE FOR EUROPEAN INVALIDS.

By Dr. W. Reil.

The principal difference between Cairo and Madeira appears to consist in this, that in Cairo the air is very dry, and the thermometrical oscillations are pretty considerable during the winter months; whilst in Madeira the air is very moist, and the variations of temperature are very trifling. Hence it may be asked, how can phthisical patients recover their health under such different circumstances; and is there really no difference in the mode of operation of such very different external agencies? Dr. Riel confesses that statistical evidence is wanting, especially in the case of Cairo, to enable us to give a complete answer to these questions. The following is, however, his individual opinion, which agrees with that of a large number of medical men who have been for a time settled in Egypt.

An acute form of phthisis, with intense fever, frequent short cough, great dyspnœa on exertion, expectoration scanty but often sanguinolent: such are the symptoms which appear to indicate a residence in Madeira; whilst patients in whom the pulmonary disease is running a chronic course, who are subject to exacerbations followed by ameliorations, in whom the expectoration is abundant, not accompanied with hæmoptysis, are very speedily benefited by the dry atmosphere of Cairo. In cases where we can only anticipate the gradual sinking of our patients, when we would leave them to die in the open air, as it were, Madeira should be preferred, were it only for the superior comforts which that island affords.

At the first onset of pulmonary tuberculosis, when the patient is young, while his constitution is still strong, and his powers of assimilation are normal, Dr. Reil recognises no difference between Cairo and Madeira. It is certain that the air in the former of these localities, on account of its great dryness, its purity, and its comparatively lower temperature, may act as a tonic and excitant; whilst, on the other hand, the warm and moist air of Madeira is suitable to the irritability of certain temperaments, although in itself it has also a relaxing and debilitating action.

Dr. Mittermaier has found almost invariably, on examining the bodies of phthisical patients in Madeira, hyperæmia and granular degeneration of the kidneys, the latter condition often to a high degree. But, says Dr. Riel, this pathological condition is not so constantly found accompanying pulmonary phthisis as to warrant us in considering it an integral part of the disease, even if we admit a physiological relation between the two affections. We are warranted, then, in expressing the suspicion that the renal affection has been determined by the influence of the climate; on this most favorable hypothesis, it has certainly not been cured by it. On the contrary, Dr. Reil appeals to his fellow-practitioners in Egypt, Drs. Reyer, Lautuer, and Bilharz, to confirm his own observations, that considerable proportions of albumen detected in the urine of phthisical patients lately arrived from Europe, disappear during their residence in Cairo or Upper Egypt, and in some *post-mortem* examinations kidneys have been found presenting the appearance of cured granular degeneration. There is also no doubt that, at the autopsies of phthisical patients who have died in Cairo of some acute intercurrent affection, large cavities have been met with, lined with a smooth false membrane, and consequently in progress of cure, as well as smaller vomicæ already in process of conversion into a dense cicatrix tissue.

On one point all the medical authorities are unanimous, that, even at the very commencement of the disease, a cure cannot be effected, except on condition that the patients reside for two or three winters in Cairo or Madeira; and that for a more advanced stage of the disease, with infiltration and formation of cavities, the only chance of success is in a permanent abode in the country.

Far more certain than the cure of pulmonary phthisis either in Cairo or Madeira, is the cure of affections of the respiratory passages not connected with the tubercular diathesis. Such are chronic bronchitis, which has followed or has accompanied pleuritic effusions, chronic laryngitis, and emphysema. In these cases the choice between Cairo and Madeira will depend upon the constitution of the individual patient, and in the special effects required from the climate in each.

There are other diseases characterized by a condition of asthenia, in which a residence in Egypt is indicated; such are anæmia, hydræmia, and chlorosis. It is scarcely necessary to add what good effects may be expected in the case of patients convalescent after severe fevers of abundant hemorrhage. —*Gazette Hebdomadaire.—Edin. Med. Journal, April,* 1860, *p.* 959.

157.—*Antimony and its Detection.* By MM. LASSAIGNE and LORAIN.—M. Lassaigne, in commenting on a case in which it was necessary to discover whether antimony were or were not present in the tissues of a person supposed to have been poisoned by successive doses of emetics, makes some objections to the use of Marsh's apparatus. He argues that, after complete oxidation of tissue, the antimony passed over from the Marsh bottle, in the form of antimoniuretted hydrogen, only represents a part of the absolute amount contained. In this respect, Marsh's process contrasts very unfavourably as compared with its application to the detection of arsenic. The following is a description of the more simple plan for the qualitative detection of antimony in an organ, as the liver.

MM. Lassaigne and Lorain burned fifty *grammes* of the liver in a new porcelain crucible. The carbon proceeding from the burning was calcined by maintaining during several hours a cherry-red heat. The ash collected, reduced to a fine powder, was successively treated with weak nitric acid an pure hydrochloric acid. The solution, to which was added a solut weak tartaric acid, was filtered and brought into contact with three times its volume of hydrosulphuric acid. This reagent immediately threw down a yellowish orange flaky precipitate of hydrated sulphide of antimony, the volume and colour of which, compared with a precipitate formed in a standard solution of tartar emetic, permitted the operators to estimate the proportions of the salt contained in the hepatic tissue submitted to observation.—*Ann. d'Hygiène publique.—Brit. and Foreign Medico-Chirurgical Review, April* 1860, *p.* 527.

158.—DETECTION OF ARSENIC, ANTIMONY, COPPER AND BISMUTH, BY ELECTROLYSIS.

By Prof. Bloxam.

At the last meeting of the Chemical Society, Professor Bloxam, of King's College, gave the results of a very successful investigation of this subject, and showed that, by proper refinement in the method of operating, the process of electrolysis may become a certain and delicate means of detecting one or all of the metallic poisons, at least with but few exceptions. In an examination for arsenic by this method, the metal is obtained in the form of arseniuretted hydrogen; the process is therefore very similar to that of Marsh, over which, indeed, it does not present any advantage in point of delicacy. Marsh's process, however, although it is capable of doing all that can be done by electrolysis with even greater delicacy, is open to several well-known objections, which have stood in the way of its practical adoption by toxicologists. The process of electrolysis does not involve the use of zinc, which is so difficult to obtain pure. It forms a general method for the detection of several metallic poisons at once, and the material tested is not destroyed or inconveniently contaminated, but may be used for another operation. When the arsenic, on the other hand, is present in a state of arsenic acid, it cannot, according to Prof. Bloxam's experiments, be detected with certainty by electrolysis. It is consequently necessary to reduce the arsenic acid by means of sulphurous acid. This is an objection which does not apply to Marsh's process. In his earlier experiments, Mr. Bloxam made use of a U tube containing dilute sulphuric acid; the substance to be tested was introduced into one of the limbs, and a cork with a bent tube fitted to its mouth; two platinum plates, leading from the poles of a battery containing five cells of Groves, were introduced into the two limbs, and the liberated hydrogen passed through the bent tube, which was heated by a lamp, when the arsenic, if present, was deposited.

The form of apparatus ultimately adopted as being the most convenient, consists of a two or three-ounce bottle, the bottom of which has been cut off, and replaced by a piece of vegetable parchment, bound on with platinum wire. To the mouth of the bottle is fitted a cork with a bent tube and a piece of platinum wire, which passes through the cork and turns up beneath in the form of a hook. A slip of platinum then hooks in the end of the wire, and passes nearly to the bottom of the bottle; it forms the negative pole of the arrangement. The bottle stands in an ordinary test-glass, and the positive pole, also of platinum, stands in the glass. Dilute sulphuric acid is put into the bottle, and also the glass, so as to stand to the same height in both vessels. The substance to be tested is introduced into the bottle, the cork adjusted, and the wires connected by five cells of Grove's battery; the heat of a spirit lamp is applied to the bent tube, and in the course of a quarter of an hour a distinct mirror is obtained, if arsenic is present. Standard solutions, containing respectively a tenth, a hundredth, and a thousandth of a grain of arsenious acid, were prepared and examined by this process, and in every case a successful result was obtained. These solutions were then mixed with organic substances, such as the ordinary articles of food—meat, eggs, milk, &c.—and the resulting matter examined.

It was got into solution by means of chlorate of potash and hydrochloric acid, and the resulting fluid evaporated down by means of a water-bath to a thick, syrupy liquid. The arsenic was thus obtained in the state of arsenic acid, which does not give a certain result by the electrolytic process. Some sulphurous acid was therefore added, and the mixture introduced into the bottle, after expelling the excess of sulphurous acid by evaporation; a drachm of alcohol was then poured over the surface, and the process put into operation. The author prefers to add to this drachm of alcohol in every case, inasmuch as it not only allays the frothing, but also affords an additional indication of the presence of arsenic; for when these two substances are present—the alcohol and the arsenic—the gas which escapes at the open end of the test tube, possesses a very peculiar odour, resembling alkarsin. If a little sulphurous acid be present, it also furnishes an additional character indicative of arsenic; namely, a slight yellow deposit, consisting of sulphide of arsenic, close to the borders of the metallic mirror. In all these experiments, of which a great number were made, the thousandth of a grain of arsenious acid was readily détected.

The other metals which may be detected by this process are mercury, antimony, copper, and bismuth; lead is precluded by the sulphuric acid which is present. These are all precipitated in the metallic form upon the slip of platinum, and even in the case of antimony a mere trace of antimoniuretted hydrogen is formed, the metal being all deposited upon the negative pole. The mode of proceeding in these cases is precisely similar to that adopted for arsenic; when the operation is concluded the slip of platinum is detached, washed, and the deposit dissolved off in the usual manner. Thus, where an organic mixture has to be examined for arsenic, mercury, copper, antimony, and bismuth, it is prepared in the manner just described for arsenic, and the resulting liquid introduced into the bottle, the drachm of alcohol poured over the surface of the contents, the cork adjusted, and the battery connected. The heat of a spirit lamp is applied to the bent tube, and the operation continued for about a quarter of an hour or twenty minutes, when, if arsenic is present, a metallic deposit, accompanied by some crystals of arsenious acid, will be formed in the tube, and the escaping gas will have the alkarsin-like odour. The piece of platinum in the bottle is next removed, washed, and boiled in yellow sulphide of ammonium. Antimony would be dissolved, and might be obtained as sulphide by evaporating this solution to dryness. The other metals would still remain in the plate; it is next boiled in nitric acid containing a trace of hydrochloric acid, the solution evaporated to a small bulk, and an excess of ammonia added. Oxide of bismuth would be precipitated, together with whatever traces of platinum had been dissolved. The precipitate may be dissolved in hydrochloric acid, and tested by pouring into water, etc. The ammoniacal filtrate would contain the copper, indicated by its blue colour, and the mercury. By boiling with hydrochloric acid and a slip of copper, the latter would be separated in the metallic form.—*Pharmaceutical Journal, Jan.* 1860.—*Brit. and For. Medico-Chirurg. Review, April,* 1860, *p.* 527.

ABSTRACTS FROM WORKS PUBLISHED DURING THE PAST SIX MONTHS.

[Intended as this Work is, for the perusal of the large mass of medical men, whose time is too fully occupied to read all the Journals and Works published, we shall, in the following abstracts, chiefly notice those parts of works especially bearing upon the *treatment* of diseases.]

159.—*Clinical Lectures on certain Acute Diseases.* By ROBERT BENTLEY TODD, M.D., F.R.S., Physician to King's College Hospital. *London,* Churchill, 1860. pp. 487.

[The design of the lectures published in this volume is to illustrate the clinical history and treatment of the more important acute diseases. The principal diseases treated of are Rheumatic and Continued Fevers, Erysipelas, and Pneumonia. In the preface, the conclusion to which the observations detailed in the lectures tend more or less to establish, are thus summed up:]

1. That the notion so long prevalent in the schools, that acute disease can be prevented or cured by means which depress and reduce vital and nervous power, is altogether fallacious.

2. That acute disease is not curable by the direct influence of any form of drug or any known remedial agent, excepting when it is capable of acting as an antidote, or of neutralizing a poison, on the presence of which in the system the disease may depend (*materies morbi*).

3. That disease is cured by natural processes, to promote which, in their full vigour, vital power must be upheld. Remedies, whether in the shape of drugs, which exercise a special physiological influence on the system, or in whatever form, are useful only so far as they may excite, assist, or promote these natural curative processes.

4. That it should be the aim of the physician (after he has sedulously studied the clinical history of disease, and made himself master of its diagnosis), to inquire minutely into the intimate nature of these curative processes—their physiology, so to speak—to discover the best means of assisting them, to search for antidotes to morbid poisons, and to ascertain the best and most convenient methods of upholding vital power. (p. vi.)

[We extract Dr. Todd's observations on the treatment of *rheumatic fever*. He considers that as many as seven different plans of treatment may be specified, all at one time or another having strenuous advocates. The first plan is that by venesection:]

It was formerly the prevailing opinion, and it is still, unfortunately, thought by some, that when called to a case of rheumatic fever one had only to open a vein, and if he could succeed in taking away a sufficient quantity of blood, which, in many instances, it was laid down should be little short of one or two pints, that by this large and rapid abstraction of blood, the disease may be cut short, and a malady, which ordinarily lasts some weeks, may be converted into one of a few days' duration. (p. 27.)

[Now, although by this mode of practice, frequently the more urgent symptoms are apparently quickly overcome, yet patients so treated linger on for considerable periods, suffering much from chronic rheumatism, and exhibiting an extreme anæmia, from which they but slowly, if ever, recover. The number of supporters of this system is very small at present.]

It is a practice from the adoption of which I would most earnestly dissuade you, as having the support neither of reason nor of experience, and as being fraught with the most dangerous consequences to your patients, (p. 28.)

The second plan of treatment is that by moderate bleeding and diaphoretics. This may be called an "expectant" treatment; but it is more than that as regards the venesection; while in other respects it sufficiently merits the name. The advocate of such a plan will say—"When I am called in to a case of rheumatic fever, I think it advisable to commence the treatment by abstracting about ten or twelve ounces of blood, and then to give sudorifics and purgatives."

Now, the objection which I entertain to such treatment is this:—that the routine abstraction of blood can scarcely be called *necessary* in any case, and that in many it is injurious. The tendency of rheumatic fever is to impoverish the blood, especially as regards that highly important portion of it, its colouring matter. All that bleeding really effects is to relieve pain (which, however, may quickly return) for a few hours, while it undoubtedly aids the bleaching power of the rheumatic matter, and, as I have observed in several cases, it increases much the tendency to a chronic rheumatic state, and consequently prolongs the convalescence. That bleeding in rheumatic fever is unnecessary, and that its omission diminishes rather than increases the tendency to certain internal inflammations, I am so convinced, that for several years I have not abstracted blood, in any way, in a single case of the disease. The treatment of rheumatic fever by the abstraction of blood, even in moderate quantity, but more especially in large quantity, appears to me to increase the danger of internal effusions into the pericardium and the pleura, and also into the synovial sacs of the joints. Under this treatment we also meet with the most violent and troublesome cases of delirium, which, under other methods, either does not occur, or is developed in a form sufficiently easily controlled. I am very much disposed to believe that this treatment predisposes to pericarditis and endocarditis; and that, if these affections occur in a case in which venesection has been freely practised, they are much less tractable than when you have to deal with them in a patient who has not suffered from loss of blood.

A third plan is that by mercury. Some recommend that calomel and opium should be freely administered until salivation is produced. The great objection to this treatment is, that it is an attempt to cure one fever by setting up another, and, in some respects, a worse; even supposing the original disease succumbs, your patient comes out of his rheumatic fever with loose teeth, ulcerated gums, and all the painful and offensive concomitants of ptyalism. Now, I say, that under such circumstances the remedy is nearly as bad as the disease; and, moreover, it does not in the least guard the patient against what may be termed the accidents of his malady—those severe internal inflammations—pericarditis, endocarditis, pneumonia, pleuritis, peritonitis. I have more than once seen pericardial inflammation supervene while the patient was in a state of salivation, of which the Case (2) of Sarah Green, detailed in the first lecture, is a good example. When we consider how differently various persons are affected by a mercurial course, and how much some suffer from it, even if given in small quantity, it would seem highly inexpedient to adopt this plan of treatment, for it assuredly offers no prospect of effecting either a speedy cure or a speedy convalescence, much less both together.

It is worthy of remark that rheumatic patients sometimes exhibit a distinct toleration of mercury, and are with difficulty salivated. The following case is an instance in point, affording, at the same time, but little encouragement to the supporters of the mercurial treatment, and yielding no evidence of the anti-rheumatic power of mercury. (p. 39.)

[A fourth plan of the treatment is by colchicum and guaiacum. Colchicum especially has long been considered to possess a specific influence over rheumatic and gouty affections. The influence of this drug in gout is another question; but in rheumatism it is of no use. Guaiacum has even less claim. Even in these few cases in which any good effect may follow their administration, a degree of prostration and debility is produced which is more dangerous than the original disease.]

5. Treatment by Opium.—This plan of treatment has lately been revived by a very able physician, Dr. Corrigan, of Dublin. It has much to recommend it, and, on the whole, you will find it extremely serviceable in practice; but I do not recommend it alone; its great value consists in relieving suffering, and soothing the nervous system, while it promotes diaphoresis. The opium is given in large and frequently repeated doses, care being taken not to produce too much narcotism; but upon this point, in general, there s not much need for fear, as there seems to be in the generality of patients,. i remarkable tolerance of opium. Our patient, Elizabeth Stocking, whose case I have described in the last lecture, was ordered on the 23rd a grain of opium, to be given every three hours, in addition to half a grain of muriate o morphia, which she had previously been taking at night; in forty-eight hours she thus took sixteen grains of opium, exclusive of the morphia, yet her pupils were not at all contracted, nor was she in any degree narcotized. The effect upon her has been most beneficial:—her nervous excitement has been calmed down, and her pain materially relieved. The same plan was pursued in the case of S. Green, also detailed in the last lecture; she had one grain of opium every three hours from January 8th to February 10th, excepting for one week, in which the dose was reduced to half a grain; the same tolerance of the remedy was observed. It will not, however, do to

employ this plan alone; it should be conjoined with other treatment. I do not recommend it by itself.

I may here adduce another case in illustration of the benefit to be derived from opium, where there is much disturbance of the nervous system with restlessness and delirium. (p. 48.)

A sixth plan of treatment, proposed long ago, by Dr. Haygarth, consists in giving bark in large doses, for which, more recently, the less bulky sulphate of quina has been substituted. Now just imagine the state in which the pathology of a disease must be, when measures so completely at the opposite extremes of our therapeutical resources are advocated for it—as venesection, to the amount of two or three pints, on the one hand, and large doses of quinine on the other; some would even give as much as five or ten grains two or three times a day. Now I have tried both methods of treatment, and I approve of neither; but if I were tied down to one or other of them, I should not hesitate to choose that by bark. In cases where the sweating is colliquative, and the urine copious and pale, with abundant precipitates of *pale* lithates, I have seen great good done rapidly by the use of quinine; but I am not prepared to advise you to adopt this treatment from the beginning, because it tends to check secretion, and so may favour the development of internal inflammations.

The seventh and last mode of treatment that I shall mention to you is, that which you have seen me adopt frequently at this hospital, namely, *the treatment by elimination*. I give it this name, in order that you may keep well in view its main object—to promote the elimination of morbid matter by the various emunctories, and also that you may bear in mind the view of the pathology of the disease upon which it is founded.

It is probable that *the materies morbi* in rheumatic fever is lactic acid or some analogous agent. We know that the natural emunctory of this is the skin. Many chemists maintain that it will also escape by the kidneys; and if it ever does so, perhaps this is more likely during rheumatic fever than at any other time. Again, since vitiated digestion is apt to produce it in undue quantity, and it, therefore, is formed abundantly in the stomach, there is every reason to think a certain proportion of it may be carried off through the alimentary canal. The indications are, then, to promote the action of the skin, the kidneys, and the bowels; to use antacid remedies; and to give large quantities of fluid for the free dilution of the materies morbi, and to supply the waste caused by the drainage from diaphoresis and diuresis.

The best way to promote the action of the skin is by opium, especially if you combine with it nitre and ipecacuanha. For this purpose I sometimes use a compound which resembles the original Dover's powder, in containing the nitrate of potass instead of sulphate of potass, as prescribed in the compound ipecacuan powder of the Pharmacopœia. Our usual prescription is one grain of opium, one grain of ipecacuanha, and five grains of nitre; this must be given every two, three, or four hours, according to the urgency of the symptoms, and the need the patient has for opium. This drug quiets the nervous system, and procures sleep, and with the ipecacuan promotes sweating; while the nitre acts upon the kidneys, and the ipecacuan may exercise some influence on the liver.

The best alkali on the whole is the bicarbonate of potass, which may be given in large and often-repeated doses—a scruple or half a drachm every third hour. Sometimes the acetate of potass answers very well in similar doses, and many physicians much prefer it to any other alkaline salt.

Next you must give purgatives to such an extent as to keep the bowels in a loose state, taking care not to carry this treatment so far as to weaken your patient, or to worry him by obliging him to be frequently moved in and out of bed. You will find it advantageous to use an alkaline purgative; and there cannot be a better medicine for this purpose than our hospital nostrum —the white mixture containing magnesia and sulphate of magnesia. Sometimes you may give the potassio-tartrate of antimony with advantage; but as it is a depressing remedy it is seldom advisable to use it.

But while we are thus alkalising our patient, and giving internally sudorifics and diaphoretics, ought we not to attend to the state of the joints? The diligent physician will tell you by all means to attack them at once:—but there is such a thing as "nimium diligentiæ" in physic as well as in other matters. Many will say, the best thing you can do is to leech a painful and swollen joint; I formerly tried this practice extensively, but for some time past I have not done so, as I generally found it either useless or injurious. You may apply leeches, and in a short time after you will find the pain and swelling removed, and you may be disposed to say, "here is a proof of their efficacy;" but wait twenty-four hours, and then you will generally find the pain and swelling as bad as ever, and the joint in just the same condition as before. Now apply leeches, and you will probably fail to give any relief. You have by the first application relieved the pain for a time, but you have produced no permanent good—you have rendered the disease more erratic, and less amenable to subsequent treatment. Frequently, when you leech a joint, the pain and swelling subside, but its fellow becomes swollen; leech it, and the swelling and pain return to the original joint. Nothing is more important to avoid, nor more troublesome if not prevented, than the erratic tendency of the rheumatic state. It will fly from joint to joint, and in pursuing it with leeches you will only drive it out of one joint into another. I am satisfied that leeching the joint favours this erratic tendency.

I am not prepared, however, to advise you to neglect the local treatment of the joints. When they are much swollen and painful, you may give great ease to your patient by enveloping them in a large quantity of the soft carded cotton—commonly called *cotton-wool.* Over this you must wrap a sheet of oiled silk, so as to cover in the wool completely, taking care to have no part of it exposed. By this air-tight covering you keep the joints in a complete vapour bath; and when you come to remove the oiled silk and wool, after twelve or twenty-four hours, you find the wool completely saturated with moisture, which generally is strongly acid. You have seen this in Elizabeth Stocking's case. We find the plan so generally useful, that it is adopted in the hospital in nearly every case; it affords great relief, supports and keeps the limb steady, and at the same time promotes sweating. I may just mention, that this plan of enveloping the joint in wool and silk is also very beneficial in gout.

In a few, and only a very few, cases, I have found the pain aggravated by the heat which this mode of wrapping generates; and in cases where it is desirable to keep down the sweating, it is not advantageous to carry this plan beyond a day or two.

The best additional local treatment is that by blisters of small size, applied on or near the affected joints; they are very useful both in acute rheumatic and acute gouty joints. I shall refer to this subject again.

You perceive that all the means employed in this mode of treatment tend to elimination, and to the relief of pain; the opiate sudorific affecting the skin; and the nitre and alkaline salt acting on the kidneys; the purgatives on the mucous membrane of the bowels; the wool and blisters on the joints.

During this treatment, while you allow your patients the liberal use of simple diluents, you must give a fair amount of nourishment from the first; and I think this may be best supplied by a small quantity of good beef-tea, given frequently throughout the day. (*pp.* 49—54.)—*Dr. R. B. Todd.*

160.—*The Causes and Treatment of Imperfect Digestion.* By ARTHUR LEARED, M.B., M.R.I.A., Physician to the Great Northern Hospital.—*London,* Churchill, 1860. pp. 224.

[From this work, which is well worthy the perusal of our readers, we extract the following on the efficacy of strychnia in the treatment of dyspepsia.]

Strychnia acts not only as a bitter, but possesses other valuable properties in dyspepsia.]

It need hardly be urged that this energetic drug requires to be cautiously administered, but its effects will amply repay the care. Speaking from extensive experience, I know no single medicine of more value in this disease. Strychnia is particularly indicated in dyspepsia attended with nervous debility. In that numerous class of cases in which abnormal sensations in various parts of the body—as the throat, the head, or the limbs—are experienced, it will generally be found useful. It is the best tonic for the class in which mental symptoms predominate. But it also possesses excellent local effects, and acts by increasing the tone of the muscular coats of the stomach and intestines. When these coats are relaxed, gases are generated, mainly owing to retardation of the aliment in the cavities. No remedy has in my hands proved so permanently effectual as strychnia against this inconvenience. In the case of a gentleman who suffered most severely from sudden and almost daily accumulation of gas in the stomach and bowels, these attacks were attended with great mental oppression; often by fits of crying. The symptoms, in fact, resembled those of hysteria very closely. I mention the case particularly on account of its severity, and because the patient was cured by strychnia; and some time has now elapsed without a return of the attacks.

Strychnia should always be given in solution for the sake of its more effective diffusion in the stomach, and for the sake of greater certainty in apportioning the dose. A proportion of some acid, as a drachm of dilute acetic acid to a grain of strychnia, should be prescribed with it, as the action of strychnia is greatly favoured by association with acids. The discrepant accounts given of it, in all probability depend on different degrees of acidity in the stomachs into which solid strychnia was introduced. As a bitter tonic, the fortieth part of a grain is a sufficient dose. When it is desired to insure specific action, the twentieth part of a grain may be given; but it will seldom be necessary to exceed this, as the good effects of strychnia on the gastro-intestinal muscular fibres are usually secured by a quantity that does not affect the voluntary muscles. A solution of strychnia of definite strength that will keep, which that made with acid will not do, is a desideratum in the forthcoming national Pharmacopæia.

A preparation of citrate of iron and strychnia, has lately been introduced, but it has no claim to be regarded as a chemical compound. It is simply a useful mixture; but the variable proportions adopted by the makers, within my own knowledge, ranging from one part of citrate of strychnia in ninety parts, to one part in one hundred and fifty, is highly objectionable.—*Dr. Leared, p.* 185.

161.—*Hemorrhoids and Prolapsus of the Rectum, with especial Reference to the Application of Nitric Acid.* By HENRY SMITH, F. R. C. S., Surgeon to the Westminster General Dispensary; formerly House Surgeon to King's College Hospital.—*London*, Churchill, Second Edition, 1860, pp. 108.

[Various papers have at different times appeared in our pages, upon the treatment of Hæmorrhoids by the application of Nitric Acid. The following gives, in a very clear light, the class of cases in which this mode of treatment is especially useful, distinguishing them from those cases in which the use of the ligature is more suitable.]

I have stated, whilst describing the nature of internal hæmorrhoidal diseases, that the condition in which they are found varies. I have endeavoured to point out those cases to which the operation of the ligature is applicable, and have stated that this practice is necessary and justifiable in those instances where the tumours are large, mainly composed of tissues in which the veins predominate, and have become indurated. There are, however, other instances when the hæmorrhoidal tumours are small or moderate in size, and where they are evidently composed of morbid texture, in which the small arteries rather than the veins are interested, as shown by their bright florid aspect, and their tendency to pour out arterial blood whenever the patient is at the closet, or when the tumours are handled. These tumours are generally not very prominent. They produce exceeding annoyance, and indeed prove more destructive to the health, as they generally yield a great deal of blood. Now, in such cases the ligature will undoubtedly be as effective as in the other instances before described; but this proceeding is not necessary, as the local use of the nitric acid is so eminently suited to them. The relief which one single application of the acid gives in these cases is remarkable, and an excellent cure may be effected, if the whole of the diseased texture be subject to its action.

About these particular kind of cases, there is no doubt in the mind of any surgeon who has seen the nitric acid applied in a proper manner. There is, however, a mixed class of cases where the remedy is an uncertain one, but in which, nevertheless, the surgeon is justified in trying it, and where I sometimes have succeeded when I little expected it. I refer to those cases where there is a hæmorrhoidal mass, consisting perhaps of one tumour, mainly composed of venous ramifications, and of a bluish color, with one or more presenting the characters of the florid sessile pile; or one portion of the tumour or tumours may present the dark blue appearance and thickened membrane, and another portion of it may be brightly vascular, have its mucous covering granular and slightly ulcerated. In this kind of mixed case, I do not hesitate to try the acid if the patient is particularly desirous, but I make a point of stating that it is impossible to depend upon any curative action in such, although in some instances the remedy has acted most efficiently.

To apply the agent to those cases where the tumours are large and indurated, and have a deep blue colour, would be perfectly useless, and only bring discredit upon the nitric acid as a means of cure in other kinds of hæmorrhoidal disease. I particularly wish to point this out, as it is very probable that some of my brethren think that I am an enthusiast with regard to the use of this remedy, and recommend it too strongly. On the contrary, I always urge upon the patient the necessity and propriety of the ligature in such, and even in the more doubtful case just alluded to. It is not necessary, because a surgeon may have reason to adopt, with confidence, a particular kind of remedy, that he should be an enthusiast, or should be blind to the value of those other means which are generally recognised as suitable and efficient.

It is, however, in that class of cases not unfrequently met with, where there is not so much any decided hæmorrhoidal tumour, but where there is a generally congested and relaxed condition of the mucous membrane of the rectum, attended with bleeding to a greater or less extent, that the nitric acid acts so beneficially. Dr. Houston has compared this condition of the rectum not inaptly to that of the thickened conjunctive after long-continued ophthalmia. The application of the acid to the diseased points from which the bleeding proceeds, will soon remedy all the bad symptoms.

Having described the cases to which the agent is applicable, it is fitting that something should be said about its mode of action.

When the strong nitric acid is applied in a very limited degree to the vascular mucous membrane, or granular tumour, I believe it acts beneficially, much in the same way as does the nitrate of silver, or other powerful caustic, when applied to a spongy, irritable sore on the leg by altering the condition of the minute vessels contracting them and perhaps causing coagulation of the blood in them. When, however, it is used more freely, superficial destruction of the tissue to which it is applied takes place, an eschar forms, this is in time removed, cicatrization necessarily ensues, vessels which formerly bled are closed up, the tissues generally are braced up and contracted, and probably adhesion is formed between the mucous and muscular tissue of the gut. By further applications of the acid to the same part, the morbid texture may be actually destroyed; and hence the remedy, powerful as it is, requires great care in its use.

I will now describe the manner in which the acid should be applied.

The bowels having been well evacuated some hours previously, the diseased portion to which the application is going to be made should be well exposed, by making the patient sit over hot water for some few minutes; or, if this is not sufficient, an enema of water should be thrown up the rectum, and the hemorrhoidal disease will be brought well into view. The part to which the acid is to be applied, should then be carefully wiped with a portion of lint. The surgeon then dips the extremity of a small, flat piece of wood into the nitric acid, and touches the diseased surface carefully with it. The part touched, and the neighbouring mucous membrane is well smeared with oil, and the whole of the exposed part is returned within the orifice.

There are certain details connected with the application of this agent which require attention. Thus, the acid which is used should be the strongest and purest which can be obtained. I have generally employed the colourless acid, but sometimes I have thought it desirable to use the brown *fuming* nitrous acid, which acts more energetically. I have used for the most part a piece of wood as the means of carrying the acid on to the part, but some object to it as liable to be acted upon by the agent, and therefore recommend a glass rod or brush, both of which suffice very well, but I do not think it much matters.

It is important to touch the part only lightly, especially in those cases of vascular piles where bleeding easily takes place; for the blood escaping and mixing with the acid, will in a measure neutralize its effects. I also lay great stress upon the necessity of wiping the diseased part with lint, previous to applying the acid, for there is always a certain amount of mucous covering these tumours, and unless this be removed, the nitric acid will not act efficiently.

I have recommended that the diseased parts should be brought down either by the action of an enema, or by the efforts of the patient sitting over warm water; but there are cases where it will be difficult and unpleasant to effect this object in either way, and in this case I adopt the plan recommended many years since by Mr. Fergusson, of using a silvered glass speculum, by the introduction of which a good view of the hemorrhoidal tumour may be had, and the nitric acid can be readily applied to the part through the instrument, into the eye of which the diseased tissue is by a little management easily engaged.

[In these cases we have usually employed Weiss' female urethra dilator, (a wood-cut representing a modification of this instrument, will be found at p. 161 of this volume.) It is applied with the greatest ease, and permits of a freer application of the acid, than the speculum.—Ed. Retrospect.]

How often will it be necessary to apply the acid? The answer to this question will depend upon the nature of each case. When there is only one vascular tumour, or a limited amount of disease, one application pretty freely made, will suffice; but in cases where there are several tumours, or points of disease, as many operations will be needful.

It is necessary for me to say a few words regarding the amount of suffering produced by the application of the nitric acid. Most persons naturally connect *pain* in their imagination with the use of any caustic, arguing from the effects of the agent on the integument. Doubtless, the various caustics which are used cannot be applied to the skin without causing much suffering; but is far otherwise with the mucous membrane, especially that of the rectum. The general and immediate effect of the acid in most cases, is to cause merely a slight smarting or warmth, which goes off very speedily. In some instances, actual pain of a somewhat severe character is produced, especially when the surface touched is close upon the anus. There are however instances of hemorrhoids and prolapsus of long standing, where the parts have become so insensible, that the patients actually do not know when the acid is applied. The reason why I prefer applying it through the speculum is, that the proceeding is perfectly painless, patients having asked me the question whether the acid has been applied or not when the operation is quite finished. It is however a very different matter if any portion of the caustic comes in contact with the sensitive skin of the anus. Should such occur, the pain will be very severe, and will last for hours; hence the obvious necessity of taking the utmost precaution against this.

I can truly say that it has never occurred to me to witness anything like a fatal, or even a dangerous result, after having had a large experience of this remedy. In one case of a patient, who was most anxious to be cured by one operation, I applied the nitric acid much more freely than usual, and produced great suffering for two or three days, with the effect, however, of making a good cure. In another instance, I heard, but was not a witness of the fact, that copious bleeding followed the use of the acid. In a third instance, which occurred very lately, a young lady was treated with the nitric acid for a florid pile. Severe and unaccountable suffering was produced for many days. On making a careful examination, at the end of this period, I discovered a small ulcer, situated at the posterior verge of the anus, and exquisitely painful; suitable remedies relieved the pain in a few hours. On inquiring more minutely into the particulars of this case, I have reasons for believing that this ulcer existed before I applied the acid, and that some of the caustic came into contact with the sore, and produced the most severe suffering. The existence of a painful ulcer or fissure, in conjunction with hemorrhoidal tumours, is not unfrequent; and an examination, with a view of ascertaining this point, should be instituted, especially if it is found that the patient complains of more than usually severe pain, when at the closet, and lasting for some time subsequently. It is needless to mention that the application of the nitric acid to the rectum, when this does exist, must not be thought of.

I have now and then met with cases where retention of urine, and pretty smart bleeding, have occurred after the free use of the nitric acid, but never sufficient to cause me anxiety, and they are symptoms which are easily met.

It is not necessary to confine patients to their bed after the acid has been applied, and this is one of the reasons why the remedy is so desirable, many patients having neither the time nor inclination to submit to an operation which may keep them from their business or pleasure for a fortnight or more. (pp. 38–48.)—*Mr. H. Smith.*

162.—*A New and Rational Explanation of the Diseases peculiar to Infants and Mothers; with Obvious Suggestions for their Prevention or Cure.* By THOMAS BALLARD, Esq., M.R.C.S., L.S.A.—*London*, Churchill, 1860. pp. 128.

[The proposition which the author in this work endeavours to prove is, that "the exercise of the instinctive act of sucking, under circumstances unfavourable to the infant obtaining food, is a principal source of disease to infants and puerperal women."]

Dr. Brown Séquard, in his 10th lecture, published in the 'Lancet,' says, "The excitation of the nerves of taste produces an abundant reflex secretion of gastric juice, and also a flow of bile and pancreatic juice in the bowels." The act of sucking being, par excellence, the mode to induce this excitation, it is obvious that these juices must be secreted abundantly during the exercise of this act; and if at the same time a supply of food is received into the stomach, the solvent power of the gastric juice is exerted upon it, and the first process of a healthy digestion results; but should the *sucking* have been *fruitless*, or partially so, it will in all probability have been *forcible* and *prolonged*, and therefore attended by an excessive reflex secretion of gastric juice, which, not meeting with a proportionate supply of food whereon to expend its solvent power, acts upon the mucous coat of the stomach and intestines, and causes various degrees of injuries thereto, viz., destruction of the folds of mucous membrane in the upper portion of the small intestine, known as the valvulæ conniventes, thinning and softening of the coats of the stomach and intestine, and, as an extreme effect, complete destruction of the posterior wall of the stomach itself.

The evidence of this process of injury to the intestinal canal being in operation, is the presence of those symptoms which immediately result from it, viz., abdominal pain, and the frequent and green stools to which sucking infants are so liable. A persistence of this morbid state causes many of those ailments which are usually attributed to Teething, but which are really remote effects, dependent upon the imperfect development of the various tissues of the body, which necessarily results from the gradual injury inflicted on the organs concerned in the first and most essential processes of digestion; these are exemplified in the skin by erythema, urticaria, and eczema; in the system generally, by defective appetite, anæmia, tardy growth of the whole body, and general debility; and probably the establishment of that condition of the system known as strumous; in the osseous system by imperfect development of the bones, giving rise, in the case of the long bones, to crooked extremities, and in the short bones to those various forms of carious disease which are apt to succeed to a very trivial local cause; and by the tardy growth of the teeth; in the nervous system by laryngismus stridulus, convulsions, cerebral congestion, and hydrocephalus; in the abdominal viscera, by occasionally causing invagination of the intestine, or eventually producing the morbid condition known as tabes mesenterica; in short, all the diseases of infancy, in which we can recognise a failure of the processes of growth and nutrition, may fairly be attributed to FRUITLESS SUCKING, as a principal, if not the chief cause. (p. 1.)

[Dr. Budd in his admirable work observes:]

"In infants the softening of the stomach is found unconnected with organic disease of other organs much more frequently than in adults, because in them the functional gastric disorder, which may be excited by teething or other causes of disturbance, rapidly exhausts the strength, causing a state of collapse, and thus proving fatal of itself."

Neither Dr. Wade nor Dr. Budd attempt to assign a cause for these morbid changes in the intestinal canal of the infant; but I think I shall be able to show that they may be attributed to the action of the excessive secretion of gastric juice, which results from the persevering efforts which are made by an infant sucking, to obtain its food, when placed in unfavourable circumstances for doing so.

The state of fatal collapse alluded to by Dr. Budd, is probably more frequently attended with entire destruction and consequent perforation of the posterior wall of the large end of the stomach than has been generally noticed, and I suggest that it is the cause of death in many of those cases of "infants found dead in bed;" of the nature of which cases the following notice, extracted from the 'Lancet,' of February, 1855, gives some idea:—

"These lifeless little bodies are discovered in at least ninety-five instances out of every hundred, after three o'clock in the morning; not one out of a hundred of such bodies is discovered dead between nine and twelve at night. An experience of fourteen years in a coroner's jurisdiction, embracing between eight and nine hundred thousand souls, has established the accuracy of these statements, by proofs which admit of no dispute. Equally true is it, that out of hundreds of examples of infants found dead in bed, only two instances have been seen in which the proof was conclusive, that the little creature had been destroyed by the pressure of persons who had been lying with them in bed, even in one of these the question might have been fairly raised." The probable explanation of these cases is the following:—The infant is accustomed to fruitless sucking, and has, in censequence, suffered for some time before, with frequent and green stools, the stomach and intestines being consequently thinned and softened; after midnight there is no prospect of food—if the mother's breast can be reached it has been previously drained, and her nervous system requires the recruiting influence of rest and sleep to enable it to secrete more milk, the infant's cravings are not satisfied, it sucks its thumb or fingers violently, or perhaps it is supplied with the *sugar teat*, with the view of pacifying it to sleep, but under any of these circumstances, the little sufferer sucks on in search of food, none is obtained; the gastric and other juices attend as usual, reflexly to their summons, and meeting with no food on which to expend their powers, complete the task of destruction they have been long engaged upon; the stomach is perforated—and instant death results. (p. 3).

The author then passes on to trace individual disease to excessive and fruitless sucking, His views on *thrush* are explained in the following short paragraph:]

The true nature of the disease, I suppose to be, that the delicate mucous membrane of the mouth becomes inflamed, in consequence of excessive sucking, and that an inflammatory exudation results, which, from its situation, being warm and moist, affords a very appropriate nidus for the growth of the oidium which is found so universally growing under circumstances equally favourable to its nature. The accompanying enteric symptoms coincide with those I have previously described as resulting from fruitless sucking. The best confirmation I can adduce in proof of the correctness of this opinion, is the fact, that in all instances where I have been able to make my views of the cause of the disease understood, no case of thrush has occurred; and, in addition to this, the relation of some cases of which I have notes, where I have successfully treated the disease in accordance with them. (p, 13.)

[How is the infant to exist for three days, if, as is generally the case, the mother has no milk for that period? Mr. Ballard answers this question by relating the following case:

A healthy woman gave birth to her ninth child at the full period. The nates presented, and the accoucheur being young and timid, the delivery was not effected with sufficient rapidity to save the life of the infant, which was exceedingly well developed. Having obtained permission to examine it, I found the stomach full of a soft pultaceous substance, somewhat resembling apple-sauce; the capacity of the organ was equal to four fluid drachms. The small intestines contained a large quantity of a similar substance, rather brighter in colour, and apparently more coagulated; the colon contained the peculiar matter known as meconium. Is it not reasonable to suppose that these substances were natural to the situation in which I found them, and that a similar condition exists in the intestines of all infants? and may we not suppose that an infant would not require milk until these substances had been disposed of? (p. 113.)—*Mr. T. Ballard.*

[In conclusion, we may say, that this little work is full of original matter, and is evidently written by a man accustomed to think for himself. We confine ourselves to giving an author's views, and leave our readers to form their own opinions as to their truth or error.]

A SYNOPSIS,

CONTAINING A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THE PRECEDING PAGES: SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE LAST HALF-YEAR. (ARRANGED ALPHABETICALLY.)

AFFECTIONS OF THE SYSTEM GENERALLY.

GOUT.—The dietetic and medicinal treatment of gout may be thus summed up:—In the majority of cases all that is necessary is light amylaceous diet, with free use of diluents, but no alcoholic stimulants. Simple alkaline salts with moderate doses of colchicum are required, and, if necessary, an occasional purgative. If there be plethora. the abstraction of a few ounces of blood is advantageous, but, on the other hand, if the vital powers are at a low ebb, sesquicarbonate of ammonia should be substituted for the other salines, and the colchicum altogether omitted. Lithia possesses the remarkable property of forming the most soluble salt known of uric acid; hence the carbonate of lithia is a salt well calculated to be used a a remedial agent in the treatment of gout. (Dr. A. B. Garrod, p. 35.)

RHEUMATISM, *Acute.*—It is a mistake to suppose that the profuse sweating in cases of acute rheumatism is an eliminative effort of nature: those cases are the worst in which it is most marked and prolonged. Any treatment tending to increase this will prolong the case. Tincture of the muriate of iron is a remedy of much value, when this symptom is excessive. (Dr. Inman, p. 37.)

In acute rheumatism where there is intense suffering, and constant restlessness, give opium freely. If necessary give a grain every hour, until relief to the patient's suffering is obtained. It may be advantageously combined with small doses of the compound colocynth pill. Thus administered it will not be found to "lock-up" the secretions. In fact, it acts favourably as an eliminator, and takes the patient pleasantly ("jucunde") through his disease; and is often followed by a rapid convalescence. (Dr. W. O, Markham, p. 77.)

Colchicum. when combined with opium, acts far more efficaciously than when given alone, the one drug seeming to modify the action of the other. Twelve parts of colchicum wine may be mixed with two of tincture of opium, and taken three times a-day. The colchicum wine is more active when made according to the Prussian Pharmacopœia, which directs 150 parts of colchicum seeds to be macerated in 770 of alcohol. This treatment is of no use in old and chronic cases. (Dr. Eisenmann, p. 38.)

STHENIC INFLAMMATIONS.—Antimony is particularly adapted to arrest pulmonic and renal inflammation; mercury, iritis and peritonitis; chlorate of potash, buccal inflammation; iodide of potossium, periosteal; and colchicum, synovial inflammation. Alkalies internally and applications of lead externally, arrest cutaneous inflammations; mercury and iodide of potassium, syphilitec; and alkalies and their salts, rheumatic inflammation. (Dr. C. H. Jones, p. 288.)

AFFECTIONS OF THE NERVOUS SYSTEM.

COMA AND NARCOTISM.—When a patient is comatose the lungs gradually become more and more congested, effusion takes place into the pulmonary tissue, and accumulates in the bronchial tubes; yet there is no actual paralysis of the respiratory muscles. In death from narcotism the respiratory muscles and tongue are paralysed, but there is neither the rapid effusion nor the great congestion of coma. Now the treatment of these two cases is quite different. In the treatment of the former, venesection is our sheet-anchor, to relieve effusion and prevent its further formation, and to favour the exit of the bronchial effusion the prone position is necessary. Artificial respiration is unnecessary. In the latter, artificial respiration is invaluable, whilst venesection is uncalled for. The prone position is necessary on account of the lingual paralysis. (Mr. W. C. Hunter, p. 54.)

DELIRIUM TREMENS.—In the treatment of this disease a successful issue can be obtained without the use of either opiates or stimulants. It suffices to keep the room in a state of tranquility and partial darkness, giving gently nutritive and easily digestible food, and keeping the bowels open by gentle cathartics. If the patient is yet labouring under the effects of a very recent debauch, an emetic may be given with advantage at the commencement of the treatment. In time the hallucinations will disappear, sleep return, and the patient be restored to health. The results of actual experience in this plan of treatment are favourable. (Prof. Dunglison, p. 55.)

EPILEPSY.—The phenomena of epilepsy are owing to the "gradual accumulation and sudden explosion" of nervous energy; consequently, one of the principal agents in treatment is to prevent this gradual accumulation. The object may be attained in two ways viz., "the artificial and the natural." The former consists in keeping up a constant involuntary action of the muscles, by the administration of strychnine internally, and the application of electro-galvanism externally. By these means the attacks can be warded off to an indefinite period. The natural method consists in the constant voluntary exercise of the muscles combined with very simple diet and pure air. By his temperance in living, by the simplicity and activity of his habits, by the multiplicity of his avocations, and by the hardiness of his amusements and pursuits, the late Duke of Wellington prevented the recurrence of the fits of epilepsy, which appeared in the later years of his life. (Dr. I Pidduck, p. 51.)

PERIPHERAL OR CUTANEOUS NEURALGIA.—Rapid and permanent relief may frequently be given by the application of the continuous galvanic current. The best apparatus for this purpose is the Pulvermacher chain, as it can readily be applied to any part of the body. (Mr. H. Lobb, p. 57.)

PARALYSIS AGITANS.—In paralysis agitans confined to one limb, benefit may be expected from the application of a continuous galvanic current (direct) to the limb. It will require perseverance, and must be backed up by measures tending to improvement of the general health. An interesting case of this nature is related, in which, after five applications, the involuntary movements completely ceased. (Dr. J. R. Reynolds, p. 53.)

SCIATICA, LUMBAGO, AND OTHER PAINS.—In many cases of local pain, as in sciatica and lumbago, the pain returns as bad as ever after the narcotic effect of the morphine injected passes off, when that drug is injected hypodermically; but when atropine is employed, the pain disappears more permanently. The strength of the solution of sulphate of atropine employed, should be gr. ij. to ℥j. of water; and of this, from ten to thirty minims may be injected at once. Besides the relief of the pain, the other symptoms produced are, slight drowsiness, dryness of the throat, and dilatation of the pupil. (Dr. Cowdell, p. 60.)

TETANUS.—A case of tetanus is related, in which the internal administration of chloroform appears to have relieved the paroxysms. Ten drops were at first given every twenty minutes. (Dr. A. Dick. p. 51.)

Mr. Ellis, in the nineteenth volume of the Medical Commentaries, recommends the use of injections of tartar emetic in large doses, a drachm at a time, and repeated two or three successive times, if necessary. Its combination with opium, the sedative power of which it remarkably increases, is recommended. (Dr. Mackenzie, p. 380.)

Woorara.—Up to this time (Dec. 1859) two cases of chronic tetanus in man on the continent, and one in this country, have recovered under the treatment by woorara. When taken by the mouth the action of woorara is extremely doubtful. It may be applied to the wound, or inoculated in the arm. It is brought to this country as an extract, the active principle of which is the alkaloid curarina. Woorara paralyzes the motor and not the sensor nerves, and may be so administered as to destroy entirely the power of voluntary motion without impairing the consciousness of the animal; and if a poisonous dose of strychnine be given to an animal, woorara may be administered to the extent to paralyze all the voluntary muscles except those of respiration, and by continuing the thus moderated action of the drug until the kidneys have time to eliminate the strychnine, the life of the animal may be saved. (Mr. T. S. Wells, Dr. Harley, p. 47.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

ACUPRESSURE.—*A new mode of arresting Surgical Hemorrhage.*—With the single exception of torsion, we may safely assert that at the present day ligatures are the means universally employed with the view of arresting hemorrhage. Yet to sew up the outer or external lips of a large surgical wound by metallic non-irritating sutures, and to leave within its depths a series of other ligatures, each of which must, before it can be removed, produce ulceration, suppuration, and gangrene, at the tied arterial points, is a very paradoxical state of matters. To obviate this, Dr. Simpson proposes a new mode of arresting hemorrhage from wounded arteries, which he has tried with perfect success in several operations upon the human subject. The instruments required are some very sharp-pointed slender needles, or pins of passive or non-oxydizable iron, headed with wax or glass, like the hare-lip pins commonly used by surgeons at the present day. By these the arteries are to be compressed for about two days after the operation, when they may be withdrawn, and there will not be any irritating substance within the wound. If the artery be within the flap a needle is passed through the skin down to one side of the vessel, then passed over it, and again re-entering the surface of the wound brought out through the skin at the other side, exactly as a flower is fastened in the lapelle of a coat by an ordinary pin. The amount of pressure can be regulated and increased when required by the acuteness of the angle at which the needle is introduced and again passed out. If the artery be deeply situated, and in the midst of soft textures, two needles may be employed and passed down parallel to each other and at a little distance apart, till the vessel is reached and just passed, then, by changing the direction of the needles to make them converge, and pressing a little further in, the vessel will be compressed between the two. When the vessel is near a bone, it may be compressed against it, and only one needle is required. This mode of arresting hemorrhage is more easy, simple, and expeditious in its application than the ligature, and little or no irritation is caused in the wound. Phlebitis, pyæmia, and other dangerous complications will be much less likely to arise when this mode of arresting hemorrhage is employed. Some will question whether hemorrhage can be effectually arrested by these means, but it is practically found to answer very well, and much less pressure suffices than would a priori be supposed. (Dr. J. Y. Simpson, p. 132.)

EPISTAXIS.—A case of most intractible epistaxis is related by Dr. Davenport of Iowa, U.S., in which almost every conceivable measure seems to have been used without any avail. Plugging had no effect, the blood still forcing its way out. Tannin was introduced and nitrate of silver injected. At last Dr. D. injected a quantity of the undiluted perchloride of iron with the effect of at once arresting the hemorrhage, but not before alarming symptoms had been produced from loss of blood. (Dr. F. Davenport, p. 88.)

IMPAIRED CONTRACTILITY OF THE HEART.—It is well known that valvular diseases of the heart, if only slight actual changes have taken place, may continue for years without causing much inconvenience to the patient, providing the contractility of the chambers is not impaired. This applies almost with more force to slight mitral than to slight aortic diseases; for the thickness and great elasticity of the endocardium of the left auricle, powerfully supports the muscular wall of that cavity. The first beginning of downward progress of the case is, then, when the muscular power begins to fail, and it is precisely at this stage of the case that digitalis is most useful. It should always be combined with iron. Under its influence the action of the heart becomes slower and more regular, and the pulse fuller. To have the above effect, digitalis must be given in very small doses (four or five minims of the tincture); if given in larger doses, the diastole of the heart is prolonged, and we lay the foundation for the supervention of that very condition which it is our object to avoid—dilatation. (Dr. J. Cockle, p. 73.)

FIBRINOUS DEPOSIT IN THE HEART.—There are some cases in which death takes place not from the disease itself, but from fibrinous deposit in the heart—as occasionally in puerperal fever. In many others this event accelerates death, as in pneumonia, pleurisy, and other sthenic inflammations. Death is occasionally seen to follow abstraction of blood or the administration of purgatives in inflammations, owing to fibrine being deposited in the heart, in consequence of its relative increase in quantity to the watery parts of the blood, (Dr. W. B. Richardson p. 62.)

HEMORRHAGE FROM TONSIL.—An interesting case which occurred at the Marylebone Infirmary, very forcibly illustrates the value of perchloride of iron as a styptic. Hemorrhage had continued from an incision in the inflamed tonsil, till the patient was pale and pulseless. It was speedily arrested by the perchloride. A glass brush should be kept for use with it. (Mr. H. Thompson, p. 138.)

LEECH-BITES.—A few layers of collodion will rapidly arrest hemorrhage from leech-bites, when too profuse. (M, S. Martin, p. 139.)

RHEUMATIC PERICARDITIS.—The great danger in this disease is, failure of the heart's powers. This is generally preceded by certain symptoms, as a feeble, intermittent, and irregular pulse, turgescence of the jugular veins, feebleness of the first sound of the heart. Inflammations of this character are much more liable to supervene in weak than in strong constitutions, and must, to be treated successfully, be regarded as signs of weakness. We must be very cautious, in the first instance, not to injure the patient by depletion; and, in the second place not to delay too long the use of stimuli. (Dr. Stokes, Dr. W. O. Markham, p. 77.)

VARICOSE VEINS.—All operations for the cure of varicose veins, which include opening the vein, whether it be done by caustic or the knife, are dangerous, by leading to suppuration within the vein. We have repeatedly alluded to Mr. Erichsen's plan of operating in these cases, and this plan is still followed by him with great success. In his recent cases the operation is thus performed:—A hare-lip pin is passed underneath the vein on one side, and its point brought out on the other; a piece of elastic bougie, about an inch in length, is then laid over the vein parallel to its course. Then, by means of a silken thread twisted over the bougie, and under the two ends of the pin, the vein is compressed between the pin and the bougie. The principal points to be noted are—to dip the pin sufficiently deeply so as to avoid all chance of transfixing the vein, and not to apply the ligature so tight as to cause ulceration of the parts. The pin should be removed about the tenth day. The injection of perchloride of iron is decidedly more dangerous than the above plan, but must be resorted to when the knots are so large, or so closely matted together that the pin cannot be passed underneath them. (Mr. J. Erichsen, p. 139.)

Nævus and Vericase Veins—The perchloride of iron requires to be used with great care, when injected subcutaneously. In a child, it produced sloughing of the tissues down to the mucous membrane when injected into a congenital nævus of the cheek; and in a case of varicose veins, it produced suppuration and much constitutional disturbance. (Mr. H. Thompson, Mr. P. Hewett, p. 138.)

AFFECTIONS OF THE RESPIRATORY ORGANS.

ASTHMA.—In a case of asthma, of considerable severity, the injection of a solution of sulphate of atropine over the seat of the pneumogastric nerve, was followed by great relief. All the usual remedies had been tried without avail. No organic pulmonary or cardiac lesion of any consequence existed. The point selected for injection was over the track of the pneumogastric nerve, between the left sterno-mastoid and the thyroid cartilage. The usual symptoms of belladonna poisoning occurred in a few minutes. This was repeated several times, the unpleasant effects being counteracted by the employment of opium. The spasmodic attacks were arrested, and apparently permanently. (M. Courty. p. 89.)

CROUP.—The following plan of treatment is the result of much experience in a purely country practice. At the commencement, give an emetic of pulv. antim. tart.; and if the child is an unusually strong one, a leech may be applied at the same time. Then, for an infant of six months, give a grain and a half of calomel every hour till it purges freely. To a child a year old, give two grains, and to one two years old, even four grains. The stools will become of a greenish colour, something like chopped spinach, and, immediately consequent on this discharge, an alleviation of the symptoms follows. Salivation cannot be produced by this treatment *in children.* The calomel must be given till the disease succumbs. Of course, this mode of treatment is inapplicable in cases occurring in large towns. (Mr. C. Fergusson, p. 90.)

Dr. Richardson states that he has treated croup at its onset with chloroform by inhalation with a success such as he has not seen follow any other remedy. But the effects of the drug must be fully carried out, and sustained for many hours.—(Brit. Med. Journal, April 7, 1860, p. 261.)

PHTHISIS.—We must cease to regard phthisis as a single specific disease, to be treated on one uniform plan. It is essentially a state of system in which there is excessive waste, or deficient and imperfect formation of tissue. Now this may proceed (amongst others) from two different and opposite causes, viz., excess of acid, or excess of alkali in the system—for a normal alkalinity of the blood is essential to a proper performance of the formative process. It is well in cases of phthisis to examine the perspiration. If very acid, great advantage will frequently be derived from the administration of a small quantity of alkali; bicarbonate of potass and gentian may be given. If, on the other hand, the perspiration be neutral (it is seldom alkaline) acids are indicated. A case is related in which, with profuse acid sweats, the administration of oils and other remedial means were not productive of any benefit, till the excessive acidity had been neutralized by a little alkali. (Dr. E. Smith, p. 80.)

The opinion of medical men on the subject of the utility of hypophosphites in the treatment of phthisis is singularly at variance. Drs. Quain and Cotton, both physicians to a hospital for consumption, find the salts of no use whatever, and patients who had been stationary under their use immediately began to improve under steel and cod-liver oil. Dr. Dickson, of Jersey, has, on the other hand, found this plan of treatment of great advantage, in two-thirds of the cases in which he has tried it (about thirty), and several of his medical friends have told him that the result in their hands has been most extraordinary. Dr. Churchill, in Paris, has followed it extensively with much success. A writer in the Medical Circular, signing himself R. C. H., says that the hypophosphites stimulate the appetite for food, diminish the expectoration and sweating, and though not curing phthisis, they retard its progress. (Dr. R. Quain, R. C. H., Dr. Dickson, pp. 83, 84, 86.)

The chief differences between the climate of Cairo and that of Madeira, are that the latter is very moist, and the variations of temperature very trifling; whilst the climate of the former is very dry, and the atmospheric variations pretty considerable during the winter months. These two climates are suitable to different classes of cases of consumption. "An acute form of phthisis, with intense fever, frequent short cough, great dyspnœa on exertion, expectoration scanty, but often sanguinolent; such are the symptoms which appear to indicate a residence in Madeira: whilst patients in whom the pulmonary disease is running a chronic course, who are subject to exacerbations followed by ameliorations, in whom the expectoration is abundant, not accompanied with hæmoptysis, are very speedily benefitted by the dry atmosphere of Cairo." At the very first onset, when the constitution is still strong, there is no difference between the climates of the two places sufficient to make either preferable the one to the other. (Dr. W. Reil, p. 315.)

PNEUMONIA.—The physiological effects of antimony (sickness, nausea, prostration) are quite opposed to its therapeutical action upon an inflamed lung. By giving the medicine in very small doses (1-10 or 1-12th of a grain) along with diffusible stimulants, we obtain the therapeutical action alone, and may, with the greatest advantage, use it in cases of pneumonia in tubercular and debilitated subjects. (Dr. W. T. Gairdner, p. 80.)

Ægophony without Pleuritic Effusion.—Ægophony is owing to vibration in compressed lung, and only differs from bronchophony in being produced by a different degree of compression. It may thus occur in pneumonia without any pleuritic effusion at all. (Dr. Law, p. 86.)

THROAT.—*Instrument for Examining the State of.*—In many cases it is extremely difficult, if not impossible, to get a proper view of the state of the throat and fauces, in affections of these parts. To facilitate examinations of this kind, an extremely useful little instrument is made by Mr. Matthews, of Portugal-street. It consists of a small disc of hard steel, very highly polished and attached by a moveable joint to a rod of softer and bendable metal. By this means the tongue can be held down and the light reflected upon the parts. (Mr. P. C. Price, p. 142.)

TRACHEOTOMY.—The tube introduced should be of sufficient size to admit air freely without over-distending the trachea, and it should always be double, the inner tube fitting pretty close, but not secured by any fastening to the outer, so that it may be easily removed for cleaning, or expelled by the efforts of coughing, if obstructed by mucus. (Mr. J. Spence, p. 89.)

Hook for fixing the Trachea.—Dr. Lawton, of Leighton Buzzard, recommends an instrument for fixing the trachea and guiding the knife in the operation of tracheotomy. It is essentially a "hook," with a groove on the concave surface. It may be obtained of Coxeter, of London. (p. 142.)

CANCER OF THE LIVER.—In many or most cases of cancer of the liver in females, a small subcutaneous tumour will be found in one of the breasts. It is situated just beneath the skin, and is not usually larger than a pea. This little tumour is pathognomonic of cancer of the liver, and may, if discovered in doubtful cases, materially assist diagnosis. (Dr. Harding, p. 95.)

CHRONIC DYSENTERY OF HOT CLIMATES.—Dr. Oliver, assistant-surgeon to 1st Battalion, 60th Rifles, writing from Calcutta, observes, that in the treatment of the chronic dysentery of that country, having tried with various success every remedy which experience could suggest, he finds bismuth, creasote, and copaiba, the three remedies of most value. Bismuth is most adapted to cases in which tenesmus and tormia have ceased, but there still remain colicky pains over the entire abdomen, the abdomen being impaired and abdominal pulsation marked. Creasote is most useful in cases with much irritability of stomach; and copaiba where the disease occurs in strumous patients. The subnitrate of bismuth may be advantageously combined with morphia; and the creasote given in effervescence with tartaric acid and soda, the latter being in excess. The usual dietetic remedies must, of course, be attended to, and full opiates at night form a useful auxiliary. (Dr. W. S. Oliver, p. 94.)

FISTULA-IN-ANO.—Cases of fistulo-in-ano are now treated in the Parisian hospitals by injections of strong tincture of iodine, according to the plan followed so successfully by our own surgeons. It is a good plan before injecting, to introduce a tent into the bowel to absorb the superfluous liquid and prevent irritation of the mucous membrane. (M. Bonnafont, p. 284.)

HERNIA.—After performing an operation for the radical cure of hernia, it is a mistake to make the patient wear a truss for a long period, for it not only masks the failure of the operation, but promotes the absorption of the newly-formed tissues, which are depended upon, in most methods of cure, for preventing the re-descent of the bowel. Mr. Wood has now performed his operation fourteen times, in all with success. The operation is not a severe one, as the patients are generally well and out of bed in two or three weeks. (Mr. J. Wood, p. 143.)

A new and very simple operation for the radical cure of hernia is the following:—Having invaginated the skin of the scrotum into the inguinal canal, pass a ligature by means of a curved needle along the invaginated portion, through the internal wall of the canal and out through the skin of the abdomen. The needle is then withdrawn leaving one end of the ligature protruding through the opening. By a similar proceeding pass the other end of the ligature by the same opening into the invaginated skin, through the external wall of the inguinal canal and through the skin of the abdomen, withdraw the needle, one extremity of the ligature being left free projecting through this opening. The two ends of the ligature must now be tightly tied together, bringing into conjunction the opposite sides of the inguinal canal, and embracing a portion of the superjacent skin. The ligature must be allowed to ulcerate out, and the lymph effused during the process will completely consolidate the parts. (Mr. H. Lee, p. 147.)

Mr. Furneaux Jordan, of Birmingham, has invented an ingenious modification of Wutzer's instrument, by means of which the parts are consolidated by fluid pressure. Thus the pressure is equable, whatever the position or shape of the aperture. The instrument, and its mode of application will be found fully described at p. 148.

POLYPUS OF THE RECTUM IN CHILDREN.—During the last two years Mr. Bryant has seen at least twenty cases of polypus of the rectum in children. This affection, though usually considered rare, is in reality of tolerable frequency. It is usually mistaken for piles, and most of the cases had been under treatment for them, of course without any benefit. The great and prominent symptom is hemorrhage from the bowel. Where in a young child this symptom occurs, be sure to examine the rectum. Polypi of the rectum are usually situated about one or two inches up the bowel, and vary from the size of a pea to that of a large nut. They must be removed either by the forceps or ligature. In some cases the polypus was broken off its attachment by working the finger round its pedicle. No bad result has ever followed. Mr. Bryant has never yet seen a child suffering from piles, and believes that cases so described are generally mistaken, and are in reality cases of polypus. (Mr. T. Bryant, p. 255.)

ULCER OF THE STOMACH.—Ulcer of the stomach may be diagnosed from ordinary dyspepsia by the peculiar grouping of the symptoms. The pain after meals supervenes in different cases at various periods, from a few minutes to an hour or two. The character of the pain is generally severe and lancinating, and is often relieved by position, according to the site of the ulcer. It is speedily relieved by vomiting. The countenance is peculiarly worn and suffering. Hæmatemesis must only be regarded as one of a group of symptoms, without the other members of which it loses much of its significance. The treatment which is more beneficial than all others is the adoption of a purely milk diet. The patient must be directed to take as much at each time as he can do, without bringing on the pain. Patients previously worn out and emaciated, will be found to gain strength as well as comfort, under this diet. The irritability of the stomach in these cases suggests the use of nitrate of silver and opium, of each about a third of a grain three times a day, with one-eighth of a grain of extract of belladonna, and if the bowels require it, a little castor oil and henbane or colocynth. During recovery, citrate of iron may be given with advantage. The ordinary diet must be returned to with great caution. (Dr. W. F. Wade, p. 93.)

AFFECTIONS OF THE BONES AND JOINTS, &c.

Amputation.—When amputating at a joint, do not follow the usual practice of removing the articular cartilages, for this plan is based upon the idea that these cartilages will disintegrate and prevent union, whereas this is not found to be the case in practice. (Mr. H. Walton, p. 130.)

Amputation of the Knee-Joint.—Amputation at or through the knee-joint is not resorted to by surgeons as it deserves to be. Patients in whom this operation has been performed have recovered with stumps of the most useful and serviceable kind, much better adapted to support the weight of the body, by proper appliances, than when the stump exists in the thigh itself. In performing the operation, a large anterior cutaneous flap may be made in front of the joint, right down to the insertion of the ligamentum patellæ, which is there reflected upwards, the knife being carried through the joint. A small posterior flap is then made. The patella is retained, forming a round smooth surface to rest upon. Mr. Fergusson makes two flaps of equal length and removes the patella, but none of the articular surface. (Mr. W. Fergusson, p. 120.)

Anchylosis of Joints.—In cases where, after injuring a joint or the parts adjoining it, anchylosis of the joint follows, forcible flexure is constantly resorted to at the London hospitals, and mostly with success, provided the union has been osseous. Chloroform will considerably facilitate the operation. (Mr. Brodhurst, Mr. C. Hawkins, Mr. P. Hewett, p. 131.)

Contractions and Anchylosis of the Knee.—Excluding cases arising from injury, the remainder (including by far the greater number) may, for practical purposes, be divided into those arising from rheumatic and those from scrofulous disease of the joint. In the latter, there is seldom or never bony anchylosis, for the disease occurs necessarily in persons whose systems are weak and altogether below par. On the other hand, rheumatic disease is very liable to be followed by bony anchylosis; in fact this is the ultimate termination to be expected. The treatment of the two cases is very different. In cases of scrofulous origin, infinitely more good can be effected by steady slow mechanical extension, than by any amount of sudden violence, however skilfully employed. In cases of rheumatic origin, it is necessary to forcibly break through the bony adhesions (which are rarely very strong), and afterwards to gradually extend by mechanical apparatus, as in the former case. (Mr. H. Coote, p. 125.)

Excision of the Knee-Joint.—Be very careful how you excise the knee-joint before the patient has arrived at his full growth; otherwise, even although you may preserve the limb, it will probably fail altogether to grow in proportion to the rest of the body; the result being, that the limb will be not only useless, but an actual encumbrance. (Mr. O. Pemberton, p. 114.)

In excisions of the knee, as in all other cases of excision, the most important thing is that *the whole* of the diseased parts be removed, and where this cannot be accomplished, the operation should not be performed. Periosteal abscesses in the thigh, connected with bare bone, are a positive counter-indication to the performance of the operation. Not so pus in the joint; for in practice some of these cases are found to recover. The general health of the patient ought to be carefully considered, and especially the presence or absence of the tubercular diathesis. The usual H incision, as also the semilunar incision across the joint, above or below the patella, are open to the objection of making unnecessarily large wounds. A better way is to make a straight transverse incision across the front of the joint, removing the patella. A Graham's saw may be used advantageously, and may be so slanted as to cut the end of the femur slightly convex, and of the tibia slightly concave, so as to fit into one another, which, whilst saving the length of the bones, removes their entire surface. Wire sutures may be advantageously used. It is of the greatest consequence that the back part of the knee be well supported after excision, otherwise, dislocation is apt to occur from the ends of the bones falling backwards. (Dr. E. Watson, p. 121.)

FRACTURES.—*Pasteboard Splints.*—Pasteboard, or papier machie, may be manufactured into exceedingly light, firm and inexpensive splints. When wet, the substance may be accurately moulded to the part, and when dry, will retain the form so impressed upon it. A little cotton wool should intervene between the skin and the splints, (which should be divided into two equal halves) and the whole then enveloped in a bandage. This kind of splint was extensively used by the Russian surgeons at Sebastopol. (Mr. Acton, M. Merchie, p. 132.)

UNION BY THE FIRST INTENTION.—Towels, lint, and spunges should not be applied to the raw surfaces of flaps if wished to heal by the first intention, for innumerable little filaments are left adhering to the wound, and though so minute, act very effectually as foreign bodies. This may be proved by the microscopic examination of the early discharges from a wound so handled. Moreover the flaps are generally dusted over with fine-bone dust, from the use of the saw. Another cause of failure arises from the injurious action of the water (whether warm or cold) upon the blood which lies in the wound, rendering it unfit to undergo those changes through which it should pass in the process of union. (A. M., Edinburgh, p, 138.)

AFFECTIONS OF THE URINARY ORGANS.

LITHOTOMY.—One of the chief dangers in the operation of lithotomy consists in opening the "hypoprostatic space," or cellular space between the bladder, prostate, and urethra on one side, and the rectum on the other. This space, lying just beneath the recto-vesical pouch of peritoneum is conterminous with the abdominal cavity, and infiltrated urine is liable to pass into the peritoneal sac by osmosis. In the ordinary lateral operation this space is more liable to be opened the more downward the edge of the knife is directed, and in fact it is safer to cut almost directly outwards. In the rectangular staff operation, the knife is liable to be pushed too low, so as to penetrate the substance of the prostate into the lax cellular tissue beyond, the operator thinking the knife is still in the grove of the director. Dr. Buchanan's rectangular staff operation does not seem to have obtained that amount of trial which its merits deserve. It is remarkably easy of performance, and from the safety from hemorrhage, and certainty in the extent of the incisions, followed by excellent results. The staff is rectangular in shape, and grooved on the convex surface, and when introduced, must be held in the median line, and with the angle so much depressed, that all danger of wounding the bulb is avoided. The operation presents many points of similarity to Allarton's, and is in one or two points superior. The operation has been very fully described in a previous volume of the 'Retrospect,' vol. xxxv. p. 121. (Dr. A. Buchanan, p. 150.)

Median Lithotomy.—Mr. Teale, of Leeds, now performs median lithotomy, *as a rule*, in his practice, making the lateral operation the exception. He observes, that though in children the finger of the operator is the only dilator required, yet in adults instrumental dilatation is necessary. For this purpose Mr. Teale uses a prostatic dilator (see wood-cut, p. 161) which is essentially a modification of Weiss's female uretha dilator. Through the expanded blades of this instrument, he introduces the long lithotomy forceps, and then withdraws the dilator. It is often necessary to touch gently with the knife, the tense perineal fibres, which forcibly resist dilatation. (Mr. T. P. Teale, p. 159.)

Mr. Bowman uses a prostatic dilator consisting of two blades of thin steel, when fitted together having a blunt point, but when introduced, the finger or an ovoid plug can be readily passed in between them, thus entirely avoiding the danger of pushing the bladder before the finger of the operator. The blades are hollowed on their opposed surfaces, and, being very thin, admit of flexion. These instruments, like Mr. Teale's, may be procured of Weiss. A wood-cut will be found at p. 170. (Mr. W. Bowman.)

In this operation Mr. Henry Thompson uses a director hollowed on one side, differing from Mr. Bowman's in having a single blade only. The finger is passed along the director and acts as a dilator. The instrument possesses a probe point which is run along the staff before the latter is withdrawn. (Mr. H. Thompson. p. 171.)

Mr. Teale describes a probe-pointed director essentially similar to Mr. Thompson's—either will answer the purpose. A wood-cut of this instrument will be found on p. 164.

A prostatic dilator is recommended by Mr. Armstrong Todd, on the same principle as the dilator used by him in strictures of the rectum. (See last vol.) It consists of two parallel blades, introduced before the withdrawal of the staff, in a state of close approximation, and then gradually separated by means of a screw in the handle. On the seizure of the stone by the forceps, the dilator is withdrawn. Mr. Todd has not yet used his dilator. It will probably be useful in the same cases as Mr. Teale's will be; but in young children the simpler the operation is made the better, and with the assistance of such directors as Mr. Bowman's and Mr. Henry Thompson's, no better dilator can be wished than the finger of the operator. (Mr. A. Todd, p, 172.)

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Reaction of the Blood and of the Urine.—We may look upon the reaction of the urine as a measure of the reaction of the blood. The kidneys regulate the reaction of the blood. When this latter fluid is too alkaline the urine is feebly acid or even alkaline; when the alkalinity of the blood is below par, the kidneys secrete very acid urine. By observing the reaction of the urine, we can always infer the state of the blood and use our remedies accordingly. (Dr. W. Roberts, p. 98.)

Stricture of the Urethra.—The plan pursued at the Westminster hospital is the following: The stricture having been dilated to the size of a No. 3 catheter, the patient is placed under the influence of chloroform, and the stricture-dilator is introduced. This instrument is like a catheter, but consists of two lateral halves fixed on a central stem. When the instrument is fairly in the bladder the spring or collar which connects the blades is relaxed, and a large perforated rod, about the size of No. 10—12 catheter, bevelled off at the end, is fixed on the central axis and forcibly driven down the urethra. The stricture is suddenly dilated and in most cases ruptured. The instrument is now withdrawn and the stricture kept open by the passage of large catheters. No more tendency to contract exists, than after other and slower methods of treatment. A very large number of cases have been thus treated, and in no case have any bad symptoms followed. The great advantage of the plan is that it saves much time and suffering. (Mr. W. A. Hillman, p. 179.)

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In this operation Mr. Henry Thompson uses a director hollowed on one side, differing from Mr. Bowman's in having a single blade only. The finger is passed along the director and acts as a dilator. The instrument possesses a probe point which is run along the staff before the latter is withdrawn. (Mr. H. Thompson. p. 171.)

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Corns on the Sole of the Foot.—Occasionally a small exquisitely painful corn forms on the forepart of the sole of the foot, depending upon undue pressure from slight contraction of some of the important tendons, that most frequently affected being the tendo-Achillis. This contraction of the tendon may be so slight as to need careful examination for its detection, but so long as it exists the cause of the corn remains, and it will be found that any other measure less than the division of the tendon will be only paliative. The tendon must be divided subcutaneously, and the foot restored to its normal bearings. (Mr. H. Coote, p. 187.)

Eczema of the Hands.—M. Guillot recommends the following ointment: lard 20 parts; subcarbonate of soda, oil of cade, tar, of each from 2 to 4 parts. (p. 184.)

Favus.—In all cases the scabs must first be cleaned away, either by poultices or careful washing, and then some cryptogam-destroyer used. Mr, Startin applies with great success an ointment containing sulphur and the ammonio-chloride of mercury (the latter in the proportion of ℈j to ℥j of lard). Dr. Jenner uses lotions or baths, containing sulphurous acid. Dr. Hughes Bennett endeavours to prevent vegetable growth by the exclusion of atmospheric air, applying with that object cod-liver oil, and covering the scalp with oil-silk. Dr. Neligan employs an ointment of the iodide of lead. The treatment adopted must be followed out perseveringly, and not suspended because the disease may appear well for a time. The removal of the hairs by tweezers will materially increase the chance of cure. (p. 183.)

Lupus.—The tendency, in the treatment of this disease, is to rely too much upon local and especially upon caustic applications, and to overlook the constitutional origin of the malady. An interesting case is recorded by Mr. Weedon Cooke, in a young man following the occupation of an oyster-dredger. The disease had existed seven years, and was very extensive. Though there were no absolute symptoms of scurvy, yet his strumous aspect, and the nature of his occupation, suggested the possibility of that state of blood existing. An ounce and a half of lemon-juice was ordered to be taken three times a day, with meat and porter and green vegetables, and the following lotions and ointment; bichloride of mercury, eight grains; hydrochloric acid, sixteen minims; water, eight ounces; to be applied three times a day as a wash, afterwards covering the parts with zinc ointment. Under this treatment the ulcers entirely healed in three months, and the general health was re-established. Several relapses occurred in the next three or four years, but were speedily checked by the treatment so successful at first. (Mr. W. Cooke, p. 186.)

Small-Pox.—*Prevention of Pitting.*—Why is it, that pitting is comparatively so frequent in the face as compared with other parts of the surface? Some explain this on anatomical grounds, but more probably it is accounted for by the fact that, whilst the rest of the surface is kept covered, and so not only comparatively excluded from the action of the air, but in a state of humidity, the integuments of the face remain exposed, and in a dry and heated state. In the more active and non-typhoid forms of the disease, the application of poultices over the face appear to be the surest mode of preventing the consequent disfigurement. This treatment should be commenced at the earliest period, and continued to an advanced stage of the affection, and, if necessary, applied even over the nostrils (which are generally completely obstructed). By this means three important indications are fulfilled: air is excluded, local irritation is moderated, and the parts are kept in a permanently moist state, so that the drying and hardening of the scales is prevented. (Dr. W. Stokes, p. 185.)

VENEREAL AFFECTIONS.

CHORDEE.—The best remedy in this troublesome and painful affection is camphor. It should be given in the fluid form: a teaspoonful of the spirit of camphor may be taken before going to bed, and repeated whenever the patient awakes with chordee. In mild cases, one dose for a night or two is generally enough. (Mr. J. L. Milton, p, 193.)

GONORRHŒA.—M. Ricord's abortive treatment, as laid down in his 'Traite Pratique,' consists of rest, low diet, thirty or forty leeches to the perineum, followed by copaiba and *mild* injections of nitrate of silver, and he expressly confines his recommendation of a strong solution of this salt to those cases which begin "without pain, without any sign of inflammation." (M. Ricord, Mr. J. L. Milton, p. 190.)

Gonorrhœa and Gleet.—Copaiba in the treatment of gonorrhœa is not only unnecessary and very nauseous, but is even in some cases injurious. Gonorrhœa is treated most successfully by the free exhibition of the alkaline carbonates, given with the view of neutralizing the acid in the urine, which acts as a source of irritation to the inflamed urethra. As auxiliaries, lead lotion, and elevation of the penis against the abdomen, are useful. The diet must be restricted during the inflammatory stage. After the subsidence of this, and a muco-purulent discharge being left, nothing answers so well as a chloride of zinc injection gr. ij. to the ounce, or even in some cases only one grain. In strumous and dyspeptic persons, however, and in those of dissipated habits, the inflammation is not so high, nor the urine so acid. The best treatment is then the tincture of iron, or sulphuric acid and bark, or gentian, or calumba and the chloride of zinc injection, as in the other cases. In cases of *gleet*, the diet should be good, and beer or wine used in moderate quantities. (Mr. W. Cooke, p. 196.

NON-SPECIFIC SORES.—It is a fault very generally committed, to consider the generality of ulcerations on the genital organs, as of a specific nature, whereas this is very far from being the case. The majority of these cases will be found to get well speedily under the simplest possible treatment, and are not followed by secondary symptoms. Even induration around the sore is no proof of its being syphilitic: this symptom often occurs in strumous subjects. This bears on practice thus, that we must not hurriedly pronounce on the character of a sore, and at once commence a mercurial course, and irritating applications. Even supposing it to be after all specific in its nature, "nothing is lost but a little delay." (Mr. J. Harrison, p. 194.)

AFFECTIONS OF THE EYE AND EAR.

ACUTE SCLEROTITIS AND IRITIS.—In those cases of inflammation of the deeper-seated structures of the eye, attended by agonizing, often nocturnal pain, in which the usual treatment consists in bleeding, blistering, and mercurialization, give morphia freely, ¼ gr. of the hydrochlorate every third hour, and apply warm fomentations to the eye. By this simple treatment, it will be found that in the majority of cases the disease will rapidly decline; when, by the old plan of treatment, it would have remained for some time. The action of the drug must be attributed to its power of reducing nervous irritability, which may be viewed as the primary cause of the inflammation. (Mr. J. Z. Laurence, p. 203.)

GLAUCOMA.—*Division of the Ciliary Muscle.*—A new operation for the relief of acute glaucoma is proposed, by which removal of a portion of the iris, as in Gräfe's operation, is avoided. Glaucoma depends upon an arthritic condition of the blood, and the blood-vessels sooner or later become diseased, in the same way as the vessels and valves of the heart are affected in arthritic disease. At first, though the coats of the retinal capillaries appear pretty healthy, yet small aneurismal dilatations are visible, these in a more advanced stage giving way and allowing *extravasation* of the contents, clots being visible in the vitreous humour, which itself becomes of a yellowish tinge. Effusion takes place within the eyeball, and by the resulting pressure upon the ciliary nerves and retina, intense pain, and ultimately total blindness is caused. Mr. Hancock has noticed, what has never been previously observed, viz., that the eyeball is constricted, and marked by a circular depression at the point corresponding to the ciliary muscle, whilst the vessels around this part are engorged to an extraordinary degree. The eyeball is elongated in its antero-posterior diameter, and the sclerotica bulges irregularly. To this condition are attributable the ophthalmoscopic and pathological appearances of the blood vessels observed in this disease. The operative procedure proposed is the following:—a Beer's cataract knife is introduced at the outer and lower margin of the cornea, where it joins the sclerotica. The point of the knife is pushed obliquely backwards and downwards, until the fibres of the sclerotica are divided obliquely, for rather more than one-eighth of an inch; the ciliary muscle is thus divided, whilst the accumulated fluid flows by the side of the knife. By the situation and oblique direction of the incision, a free drainage of the fluid is provided for. The iris is but slightly wounded, and the pupil is preserved of its original size and shape, and in its normal situation. (Mr. H. Hancock, p. 197.)

POLYPUS OF THE EAR.—Various modes of treating polypus of the ear are in use. Soft ones are removed in the same way as polypi of the nose, or their neck is ligatured by an instrument contrived for that purpose; these methods, however, are of doubtful value, on account of the narrowness and obliquity of the canal. Often the polypus grows from the membrana tympani, and then, without great care this structure, with the tympanic chain of bones will be destroyed. Mr. Bowman lately commenced the treatment of a case by slow torsion. The tumour which extended from the meatus, was transfixed by a needle and twisted. The next day another twist was given. It was hoped by this means to cause self-strangulation of the tumour, and thus to ensure its complete removal. The result of the case is not given. (Mr. W. Bowman, p. 206.)

MIDWIFERY, AND THE DISEASES OF WOMEN.

CARCINOMA UTERI.—The two best deodorising agents in carcinoma uteri are Condy's fluid, and tar-water, both of course used as injections. Of Condy's fluid, an ounce should be added to a pint of water, and a cupful used as injection thrice daily. For preparing tar-water, the following is the best formula:—Tar, lb. j.; water, conj. j.; mix, stirring with a stick for a quarter of an hour. As soon as the tar has subsided, strain the liquor, and keep it in well-closed jars. Higginson's syringe is invaluable in these cases. A tepid solution of soap and water should be injected first, and immediately afterwards, a breakfast-cupful or more of undiluted tar-water. The syringe should have a long vaginal nozzle, that the injection may come in contact with the ulcerated surface. (Dr. T. Skinner, p. 244.)

EXCORIATED NIPPLES.—The use of the following preparation is invariably followed by good results. ℞. Acid tannic. gr. xx., glycerinæ, alcohol. aa, ℥j. M. (Dr. Pierce, p. 249.)

FŒTAL AUSCULTATION.—The pregnant uterus, like other hollow viscera, has a regular peristaltic motion, or periodic contractions. This can be at will induced by slight manipulation of the abdomen. The uterus will soon be felt hard and resisting, and it is whilst in this state that the fœtal heart can be most distinctly heard. The stethescope must be placed firmly on the womb itself, and perpendicular to its surface, and in the horizontal line level with the anterior superior spine of the ilium. The left side should first be tried, then the right. (Dr. Druitt, p. 215.)

GALACTAGOGUE.—*Castor Oil Leaves.*—Where there is a deficiency of milk in lying-in women, give an infusion of castor oil leaves, with some highly nutritious soup, as conger-eel. The effect will be to determine a copious flow of milk. In Boa Vista the leaves are applied to the breasts of any woman, who has previously borne children, at the catamenial period, with the effect of producing a secretion of milk, which can be perpetuated by the simple irritation produced at the nipple by the suction of a child. Thus a wet-nurse may be procured at pleasure. An extract of the leaves given to unmarried women produces intense pain in the breasts, possibly the suction of a child might even in them induce the lacteal secretion. The leaves, and a tincture and extract prepared from them, may be procured of Mr. Greenish, New Street, Dorset Square, London. (Dr. Routh, p. 288.)

FUNGOUS CONDITION OF UTERINE MUCOUS MEMBRANE.—*Tannin Pencils.*—In many lesions affecting the cavities of the neck and body of the uterus, particularly in the fungous conditions of their mucous membranes, with consecutive hemorrhages, tannin pencils may be advantageously substituted for intra-uterine injections. The formula for their preparation will be found at p. 246. These pencils are passed into the uterine cavity, and there left to dissolve; this is repeated every three or four days. "The fungous state of the mucous membrane progressively disappears, and the hemorrhages are arrested." (Dr. Becquerel, p. 246.)

OVARIAN DROPSY.—*Injection of Iodine.*—The cases in which an ovarian cyst must not be injected with iodine, are, when the evacuated contents are mixed up with inflammatory products (particularly if the inflammation is at all acute), and also where a multitude of gelatinous and semi-solid masses exist in the contents, rendering their complete evacuation impossible; for these masses are coagulated by the iodine, and then act as foreign bodies. With these exceptions, the injections of iodine into an ovarian cyst may be performed with safety to the patient, and in the case of unilocular cysts, already tapped once or oftener, and still free from inflammatory action, with the most hopeful prospects of a full and final cure. But where the cyst is multilocular, that cyst only will be cured which is injected. A succession of cysts may be treated by repeated injections. On having recourse to this operation, the operator must take with him—1st, An ordinary trocar and canula, with which to puncture and evacuate the cyst; 2nd, A long glass tube, with a nozzle, capable of being adapted to, 3rd, A gum-elastic tube catheter, with the opening at the end of the tube; 4th, A uterine sound; 5th, Ten or a dozen ounces of tincture of iodine, or of the liquor iodinei compositus: 6th, A piece of lint, and some long strips of sticking-plaster. (At p. 231 will be found a wood-cut of the glass-tube used by Prof. Simpson). After introducing the iodine, and freely manipulating the cyst, to insure the more effectual application of the irritant to the whole surface of the lining membrane of the cyst, *the fluid must be allowed to run out.* It must not be left there as a lasting irritant. The fluid injected may be the ordinary tincture of iodine of the Edinburgh Pharmacopœia (which contains half a drachm of iodine to every ounce of rectified spirit), or the liquor iodinii co., which, though much weaker, does not contain any spirit, whereby all chance of the intoxicating effect of the alcohol on the system is avoided. For small cysts four to eight ounces of fluid may suffice for injection, but for the larger ones eight to twelve ounces may be required to bring the irritant in contact with the whole of the lining membrane. (Dr. J. Y. Sympson, p. 227.)

OVARIOTOMY.—*When is it to be had recourse to?* It must never be dreamt of where there is organic disease of any other organ, or the constitution of the patient is in any way unhealthy; secondly, when there is strong reason to suspect, as there often is, carcinomatous deposits in the tumour; thirdo a it must not be performed in cases in which there is only a single cyst, lyr, single large one and the rest very small; until the trial of milder and safer measures has failed. (Dr. J. Y. Simpson, p. 227.)

In cases in which the stump of the ovarian tumour is unusually short, the use of the clamp is liable to be attended with a certain degree of dragging, and in all cases there are certain dangers and drawbacks which render it desirable to discover some simple means to arrest the flow of blood from the coutends of the divided vessels. Dr. Simpson believes that by the employment of *acupressure* the desired ends will be attained more safely and effectually than by any of the methods previously employed. By means of one or two acupressure needles he proposes to pin the divided pedicle to the abdominal parietes, so as to retain it effectually in position, and at the same time compress the bleeding vessels. The wound must be closed in the rest of its extent with iron-wire sutures, avoiding the peritoneum. (Mr. Spencer Wells advises this also to be included, as there is then less chance of purulent discharges finding their way into the peritoneal cavity.) (Dr. J. Y. Simpson, p. 238.)

PHANTOM TUMOURS.—A case forcibly illustrating the value of chloroform as a means of diagnosing accurately the nature of tumours, suspected to be what are denominated "phantom," is related at p. 317. The use of chloroform, in similar cases, was advocated lately by Dr. Simpson, in his excellent lectures on diseases of women, under the head of 'Simulated or False Pregnancy.' (Dr. Ramskill, Dr. Jones, p. 242.)

PREMATURE LABOUR.—One of the easiest and safest modes of inducing premature labour is that known as Cohen's method. The woman must be placed upon her back with the nates projecting somewhat over the edge of the bed, and the feet supported by two chairs. Then introduce an ordinary elastic catheter through the mouth of the uterus, keeping it close to the anterior wall, so as to pass between the uterine parietes and the membranes. This being accomplished, inject about seven ounces of water heated to 90° or 100° Fahr. After a moment or two, withdraw the tube. A considerable amount of the water will drain away, but the membranes will be effectually separated from a considerable portion of the uterus, and labour will usually follow within the twenty-four hours. (Dr. E. Noeggerath, p. 207.)

PROLAPSUS UTERI.—*Zwank's Pessary.*—One of the most harmless and efficient forms of pessary is the one originally invented by Dr. Zwank, and as modified by Dr. Eulenburg, of Coblenz, may be thus described:—It consists of two lateral halves, each half consisting of a broad, expanded, and hollowed wing, and a descending ramus or handle, at right angles to the wing. When it is introduced into the vagina the wings are closed and the rami expanded; but when fully introduced by means of an india-rubber spring the wings expand and receive the prolapsed womb, whilst the rami close, and by their form assist in retaining the instrument in position. It must be removed at night and washed, and well oiled before introduction in the morning. About four different sizes will be required, the measurements for which, with wood-cuts, will be found at p. 219. (Dr. E. Noeggerath, p. 216.)

VESICO-VAGINAL FISTULA.—Dr. I. Baker Brown, in performing the operation for the cure of vesico-vaginal fistula uses metallic sutures with a separate bar-clamp to each suture. These clamps will be found figured and described at p. 223. By the use of these clamps the duration of the operation is much shortened—a quarter of an hour will suffice in slight cases. There is also no uncertainty whether the whole extent of the edges of the wound are in perfect apposition or not; and moreover, however irregular the opening, its tortuosity can be followed, without the slightest difficulty. He uses three knives specially made for this operation. (Dr. I. B. Brown, p. 222.)

Dr. Batteo, of Georgia, U. S., proposes what he denominates "a new principle of treatment and apparatus" in the performance of the operation for vesico-vaginal fistula. This new principle consists in direct pressure upon the approximated edges of the fistula; and the apparatus by which this is attained combines the offices of both a splint or support to the wound, and a compress upon its edges. A strip of flexible sheet lead, five-sixteenths of an inch in width is perforated with small holes on one edge, and on the opposite is notched with a very fine saw. The length of the strip is of course accommodated to the size of the fistulous opening. The edges of the wound having been pared, this splint is applied much in the same way as is necessary in Bozeman's, Baker Brown's and Simpson's plans of proceeding, slight modifications of course being necessary from the altered form of apparatus.

This mode of operating admits of more direct pressure upon the approximated edges of the wound, but otherwise it is essentially the same as the plans previously recommended and acted upon with so much success. Woodcuts of the apparatus, and a detailed account of the mode of using it, will be found at p. 225. (Dr. R. Battey.)

MISCELLANEA.

Belladonna, *Poisoning by.*—A series of cases related by Mr. Seaton, of Leeds, remarkably bear out the conclusion previously arrived at, as to the mutual counteracting powers of opium and belladonna. At first vomiting was induced by an emetic, and then small doses of laudanum given until sleep was procured. This treatment was followed by the happiest results, only one case proving fatal, and that was in a subject already diseased from scrofula. "In none of the cases in which delirium was present, were the symptoms alleviated until sleep was obtained; and, after sleep, the patients felt comparatively well. The pupils were widely dilated so long as the delirium continued, and, when sleep was obtained, were either contracted or reduced to the natural size." (Mr. J. Seaton, p. 308.)

Staple Suture.—Mr. Startin recommends the use of a new form of suture, which he calls the "staple suture." It is especially adapted for closing neatly wounds in the skin, also in fissures of the soft palate and hare-lip. It will be better understood from the diagram at p. 283, than by any description.

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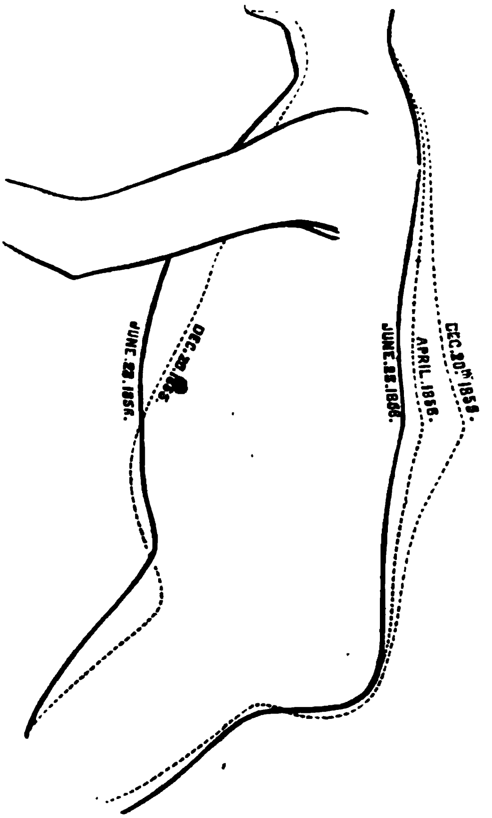

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